On Space-Time and Ontology: Extract from a Letter to Adolf Grünbaum

I pass now from small-scale editorial points, and come to a more difficult task: the attempt to formulate in a clear way the issues that divide us, as I see them. We may roughly classify these under three heads: exegetical questions; substantive questions of the philosophy of science; and questions *about* (allegedly) substantive questions—about, in particular, the importance, or even the meaningfulness, of certain questions put forward as substantive. I do not suggest that a precise or a very satisfactory division of issues along these lines is really possible, but only that it is worth having these approximate headings in mind in our discussion.

I shall say relatively little about exegetical disagreements between us, with one important exception (important, at least, to me). We have, I think, some such disagreements—but not very sharp ones—about Newton; and we have plainly more serious ones about Riemann; but the one set of divergences over the interpretation of a writer that I cannot avoid dealing with in some detail has to do—(it seems so odd, I am actually experiencing some difficulty in setting down the words!)—with the interpretation of *me*. We shall come to this in a short while.

As to questions of a substantive kind (that is to say, the questions that I am myself inclined to regard as the ones that ultimately *matter*): I have been in one way very pleased, but also in one way very puzzled, to find in reading your paper that despite what appear to be serious disagreements between us, on what I consider the central substantive points we are evidently quite close together. At least, I have found nothing in your paper that contradicts, and several passages that seem to affirm, what you

quote from me on p. 331 ("I think I am only paraphrasing Newton—with whom I agree—when I say [etc.]"). For me, this is the main general point. And as to the interpretation and evaluation of relativity theory in particular, you hold (if I read you correctly) that the status of the tensor-field g as a legitimate physical field is independent of ontological theses about "absoluteness" versus "relationality" of metrical (or any other) attributes of space-time; the same, I presume, you would maintain of the laws governing this field and its relations to any other physical structures; you grant the admissibility of vacuum solutions in general relativity, and of vacuum regions in nonvacuum solutions; and you seem to concede the conceptual coherence of Wheeler's (old) program—which I had previously understood you to challenge—since you cite it (or, as you say, "an appropriate physical theory like GMD") as a theory that *might be empirically established*, when you characterize what you call "OI" as "open to broadly empirical refutation" (p. 359). With all this I agree; and I consider it as more or less exhausting the issues that are worth serious controversy in this domain—which suggests to me that the locus of our disagreement lies in my third class: i.e., that we differ seriously about what to consider a serious difference.

The question that seems to dominate your discussion (if we restrict its formulation to the context of the general theory of relativity) is this: *by virtue of what is the tensor-field* g *to be regarded as representing the metrical structure of space-time?* Now that seems to me a legitimate question, but not a clear and precise one: it seems to me (to use a distinction I appear to be growing fond of) "presystematic" rather than "systematic," and to pose a problem of explication. I should be inclined to reformulate the question as: "Just what do we *mean* when we say that this tensor-field describes *the metrical structure* of space-time?" You, however, appear not to see such need for explication, but to regard the question in its first version as clear, and of systematic "ontological" import. (For me, the word "ontological" itself presents seriously problematic aspects. You clearly do not use it in the sense advocated by Quine; and if you think Quine's usage not a very useful one for the philosophy of physics, I quite agree with you—but this surely suggests that there is not a generally accepted systematic use of the term.) You find that hard problems arise within this ontological domain: "[I]t is presumably very much *harder* to *discover* whether the metricality of space-time is ontologically absolute or relational than to discover whether a butcher is mistaken if he . . . re-

gards the weight of a chicken . . . as . . . intrinsic to the chicken" (pp. 337–8). Even in the case of the butcher, I think some preliminary verbal clarification is needed, if only to determine whether he might distinguish in any way between what is measured by a spring balance and by a beam balance; but this is minor, of interest chiefly for the moral that verbal usage ought never to be taken quite for granted, except under the most closely controlled conditions. As to your *harder* problem, I think it may in one way be a quite *impossible* one, because ill-defined—but therefore no problem at all; and in another way (namely if, through adequate explication, it becomes well-defined), perhaps in principle not so hard. For consider: when the butcher's usage (and the intended sense of "intrinsic") has been fixed, the answer to the butcher question is logically determined by the body of knowledge codified in the theory of gravity (Newton's will serve the purpose). I ask, if the use of the phrases 'metricality of space-time' and 'ontologically absolute or relational' is made entirely clear, should we not expect that the answer to your question will also be determined by the content of the relevant physical theories—say, the general theory of relativity? Of course, the discussion of the theory—criteria for its application to phenomena, derivation of consequences, evaluation of evidence, etc.—may involve hard problems; but assuming these under sufficient control, it would seem prima facie that your ontological question, *if* it is clear, ought to be answered with comparative ease (unless, that is, contrary to what seems to me at all likely, the answer should turn out to depend upon further difficult *purely mathematical problems*; in which case these should at least be precisely posed). On the other hand, if the ontological question is not clear, there is no point in speaking of it as hard.

This complaint, lack of clarity, is just the one I have previously made—see, e.g., your quotation from me on pp. 328–9, and the end of my letter to you of February 16, 1975—against your attempt to explicate "intrinsic" in "Space, Time, and Falsifiability"; and in my letter, I excused myself from extended discussion of the matter, in part because, as I wrote, "I think it possible that you may have come to similar conclusions yourself." You have not confirmed or rejected this conjecture; but since you do not appear to rely, in your present discussion, on that earlier attempt, I shall still refrain from detailed comment on it. However, elaboration of my similar complaint against your latest discussion now seems obligatory. I have already indicated the general point, which can be put as follows:

Although I believe that in your quotation from *Faust* (p. 348) it is the student's sentiment you commend ("*Doch ein Begriff muss bei dem Worte sein*"), not that of Mephistopheles ("*eben wo Begriffe fehlen, da stellt ein Wort zur rechten Zeit sich ein*"), your practice in connection with certain crucial words—notably, 'ontological' or 'ontologically constitutive,' and 'metrical' or 'geometric'—seems to me rather of the devilish kind. I want to be unmistakably clear about this: I am not objecting to the fact that your use of these terms is different from mine—in such matters I am an extreme libertarian, believing not only that the utterer is entitled to any usage he finds apt, but also that the auditor has a certain obligation to be open and attentive, and to try to construe what is said in the way it is intended. (Just for this reason, I think it bad practice, except in connection with technical terms of quite established scientific usage, to assume that a word can be employed in systematic discussions without any need for elucidation.) What I object to is that, as it seems to me, you use the terms in question in a way that *is not clear at all*; or—to state a more modest claim, but with greater assurance—it not only "seems to me" but is indisputably true that you use those terms in a way that *is not clear to me*.

Perhaps it will be a help, in trying to give a more detailed account of what puzzles me about your point of view, if I first briefly sketch my own answer to the question raised a few pages back ("By virtue of what is the tensor-field *g* to be regarded as representing the metrical structure of space-time?" or: "Just what do we mean when we say that *g* describes the metrical structure of space-time?"). I do not think my answer differs very much, in substance, from yours: it has a good deal to do with the theory of the behavior of such things as measuring rods and clocks. But I would begin by making a preliminary, and in my opinion quite crucial, remark. This is that there is no "categorical" (in Kant's sense: derived from "categories" or "reine Verstandesbegriffe")—no innate, a priori, or (in your language) "canonical"—notion of *the metric of space-time*. Indeed, nobody before Minkowski employed such a notion at all. And when Minkowski invented, or discovered, this concept, what exactly did he do? He showed that the special-relativistic theory of space and time was tantamount to the statement that space-time has a particular structure, whose attributes are suggestively (although not perfectly) analogous to those of a Euclidean metric structure (thus, by the usual and useful liberty one takes in mathematics to *extend* or "generalize" a notion, a structure

that may reasonably be called a "metrical" one), *and from which* (essentially by "orthogonal projection") *the Einstein geometric, chronometric, and kinematic relationships are determined.* So we have two main points: (1) a structure whose characteristics are, in a certain generalized sense, of that mathematical species which is called "metrical"; (2) a theory according to which the physical facts that belong, classically, to physical geometry (and chronometry and kinematics) are manifestations of that structure. Geometry having thus been "aufgehoben" into the Minkowski structure, one quite naturally refers to the latter as "the geometrical structure of space-time."

I shall not be so pedantic as to recite to you the story of the transition from special to general relativity, but only remark (a) that this transition reinforces the geometric analogy by the expansion from Euclid-Minkowski to Riemann-Minkowski, and (b) that it is with Minkowski, and the classification of world-directions into spacelike, timelike, and null, that *light rays* (or photons) and *free inertial particles* make *their first appearance as* "probes" or "test-bodies" of the metric.

Note, then, from this, that I fully agree with you (i) that the relationships of the metric tensor to various kinds of physical structures or systems that serve to "measure" it are of capital importance for the physical significance of that tensor, and (ii) that it is in virtue of certain of these relationships that it was natural for the creators of the theory to refer to this quasi-Riemannian metric structure as "the" metric, or "the" geometry, of space-time.[1] But note also (iii) that, as I see the situation and have just sketched it, the question to which (ii) is the answer is essentially one of *etymology*—hence essentially *historical*—rather than one either of physics or ontology. Why you (apparently) consider it both of the latter[2] is something I do not understand. I have tried, just above, to indicate how I use the word "geometric"—namely, rather flexibly: now in a broadly mathematical sense referring to purely structural characteristics of a certain (but not very sharply defined) type; now in a narrower sense having some reference to the historical root meaning, connected with "lengths (etc.) of objects." But when you speak (p. 345) of the question whether the structure to which measuring instruments adjust themselves is "an *autonomously geometric* structure," I simply and honestly do not know what the words mean. And as to "ontologically constitutive," I should have thought (whatever the finer analysis of the notions involved) that the application of that expression would be to situations in which some kind of

"reduction" of one sort of entity to another was involved. Thus I have always taken Leibniz to be saying, not that spatial attributes *qualify as*—are "ontologically constituted as"—*spatial* by their relationships to *bodies,* but rather that those structures we call spatial *fundamentally consist in*—have no other meaning or being than as—*relations among bodies. This* is the doctrine that has made Leibniz's view fascinating to me; the other I should consider trivial. I might be wrong to consider it so—but only if the phrase "ontologically constituted as spatial" is being used with a meaning that I have so far utterly failed to grasp. And therefore—at least for me (*argumentum,* not *ad hominem,* but *ab homine!*)—some explication is required.

I must also say that I shrewdly suspect, from various signs, that such an explication is not to be had. I shall try to present a representative selection of these signs. But first, again, I emphasize that there is no question here of demonstrative refutations: at several points—and clearly important ones—your usage or your criteria *seem incoherent* to me; that they *are* incoherent I cannot prove, and I cannot be sure, because I do not understand them.

(1) Let me mention first your "family of concordant external metric standards," or "FCS," which in the view of your relationalist "ontologically mediates" the metrical status of the tensor-field g. Why do you include, besides the classical devices for measuring spatial and temporal quantities (measuring rods and clocks), also *photons* and *free massive particles* among these "metric standards" (p. 345)? As I have already remarked, before Minkowski these would not have been considered to be "geometrical" measuring devices.[3] Why are they now so regarded? From my point of view, the answer is plain: *because they can be used to explore the field* g, *which we now call* "the space-time metric." I—with my relaxed attitude towards the use of the term "metric"—find this entirely reasonable. But on the same grounds, I should be glad to consider as a geometric measuring device *any* instrument that could be made serviceable for exploring the field g. It is, as far as I can see, quite arbitrary to draw the line as you do, and to say in effect that those probes of the field g that are standard in the present stage of physical theory should be regarded—"canonically"—as "ontologically constitutive" of space-time geometry. [In your revised discussion, to be sure, you characterize the "set of external devices that are taken to be canonical for the metric structure of space-time" as "open-ended" (pp. 344–5). But this reinforces for me the

impression of a fluctuating—perhaps ambivalent—point of view. For "open-ended" seems hard to reconcile with "canonical": one would at least want to know how ordination is achieved! "Open-ended" and "taken to be" suggest conceptions of a dialectical and methodological order; "canonical" and "ontologically constitutive" suggest fundamental and unchanging objective principles.]

(2) Next: I wondered, on first reading your discussion of the "relationalist"'s view of vacuum solutions [(a discussion which has now been completely revised)], what you would say about vacuum regions in non-vacuum worlds. Then I found (pp. 349–50) that (the issue having already been raised by Allen Janis) you are prepared to deny the "existence of a metric" in such regions. This, to me, is astonishing. Let me quote from my own letter to you of February 16, 1975:

> The distinction between *constitutive* elements and "probative" ones ("test-bodies") seems to me a crucial one in these questions. Distance may be measured by measuring rods; it does not *consist in* (is not constituted of—or by—) measuring rods. (Descartes thought that space is constituted by extended substance—body—and so, quite consistently, argued that there can be no vacuum: i.e., that if there are no bodies between A and B then A and B must touch. But not even Bridgman would argue that if there are no measuring rods between the earth and the sun, the earth and the sun must touch.) In general, it's characteristic of probative elements (*a*) that alternative methods of probing are generally possible . . . , and (*b*) that the "field" or structure in question is presumed to exist independently of its being explored or manifested or probed.

But you seem willing to adopt something very like the view that I said "not even Bridgman" would. Consider your FCS closely for a moment: "Infinitesimal rods" in any strict sense clearly do not exist; so it is hard to see how they can be "ontologically constitutive" of anything, ever. "Free massive particles," taken in the strict sense, almost certainly also do not exist; in any case, they must be of extremely rare occurrence. "Atomic clocks": there can hardly be any such in the interatomic intervals. That leaves photons. Can one save the constitution of the metric by maintaining that photons are (quasi-)ubiquitous? Hardly—the photoelectric effect and the finiteness of total energy in a compact region pretty well exclude such a conception. Furthermore, it is hard to see how, if presence of members of the FCS is regarded as necessary for existence of a metric, one could justify considering the *mere* presence of *any* member of that family as *sufficient*—that is, without further conditions guaranteeing that

a definite metric will be "determined"; and for this, light by itself is well known to be inadequate. It would seem to follow that the real world, viewed on a fine scale, *is rife* with "nonmetrical" regions—indeed, that an ordinary line connecting two ordinary points must *practically always* pass through such regions. And this leaves you almost in the position Descartes foresaw if a vacuum were admitted (namely, one of spatial "collapse"): you are not forced to conclude that two points *touch*, i.e., that their distance is *zero*; but you seem forced to conclude that distance, or the length of a line, hardly ever "exists" at all. It appears to me that a *reductio ad absurdum* lies dangerously near—or at least that the dialectics that may be necessary to avoid such a *reductio* are likely to cost more than the theory they are called upon to rescue is worth.

(3) You consider (pp. 358–9) "Wheeler's pre-1972 GMD program" to be incompatible with the "relationalist" view, or the thesis "OI." This, in the light of your preceding discussion, is quite baffling to me. If Wheeler's program were to succeed, it would quite certainly have to embrace all the physical structures, agencies, or instrumentalities, that make up what you call the "FCS." It is true that, because Wheeler's program was for a unified field theory, these things would all have to be characterized as states of the field g, and hence (I suppose), in your terms, as not "external" to it; nevertheless, when I try to adopt your way of speaking of and looking at these matters, it seems that they could perfectly well still be thought of as independent of—and in that sense "external to"—the "metrical status" of g. You have already conceded that fields, including g, can be regarded as "physical," and as "existing" (hence "ontologically constituted"), independently of any probing devices (FCS). If the field *does* "exist autonomously," and if the devices *can* be constituted by special states of the field, why should the role of such devices as "constitutive of the *metric* character of the field" have been undermined thereby? I admit that I do not understand, from the beginning, what you mean by this "constitutiveness"; still, it is a further perplexity for me that, having granted that the field g can "exist" independently of the FCS, but having maintained that it is "constituted as metric" by its relation to the FCS, you should then say that if the members of the FCS can be "reduced" to g, the power of the FCS (which, after all, still means, not just g, but some quite special configurations of g) to constitute g "as metric" has been lost. (Indeed, as I write this, my head swims.)

[(4) In your discussion of Winnie's new considerations (pp. 353–5) I

believe I see the ghost of the same (as it seems to me) confusion that I remarked on in your earlier discussion of Winnie on Robb. In order to state the case clearly—and also because some of the substantive points I have made to you seem to me worth putting on record—I am going to allow my section on that earlier discussion of yours to stand, even though you have now deleted it from your paper.

[You said, then,] following Winnie, that "Robb's work shows that the system . . . of causal relations . . . suffices . . . to generate, via purely nominal definitions, the Lorentz metric . . . *without* the antecedent imposition of the requirement of flatness." First, there is a small technical point that ought to be clarified: it must be clearly understood that an infinite family of Riemann-Minkowski metrics is compatible with—or "exists over," in a mathematical sense—conformally flat space-time, or "Robb space." *One and only one member of this family is metrically flat* (understanding that we identify Riemann structures whose metrics are related by a constant factor). There are, thus, fully ("canonically") determined by the Robb structure (conformality; "system of causal relations"), *both* (a) an infinite class of Riemann-Minkowski structures conformally equivalent to the Robb structure, *and* (b) a unique member of this class, which is metrically flat. (I am not sure your phrase "without the antecedent imposition of the requirement of flatness" quite does justice to this situation; although, indeed, it can be shown that in a certain sense the metrically flat member of the class mentioned in (a) is the only member of that class that can be distinguished from all others by *any* criterion that utilizes nothing but the assumed data.)

Given all this, and in the light of your general discussion, I should have expected you to ask, as on p. 342, "on the strength of just what" the structure singled out by Robb's "purely nominal definitions" has "the ontological status of being *the metric field* in the sense of endowing space-time with the latter's particular metric structure." But instead, you accept the structure in question in this case without any "ontological mediation of the FCS." The reason you give is that this metric is definable in terms of ("arises from") a certain physical foundation—the Robb structure—which also allows one to define the (topological and differentiable) manifold structure of space-time; so: "It is philosophically gratuitous, or at best highly contrived, to single out the *metric* structure of this space-time as being relational in a way in which the underlying R^4-manifold structure of space-time is not. . . . And thus the metric structure is *not* ontologi-

cally dependent on relations to any *further* class of external entities or standards."

Now, first of all, a formally analogous statement can be made in the case of GTR. You have accepted the manifold structure as *somehow* "given," and as physical; and you accept the field g as physical, independently of the "FCS." This ensemble constitutes a structure that enables one to define all such notions as belong to the geometry of a four-dimensional Riemann-Minkowski manifold: the topological, differentiable, and metric notions all "arise from" this structure. Of course, you may argue that the Robb structure is "deeper": that Robb derives topology, for instance, from something physically more basic, whereas here it is cheating to say that the topology "arises from" our structure, since it has really been built into that structure explicitly and deliberately. This contention does certainly have merit; yet I feel uneasy about basing upon it a sharp ontological distinction between the two cases, since I do not see a sharp criterion for that distinction: it remains true that in both cases we have a body of structural data, of acknowledged physical and ontological legitimacy, sufficient to allow the definition of "a geometrical structure," and it is (again: to me!) obscure why in the one case but not the other something "ontologically more" is required to qualify this as "*the* geometrical structure."

But that does not yet hit the main point. The main point, from my perspective, is that whereas you and I agree, in the general case, that not the mere formal attributes of a single physical field, but also the ways that field affects particular "standard" physical configurations or "instruments," are involved in our choosing to characterize the field as describing the geometry of space-time, in this special case we unexpectedly part company on that issue: I do not think, as you do, that Robb's results have *any* bearing on the question why, in special relativity, the Minkowski structure (or "Lorentz metric") "deserves" to be called "the space-time metric." Indeed, any one of the infinite class of Riemann-Minkowski metrics compatible with the Robb structure might, without violating physical theory, be the field to which the members of your "FCS" "adjust themselves"; and if this should be one of the nonflat metrics, that nonflat metric would be the one to manifest itself in all spatio-temporal measurements, and would *undoubtedly* play the dominant role in the theory of the world. In this case, although the Lorentz metric would be "simpler" than the Einstein metric in the sense of requiring a smaller body of data

for its determination (namely, it is determined by the Robb structure, whereas the Einstein metric would not be), yet the Lorentz metric and its mathematical distinction would be a mere curiosity; the Einstein metric would be preferred, just as it is preferred (in those contexts in which it makes a material difference) in the real world: because it dominates physical processes in general, and spatio-temporal measurement in particular.

How has it happened that we seem to have reversed sides here? My answer is that I maintain the same attitude to your "FCS"[4]—and to all other possible instrumentalities for probing a field, in all cases: they are essential to our knowledge of the field; physical laws of interaction adequate to determine how the field affects their behavior form an essential part of our concept of the field (without *some* such laws, we should have no adequate concept of a physical field at all); and special characteristics of that interaction influence us in giving "names," or brief descriptions, to fields—what we call "the electric field" is what exerts forces on what we call "charges" (and so it would still be, for instance, if the theory of Gustav Mie had been successful, according to which charges are "made out of" the field itself—a situation instructively analogous to that of Wheeler's GMD); what we call "the metric field" is what governs the "adjustment" of what we call "measuring rods (et al.)." But in no case do I consider the instruments, test-particles, etc., as "constitutive" of the field. You ascribe the latter virtue to the FCS (which implies, to me, a different use of the word "constitutive" from my own); but you withdraw the ascription in two cases—Wheeler and Robb—which differ very much from one another, but which have this in common: that in each of them there occurs, although in entirely different ways, an "ontological reduction" (or "constitution") in the sense in which *I* would use those words. For in the Robb case, it is true that the Lorentz metric can be "reduced" to the "causal structure"; and in the Wheeler case, the measuring instruments—the "FCS"—are reduced to the field. It seems fairly clear, then, that you boggle at saying either that the space-time geometry is "constituted" *by two different things* (Robb structure and FCS) or that it is "constituted" *by what it itself constitutes* (FCS in the Wheeler case). This would be quite reasonable if "constitutes" were being used in a single sense; but I submit that it is not—that the sense in which your "relationalist" ascribes "constitutiveness of the metric" to the FCS is *plainly* different from the sense in which, for Robb, the causal structure "constitutes" the Lorentz metric, or, for Wheeler, the metric field "constitutes"

bodies—and thus that *your withdrawal of the relationalist ascription in these two cases exhibits a terminological confusion.*

[I take it now, from your revised paper, that you have accepted the foregoing comments *in part*—in particular, that you agree with what I have said about the bearing of Robb's theorem, but not with what I have said about the bearing of Wheeler's program.

[When we turn to the new points made by Winnie, it is to be noted first that the theorem cited by you (p. 353) is analogous to Robb's theorem: conformal structure together with a suitable "emptiness" assumption determines a metric structure. So one is led to ask, if this spirit was earlier exorcised successfully, whence does it now derive renewed power?

[The answer, evidently, is that "empty" is now a more powerful magic than before. Then we were in the context of "the pre-GTR version of the special theory of relativity," where in your opinion (p. 350) counterfactual appeal to hypothetical standards or test-bodies may be countenanced; now we are in the context of the GTR, where, you believe, appeal to such entities must be excluded, since their presence would modify the metric; and you refer (p. 351) to the *nonlinearity* of the GTR as especially pertinent here. Yet it is a standard point that this situation—modification of the field by test-bodies—exists in the classical linear fields as well. The crucial issue is not *linearity*, but *approximation*: can one suppose that "small" test-bodies "do not change things much"? If one could *not* make such an assumption in the GTR, then it is obvious that we could not apply the theory at all: for we do not know the true and exact impulse-energy distribution of the universe: in practice we ignore, not only mountains and planets, but—in cosmology—anything much smaller than a galaxy; yet you would have us reckon in, explicitly, each atomic clock.

[If, however, we envisage "weakly empty space-times" as *global approximations*—as universes in which "ordinary bodies" (and photons, etc.) are "gravitationally negligible" in comparison with gravitational radiation—then it would still be possible for you to appeal to the "FCS" as "constitutive" of the metric in such space-times. I remind myself that 'FCS' stands for "family of *concordant* . . . standards"; and conclude that *if the "standard" provided by gravitational waves did not agree with the behavior of particles, clocks, etc., it would not be allowed in as "concordant."* Is it not, then, a strange doctrine that admits gravitational waves (provided there are enough of them) as ultimate canonical ontological constitutors of "the metric" so long as space-time is weakly empty, but

that deprives them of ultimate "constitutive" power if somewhere there is a neutron?[5] Let us, for instance, suppose that there is available a set of objects of (gravitationally) negligible mass belonging to the "FCS"; and that the behavior of these objects discloses a metric structure of space-time conformally, but not metrically, equivalent to the structure of a "weakly empty" world—one, moreover, in which gravitational radiation is abundant. We should undoubtedly conclude that we had thereby detected the presence of (gravitationally nonnegligible) nongravitational energy, and that the true gravitational field and associated Einstein metric are not here determined by the conformal structure. This case is quite parallel to the Robb one; and I assume that you (unlike me) would regard the objects in question as *ontologically constitutive* of "the physical metric." Now suppose that, holding everything else the same, we allow the objects of the "FCS" to vanish. Gravitational radiation will remain—sufficiently abundant, as we are supposing for the argument's sake, to determine the conformal structure. Does it now "constitute" a "physical metric," different from the one we had before (namely, a "weakly empty" one)? Note that the gravitational tensor g—to which you allow physical significance that is independent of the "FCS," and which continues as it was before—is *not* that of a "weakly empty" space-time, although its gravitational waves determine a conformal structure (that of g itself) to which Winnie's theorem applies, and thus determine a unique associated "weakly empty" metric tensor g^*. In the absence of the "FCS," does g^* acquire "metric status" that it did not have in their presence—or can gravitational radiation "constitute" a metric only on the added condition that $g = g^*$? Such a supplementary condition does not seem easy to justify. But if it is not required, then how is it that the *absence* of measuring standards allows gravitational radiation to assume an ontological burden it was unqualified to bear in their presence?—From a comment you have made in a letter to me, I infer that you might wish to resolve this issue by allowing, in the presence of the "FCS," *both* g and g^* as "ontologically constituted" metrics (in the spirit of your discussion of "two-metric" theories). This would indeed secure a consistent treatment of the "metrical constitutive power" of gravitational radiation (so far as we have gone—but see below!). However, it would seem to conflict with your response to my argument in the Robb case, where you did *not* conclude that we must accept two metrics (one flat, the other not so), but rather

held the verdict of the "FCS" to be decisive, and your "relationalist" to win the exchange.

[In an important way, it seems to me, the discussion so far has been out of focus; for I do not believe that your requirement of sufficiently abundant gravitational radiation to determine all the null-lines of space-time—at least at some points—either *can* be realized (from this, if true, your relationalist will take comfort), or is a reasonable requirement to impose for the metric potency of gravitational radiation (so that I do not think that comfort is deserved). On the first point I speak with some diffidence, as insufficiently versed in the theory of the gravitational radiation-field; but I should expect, merely from the assumptions of continuous differentiability characteristic of this theory, that—strictly speaking—gravitational radiation can be propagated, at a given point in space-time, in no more than *one* direction. However that may be, the crucial point is the second. Here I take my stand, not on Winnie's theorem, but on what is implied by Stachel's question (quoted by you, p. 354): in effect, "If photons, why not gravitons?" In allowing light rays to play a role in relation to the metric, one surely does not restrict this role to space-times in which light alone is abundant enough to determine the metric fully (for it cannot, in principle, do so!). The most you require here is that *all members of the "FCS" taken together* are present in sufficient abundance to do the job. It seems, accordingly, when you speak of the "third position" (neither "relationalist" nor "semi-absolutist"—p. 354), that it would be fairer to the graviton—more in the spirit of your general treatment of the "FCS"—to allow metric power to gravitational radiation *whenever* it can be used to *supplement* other physical agencies in exploring the metric.

[But having said all this, and even assuming that you might be induced by these arguments to revise your stand—e.g., to withdraw your approval of my earlier discussion as a defense of "relationalism" against Winnie-Robb, advocating instead a "two-metric" view in that case, and to move further in the direction of the "third position" in the way I have just suggested—I am still left with an eerie feeling about these matters; a feeling that "Zeus has been dethroned, and Whirl is king." You cite with approbation three points of Allen Janis's, as counting against the appeal to gravitational waves alone to establish the space-time metric; and you conclude (p. 354) that Janis has thereby shown that "gravitational radia-

tion does not help the semi-absolutist." Now, since I do not myself understand the "semi-absolutist" 's doctrine, I cannot quarrel with the conclusion against him. But I do note that Janis's three points have precise counterparts in respect of your "relationalists" 's doctrine. One of those points is an argument against any appeal to "hypothetical test-gravitons"; but you, of course, also reject appeal to "hypothetical" objects of the "FCS." The other two points amount to this: that gravitational radiation may not suffice to determine the conformal (hence a fortiori the metric) structure, at all points, or even at any point, of a weakly empty space-time. Again, you acknowledge the same possibilities in respect of your "FCS" in nonempty space-times. I have discussed partially, in (2) above, what seem to me the severe difficulties this makes for your "relationalist" 's position—namely, so far as the clause about "all points" is concerned; and have suggested that a *reductio ad absurdum* threatens. I think, however, that the threat is even more dire than I implied there: for to me it seems obvious that there *cannot* be, at *any* point of *any* general-relativistic universe, a "sufficient abundance" of objects of *any* of the kinds you have considered—members of the "FCS" and gravitational radiation taken together—to "constitute" a metric in the way you seem to require. Indeed, through any point of space-time there can be at most one particle world-line; and this is surely not enough to "constitute" a metric, if I understand your use of that term—and your endorsement of Janis's arguments—at all.[6]

[If the foregoing is right, it seems to me to follow that *all* the positions you discuss in your comments on Winnie (pp. 353–5) essentially fail, *including* the "third position": that "the gravitational field can acquire added geometrical physical significance either from gravitational radiation itself or via the FCS." Rather, whatever "added geometrical significance" means, from the conditions you lay down upon this notion it seems to follow that there is *no* way the gravitational field can acquire it. And yet this failure to achieve such added significance in no degree obstructs our normal use of the language of metrical relations, and of geometry in general, to characterize the physical world; and in no degree inhibits our usual procedures of measurement. Does it not follow generally—as you suggest (pp. 349–50) in a special case—that to abandon for the tensor field g the claim to "added geometrical physical significance" is "no explanatory loss . . . but only an innocuous verbal one"?

[Let me conclude this section with two further points illustrative of my

sense of vertigo: my feeling of the presence, in all this discussion, of *Dinos—Vortex—Whirl*: (i) How does it come about that you are so concerned with the question of what "constitutes" the tensor g "metric," but that you are not concerned with what "constitutes" it "gravitational"? From what principled point of view is the notion of an empty universe with a metric structure *more* problematic than the notion of a universe devoid of matter but containing "gravitational radiation"? (After rereading your p. 304, I have tried to imagine the facial expression of Leibniz on contemplating the latter notion: too grim to describe!) (ii) Since, however, you are willing to concede not only that second notion, but the potency in such a universe (or at "weakly empty points" in a nonempty universe) of gravitational radiation—if only it is abundant enough to reveal all the null world-lines—to establish a "geometrical physical metric" for space-time, I wondered why you should draw the line here (that is, at gravitational *waves*): why not allow the *gravitational field itself* (waves or no waves) to "constitute" a metric? You now comment on this, saying that "whereas the wave-fronts of gravitational radiation pick out special space-time trajectories whose stipulated metrical nullity *then* might determine a metric tensor . . . , no corresponding determination exists in the gravitational fields of purely empty space-times: In the latter . . . , the gravitational field tensor g_{ik} would merely be *baptized ab initio* as also being physically metric in a physically redundant way."[7] Once again, you have not succeeded in making a distinction clear to me: I do not understand the principled difference between "stipulating the metrical nullity" of certain trajectories on the one hand, and "merely baptizing" a certain structure as metric on the other. (Does not Dinos seem to rule here?) As I see the two cases: (a) The "wave-fronts of gravitational radiation" are special space-time loci that can be constructed from the distribution of the gravitational tensor g; whatever "physical significance" they can claim derives from the "physical significance" of g itself, including its relationships with other structures that may have such significance. (b) Just as *some* space-time distributions of g may allow one in *some* regions to construct gravitational wave-fronts, and these in turn may allow one to construct *some* trajectories which are null-lines (of the tensor g!—see below); so *all* distributions of g allow one to construct *all* the space-time trajectories that are null geodesics of the tensor g, and all the timelike geodesics as well: an array of structures sufficient, in turn, to allow reconstruction (up to a constant factor) of "the metric tensor g." I still quite fail to grasp the basis

for your allegation of a deep difference in principle between these two constructions. Indeed, it seems to me obvious that there was never a philosophical, or a "principled" physical, reason to "stipulate the metrical nullity" of the world-lines of "gravitational rays": one is led to the possibility of using gravitational waves to explore the metric only by the *theorem of general relativity* that *such world-lines are necessarily null-lines* of g. In what I have above called the historical, or etymological, order, it is clear that the "metrical significance" of gravitational waves is derivative from, not constitutive of, the "metrical" character of g (even if the latter is regarded as a merely nominal affair). You maintain the existence, and importance, of a quite different relationship in the ontological order; but notwithstanding all the explanations you have given, the latter remains entirely dark to me.]

(5) One last point in this series (whose intent, I remind you, is to try to make plain *in what way your usage seems to me confused*—or *is* to me *puzzling*; not to demonstrate conclusively that your usage is in fact confused, or cannot in any way be coherently explicated): On pp. 345–6, you say that "the relationalist's view . . . derives its intellectual inspiration from Riemann's ontology of physical geometry" (as you construe the latter —we have some differences over it), "instead of being a holdover from the discredited homocentric ontology of operationalism or of the verifiability theory of cognitive meaning." I conclude that the Riemannian heritage—by contrast with the "discredited" sources you mention— counts as a recommendation (and on *this* we do not differ: in the matter of worshipping heroes, I would be loath to place any altar above Riemann's). But on p. 359, you make the (generalized) Riemannian "OI" (as you see it) a "broadly empirical" claim. Since Riemann does not offer a *shred* of empirical evidence, but only *conceptual argument*, in the passage that is your source for "OI," I am nonplussed.

[So I wrote before. You have now explained (p. 319) that you do not mean to invoke Riemann's authority to "lend credence to RMH." Yet you have allowed your statement on pp. 345–6, cited above, to stand—in which you contrast (I assume favorably) the intellectual inspiration derived from Riemann's ontology of physical geometry with the discredited ontology of the verifiability theory of meaning. This seems to me to leave things in a most unsatisfactory state. If you *reject* Riemann's own view, as aprioristic, in favor of an *empirical* hypothesis OI, why should you not dissociate the latter from Riemann's "discredited *a priori* ontology" just as much as from the "discredited ontology of operationalism"?

[You, I, and Riemann are in the following relationship here: You interpret Riemann's conclusion in one way; I in another. You believe that Riemann's argument for your version of his conclusion was utterly worthless (since in your version that conclusion is an empirical hypothesis, and since as I have said Riemann adduces no empirical evidence whatever); I believe that Riemann's argument for my version of his conclusion was essentially correct. Moreover, you distinguish in Riemann two hypotheses (OI, which appears at the beginning of his lecture, and DH, which appears at the end), and you argue that Riemann incorrectly inferred the second from the first; whereas in my reading, the application Riemann makes of his earlier remarks when, at the end of his lecture, he turns his attention to physical space, is entirely cogent. Under these circumstances, it is indeed altogether possible that you are right about Riemann and that I am wrong about him; but it is also certain that *if* you are right about Riemann, then Riemann was (in quite fundamental ways) *wrong* (and if I am right about Riemann, then he himself, on those fundamental issues, was *right*). It therefore seems to me very misleading—and, I venture to suggest, self-deceiving—for you to continue to claim to uphold a Riemannian philosophy of geometry.]

I should like now to try to clarify two aspects of my view of these matters, in connection with a pair of passages in which you comment on remarks of mine. First, you refer on p. 363 to my having said that mathematicians nowadays know of many more "structures" than were known in the time of Riemann; and you do not see how this is to the point. Let me try to explain what I had in mind:

In the ancient tradition of mathematics, its subject matter was conceived of as certain *particular "formal" aspects of the world*; most usually, under the two heads of "discrete" and "continuous" *quantity*. Riemann was one of the pioneers in breaking through to a freer and wider conception—one that is beautifully expressed in the saying of Riemann's devoted friend Dedekind, that the objects of mathematics (e.g., numbers) are "free creations of the human mind." In the older view, "space" was conceived, under the head of "continuous quantity," as a definite object of study,[8] having definite metrical attributes (cf. Kant, "Transcendental Aes-

thetic," section 2: "Space is represented as an infinite *given* magnitude"). *One* of Riemann's great mathematical contributions was the demonstration that, unlike the case of "discrete quantity" (where to determine a set is necessarily to determine its "quantity," or cardinal number), and contrary to all traditional expectation, in the case of a "continuous manifold" *the concept of such a manifold, and of* (roughly speaking) *its "continuity" or "smoothness" properties,* can be *separated* from *any* "metrical" determinations. *This* is what I believe to be the substance of the remarks of Riemann from which you distill "OI."

Now, it is in the older, the pre-Riemannian tradition that it appears appropriate and natural, if a "spatial manifold" is spoken of, say with various structures on it (like your flat paper with its map of the earth), to ask, "Which of these has the *added* significance of being *the* structure—topology, metric, etc.—of the spatial manifold?" In the newer conception, both of *what mathematics is about* and of *how mathematical concepts are brought to bear in physics,* such a question, even if it concerns a "physical manifold," seems to me inappropriate and even, in its tendency, obscurantist. A manifold as such *need* have *no* special, or distinguished, "metric" structure at all; on the other hand, it may have more than one such. If—as in several speculative theories you mention—physical spacetime were to have more than one physically significant structure of a "metric" type, that would pose no special difficulty; one would only need to know how these several structures are related to physical processes and phenomena. To ask which of them has the *really geometrical* significance for space-time would be unreasonable, in a way whose connection with my remark about the multiplicity of mathematical structures may be made plainer by an analogy with a purely mathematical situation—and one from number theory (whose traditional domain was "*discrete* quantity"): In the theory of algebraic number-fields, the concept of the "whole numbers" or *integers* of a given algebraic field is introduced, *generalizing* the elementary concept of the (rational) integer. The generalized concept is "absolute" (not relative to a field), in the sense that if an algebraic number is an algebraic integer of any field, it is an integer in every field (algebraic over the rational numbers) to which it belongs. Now, $\sqrt{2}$ is an integer in this generalized sense of the term. Suppose someone objected to this rather scandalous statement by asking: "But is $\sqrt{2}$ an integer in the *ontologically arithmetical* sense of the word?"—or: "*By virtue of what* does a number qualify as *whole* in the *mathematically numerical* sense?"

The point is again, that we must not be slaves to terminology. *Should* $\sqrt{2}$ be called an "algebraic integer"? Such questions have no answer: they are not well posed. The terminology of algebraic number theory is a very good one; it is clear, and it allows a rich abundance of relationships to be expressed perspicuously. But it is not *necessary*; one cannot even say that it is "optimal," or the "most natural" terminology: it is quite conceivable that some very different way of speaking might do just as good service in expressing the same relationships; and in the development of mathematics, good terminology *has* sometimes changed for the better. In any case, the question of terminology, although by no means negligible, is distinctly secondary: it has to be the *Begriffe* that control, not the *Wörter*. (Your "relationalist" and "semi-absolutist" quarrel—pp. 347ff.—over *which one* is "playing with words"; but I see them *both* as engaged in a battle over words without clear notions: *bilateral* logomachy!)[9]

The other passage I want to refer to is that on pp. 332ff., in which you use Jack, with his two feet and his unclehood, and Presidents Ford and Giscard, to dispute my skeptical comments on the notion "intrinsic to space." Here I feel distinctly to blame for certain deficiencies in my discussion (letter of February 16, 1975; referred to by you on pp. 327–8 and 337) of the difference between the cases of *space* (or space-time) and *an ordinary physical object*; and to some degree also in my conference remarks (passage quoted by you on p. 329)—which, as you know, were prepared somewhat hastily at the conference (and which utilized some material from an earlier letter to you). I think, now, that there are issues besides that of sheer individual "identification" that need to be emphasized. The phrase in my earlier statement to you (quoted by you, p. 329), referring to the difficulty "of giving an appropriate and adequate *characterization*" of the "object" in question, seems to me better chosen. The necessary point is also suggested in the earlier part of my paragraph about the globe: I said that I can, prima facie, entertain the question whether the globe has "intrinsically" the property of spherical shape (a question which seems to me, in its most natural "ordinary" interpretation, to have nothing to do with the fact that to test the globe's shape requires "external" instruments—if only, for a rough judgment, eyes and light). But I added that the qualification "prima facie" was there because "I don't think the question is a clearly posed one in this bald formulation (consider, e.g., Descartes's meditation upon what is 'intrinsic' to the piece of wax)." In other words, since I do not think you have succeeded in giving a

satisfactory general explication of "intrinsic," I have to consider *what notion of "intrinsic" is appropriate to the context of the question*; and in the next portion of my paragraph, I suggested that in the case of the globe the relevant notion—the one that seems the most likely guess at the questioner's intent—is that of the distinction between the *internal physical constitution of* and *external constraints upon* a body: so that if a hollow glass globe is filled with water, one might reasonably say that spherical shape is an "intrinsic" property of that piece of glass (as constituted at that time), but an "extrinsic" property of the water.

Now, where did all this interpretation of "intrinsicness" in the given context come from? (Incidentally: it may be *mis*interpretation; the questioner may have something else entirely on his mind; and *you* may have quite a different view about what to consider "intrinsic" to the globe. You have not commented on this.) It came, in fact, from the circumstance that (as I have already said) I know what object you mean by the globe: your description serves to distinguish for me an object of whose "nature," or general characteristics, I have a conception quite independent of that description. But it is not "acquaintance" that is critically important here, and I was wrong to mention it: certainly not acquaintance with the *object* (in point of fact, I have never seen your globe); but also, not even acquaintance with *objects of a similar type* (although that is, in the present case, a principal source of the crucial knowledge); rather, what is really important is that I know enough about the thing you are talking about to have some kind of general view of the structure or attributes of such a thing. (Even this, I remind you, is not decisive: it merely makes prima facie understanding possible of what I have to consider a somewhat vague "presystematic" question—pending a satisfactory general explication of "intrinsic," which I do not really expect to see.) But clearly, in this respect "Uncle Jack" and the globe are entirely analogous—each belonging to a kind about which I possess an abundance of lore (or prejudice), namely *bodies* and *people* (antirespectively); and are quite unlike space-time, about which I happen to be deficient of such prejudice. Tell me exactly what *you* mean by 'space-time.' "I mean that structure," says Newton, "by virtue of which the notions of *length*, *duration*, *uniform rectilinear motion*, and *acceleration* are applicable to physical systems and processes." Ah, yes, I say, a metric is intrinsic to space-time for Newton.[10] "I mean," says *X*, "the mere four-dimensional differentiable manifold, independently of any further structure"; "I," says *Y*, "mean the smooth 4-manifold,

with the distinction of directions at each point into *spacelike* and *causal* (timelike or null) directions." Ah, yes, I say, for *X* and *Y* the metric is extrinsic to space-time; whereas for *Y*, but not for *X*, the conformal structure is intrinsic. I not only have no criterion for adjudicating among these views; I have no *desire* to adjudicate, *see no point* in adjudicating, among them.

Again I feel it necessary to emphasize, so as to obviate any possible misunderstanding over it, the element of *personal judgment* in the position I have just expressed: I do not say that *there can be no criterion* for deciding among such views, or that there *is* no point in deciding; but that *I have* no criterion, and *see* no point in seeking one. It is certainly possible that I have overlooked something; but I cannot be persuaded that I have by a general argument, to the effect that such criteria are *conceivable* and *might* be of value, because that is what I have just admitted.[11]

But in reflecting further on this matter—on *why* you and I differ so much in our satisfaction/dissatisfaction with a position of openness on what is to be taken as "intrinsic"; why I am willing, and you are not, to consider this essentially a question of *façon de parler*—it has occurred to me that there is, after all, a point of "ontological perspective" involved, and one not without interest. You tend to think of the world in terms of "things"—"primary substances," in Aristotle's sense. I do not: I tend, rather, to think (Platonically?) of "structures" and "aspects of structure" ("Forms"?). Let us put aside modernity, and space-time with it; what do we mean—what did "we" *ever* mean—by "space"? Almost everyone who considers this thinks in the first instance of the question "Is there really *such a thing* as space?"—and the "absolutists" are ordinarily presumed to be the ones who answer affirmatively. But I find myself in profound agreement with Newton on this point (and I am exceedingly grateful to you for the stimulation of this discussion—because the significance of the text I am about to quote had never been clear to me until, just a few days ago, I was suddenly struck by its bearings upon our difference): Newton rejects the view that *extension* is either a "substance," or an "accident," or a "nonentity"; and asserts rather[12] that it

has its own way of existing, which fits neither substances nor accidents. It is not a substance both because it does not subsist absolutely of itself [*absolute per se*], but as it were as an emanative effect of God and a certain affection of every thing [or "every being": *omnis entis affectio*]; and because it does not stand under those characteristic affections that de-

nominate a substance,[13] namely actions, such as are thoughts in a mind and motions in a body. . . . Moreover, since we can clearly conceive extension as existing without any subject, as when we imagine extra-mundane spaces or places void of any body [vacuum solutions and vacuum regions!] . . . , it follows that it does not exist in the way of an accident inhering in some subject. And hence it is not an accident. And far less is it to be called nothing, for in fact it is more a "something" than an accident, and comes nearer to the nature of a substance. Of nothing no Idea is given, nor has it properties; but we possess, of extension, an Idea the clearest of all, to wit by abstracting the affections and properties of a body, so that there remains only the uniform and unlimited stretching out of space in length, breadth, and depth. Moreover, its many properties are concomitant to this Idea. . . .

Having ruled out each item of what on the face of it seemed an exhaustive list of alternatives ("substance," "accident," and "nothing at all"), Newton propounds his own solution to the ontological problem of space in the following terms:

Space is an affection of a thing *qua* thing [or "of a being *qua* being"].[14] Nothing exists or can exist which is not in some way referred to space. God is everywhere, created minds are somewhere, and a body [is] in the space that it fills; and whatever is neither everywhere nor anywhere is not. And from this it follows that space is an emanative effect of the first existing thing;[15] because if anything is posited space is posited.

How is this quaint, even bizarre-sounding doctrine (and I confess that until recently it did seem so to me), to be understood: the doctrine that space is "neither substance nor accident" but *affectio entis quatenus ens*? I suggest that Newton is here expressing, in terms close to those that still prevailed in philosophical school-training, essentially the same view that I put to you when I spoke (your p. 329) of spatio-temporal attributes as "objective structural characteristics of the natural world": his doctrine is that *the fundamental constitution of the world*—its "basic lawful structure"—*involves the structure of space*, as something to which whatever may exist must have its appropriate relation. Notice that, contrary to a rather widely held interpretation of Newton (based upon his references to space as God's "sensorium"), he does not here represent space as an *organ* of God (which of course would be a kind of "thing"), but as what he calls an "emanative effect" of whatever thing first exists—because it is, as it were, *an aspect of the nature of "thinghood."* Transposing to a modern context, and assuming Einstein's GTR as "fundamental theory," we may

paraphrase in this way: Whatever exists of a physical nature (let us leave out God and minds)—particles, fields—must be appropriately related to a space-time manifold with a fundamental tensor-field satisfying the Einstein equations. If, therefore, the real world is not the empty set(!), it must be characterized by the structure of such a manifold with such a field (Riemann-Minkowski metric): the latter is, in this sense, an "emanative effect" of the existence of anything. (I believe we may reasonably understand "emanative effect" to signify something that is not "created," or "produced by causal agency" (in accordance with "causal laws"), but that *is entailed by* (or "flows from") *the nature of something*.)

Perhaps I have been carried away into too long a digression upon Newtonian metaphysics. The essential point of view that I have been trying to explain is one that I believe Riemann also expresses—for instance, when he speaks of "the real"—or "actual"—"that underlies space" ("das dem Raume zu Grunde liegende Wirkliche")[16]: space, or geometric structure, is *an aspect of the structure of the world*. If, in this way, one does not think of space prima facie as a separate "thing" at all, you see that the question what is "intrinsic" to it can hardly mean anything else than, "Of the components or aspects of physical structure, which are the ones that we *call* 'spatial' or 'geometric'?" The point of view seems to me a fruitful and enlightening one; it puts the emphasis, I believe, on issues of genuine interest, and avoids (to use Riemann's term) "idle questions." Among the latter, in my opinion, one of the worst sorts—one that is not merely idle, but "reactionary," i.e., obstructive of scientific progress (and subversive of clarity)—is the demand, when some *fundamental conceptual change* occurs in scientific theory (some change in the basic characterization of physical structure), to be told—not just, as is reasonable and necessary, how the new conceptions "correspond" to the old, i.e., what the general connections are between them of agreement or disagreement in application—but what, in the structure posited by the new theory, *is the same* (or merits designation by the same term) as something posited by the old theory. (A recent example of this kind of unenlightening onomatologizing is the discussion of the question: When Newton spoke of "mass," was he referring to what we know as "rest mass" or to what we know as "relativistic mass"?) I believe that the effect of this kind of demand is the precise contrary of the admirable end proposed by Riemann, in a passage that I have quoted in my paper: Riemann, you will recall, said that investigations such as his own can serve to ensure that the work of

science shall not be hindered by a narrowness of conceptions, and that progress in the knowledge of the connections of things shall not be hampered by traditional prejudices. I know very well how far you are from wishing to defend narrow conceptions or traditional prejudices, or to trade on obscurity; but my most serious doubts about your point of view towards ontological questions of space-time lie in my suspicion that, quite counter to your intentions, that is their ("intrinsic") tendency: that to ask "Is—or *by virtue of what* is—the tensor-field *g* properly *geometric*?" is to ask a question of exactly the kind I have been deprecating.

Be it sae or be it nae, I hope at least that what I have now written will help to make it clearer to you (a) how I prefer to look at these questions, (b) how that way of looking influences my answers to them, and (c) what sorts of consideration recommend that way of looking to me. If I have been in the least successful in this, it should be apparent that the position you have assigned, in your paper (pp. 356ff.), to the "semi-absolutist," [which was presented, in an earlier version,] as an interpretation of my opinions, is very far from mine. I do not at all understand by what logic of exegesis you infer (p. 357), from the fact that I speak of the differentiable manifold of world-points and say that it "carries a physical field with the characteristics of the Einstein metric field" (a statement that seemed to me, and still seems, as simple and philosophically colorless a summary of the direct content of GTR as I can conceive), that I affirm what you call "TAM"—a verbal formula that seems to me grotesque, monstrous, and utterly devoid of sense. To try to avoid any possible misunderstanding on this issue, let me spell out more explicitly what my statement involves, and why I think it cannot have anything to do with TAM—*whatever* the latter may mean: (1) GTR postulates a four-dimensional differentiable manifold, ordinarily called "space-time"; I presume there is no controversy about this. Such a manifold, by its purely mathematical character, has associated with it various derived structures, in particular what is called its "tensor bundle"—and within the latter, the sub-bundle of "nondegenerate second-order symmetric covariant tensors of Lorentz signature." A tensor-*field* is a function or mapping of a particular sort from the manifold to its tensor bundle (a mapping of the sort called a "cross-section of the bundle"). There are also analogous notions connected with what are called "tensor-*densities*." (2) Physical theory—here speaking very generally—postulates the existence of various more special "physi-

cal" structures: i.e., among all the possible relations, functions, etc., mathematically conceivable (mathematically *existent*) within the context already laid down, certain particular ones (of particular types specified by physical laws), to be taken as "physically distinguished." Among these are (or may be), for example, "world-lines of matter-flow," "scalar-density field of mass," "tensor-density field of impulse-energy," "scalar-density field of charge," "electromagnetic tensor-field," etc. (3) More especially, Einstein in his GTR postulates a physical field which is a cross-section of the bundle of nondegenerate, second-order, symmetric covariant tensors of Lorentz signature; and he postulates certain relationships among this field and other physical structures. The field postulated by Einstein is often called the "metric field," or the "metric tensor," or indeed just the "metric." (4) We may turn now to my statement. Its reference to the differentiable manifold of world-points ought to be unproblematic, by (1). That this manifold "*carries*" a certain field means no more than that this field exists "on" the manifold—i.e., is a mapping from the manifold into the proper bundle (and, one should add, with the proper attributes: we want here a differentiable cross-section); surely, so far, no extra metaphysics. That the field is "physical": I have indicated what I mean by this in (2). Now, the critical point must somehow be the word 'metric'— since from my statement you infer "TAM," the "Thesis of Autonomous Metricality." But (a) my only use of that word to describe the field is an *indirect* one; my statement seems to me a very *model* of caution (not to say *excessively* cautious): it does not say that the field "is the—or a—metric," but only that it "*has the characteristics* of the Einstein metric field," i.e., the field *g* that occurs in Einstein's GTR and is there (often) called "the metric field." What those "characteristics" are I have indicated in (3) above: they include the structural attributes of the field considered by itself (namely, that it is a cross section of the bundle mentioned), and the laws relating this field to other physical structures (whether or not, I should add, these other structures might prove to be derivable from the Einstein field itself). I hope this is sufficient to establish my innocence of committing TAM. But in case I may seem to be constructing a lawyerly argument on the basis of my cautious language (although I assure you that the language was *deliberately* cautious, so that I was shocked to find it used against me!), let me now also point out (b) that even if I *had* meant to imply that the Einstein field *g* "is physically—or ontologically—metric,"

this *still* could not fairly be read as an endorsement of the view that its "metricality" is "*autonomous*," in whatever sense of that (to me obscure) assertion is obnoxious to you. For you, I believe, *do consider* the Einstein field to be "the metric of space-time"—although not "autonomously" so, but *by virtue of* something, namely some set of relationships to physical structures ("external metrical standards"). *But these relationships are, then, themselves among the "characteristics" of "the Einstein metric field"*; and, *ex hypothesi*, they *entitle* it to the designation "metric." Turn it which way you will, my statement *cannot* be made to yield the construction you have put upon it (and your p. 327 tempts me to add: "Q.E.D., Mr. Walter Shandy!").

Notes

1. It might, in fact, be urged that there is a motive for this usage independent of such considerations: namely, despite the correctness of your remark (p. 342) that there are other second-order, symmetric, physical tensor-fields on space-time (which in the purely mathematical sense qualify as "generalized metric" structures), the tensor-field *g* is nevertheless the only such physical field known that is *in principle* an *everywhere nondegenerate* metric; and, what is perhaps most telling, the only one that allows us to separate "spacelike" and "timelike" structures within space-time.

2. Since you ascribe (p. 342) to the tensor-field *g* both "the ontological status of being *the metric field* in the sense of endowing space-time with the latter's particular metric structure" (whereas I do not understand why this *is* an "ontological status"), and "the physical significance of being the metric tensor of space-time" (whereas I do not understand what this "physical significance" consists in); and similarly (p. 345) your "relationalist" speaks both of "the particular added *physical* significance," for the field *g*, of "being the *metric* tensor field of . . . space-time," and of "the *ontologically constitutive* mediation of the FCS."

3. It occurs to me that this can be challenged, so far as light rays are concerned—cf. the section on Riemann in my own paper for the Minneapolis and North Andover conferences. But (a) insofar as space was ordinarily conceived to be fully "measured out" by measuring rods, the law of the rectilinear propagation of light was usually regarded as an empirically established natural law, and not as in any way "constitutive" of spatial notions; and (b) for those who, like Riemann and Poincaré, did stress a fundamental role of light in geometrical determinations, that role had to do with the distinguishing of *spatial straight lines*—which is not at all the role played by light in relativistic measurement.

4. As objects; I am not enamored of their description, especially of the word "external."

[5. To prevent misunderstanding, it should be noted that this remark, in the sense in which it is intended, stands despite your acknowledgment to Winnie and Stachel that under suitable circumstances gravitational radiation may be admitted into.the "FCS": for among those required circumstances is *concordance*, whereas I am here envisaging a possible *failure of concordance*.]

[6. That there can be no more than one particle world-line through a point is a clear consequence of the assumption of the existence, continuity, and differentiability of the impulse-energy tensor. In correspondence, you have questioned the point, saying that it "would make nonsense of particle collisions and various other kinds of coincidences." But what in practice we call "coincidence"—as of a pointer with a mark on a scale—is merely near juxtaposition. As to collisions, it is enough to remark (a) that the space-time point of a collision in the strict sense, involving an *instantaneous* interaction, must necessarily be a *singularity* of the world lines involved (and therefore, presumably, on an ultimate analysis, of space-time itself)—so that "constitution" of the metric at such a point is out of the question; whereas (b) if "collision" is taken to involve, not an instantaneous, but only a *rapid* interaction, the bodies participating in it are, precisely during that process, not freely gravitating bodies—hence not qualified to "constitute" the metric.

[It might be asked how, if what I have here said is true, we ever actually determine ("measure") the metric. But clearly, in practice, our body of data is always *finite*; our conclusions about continuous structures are drawn partly by interpolation and extrapolation, and largely by inductive inferences guided by far more powerful theoretical principles—which, of course, are themselves based upon earlier instances of the same sort of process, so that an accurate genetic analysis of the whole construction would be extraordinarily intricate. (I must remark, autobiographically, that one of the crucial incidents in my own divorce from the verifiability theory of meaning in any strict sense of the latter occurred when, as a student, I read Reichenbach's *Axiomatik der Relativistischen Raum-Zeit-Lehre*, and saw that he postulates at each point of space-time an observer riding a particle in each possible state of motion, with all the observers sending and receiving light signals in all possible ways—somehow always recognizing the source of each signal! An "abundance" that is impossible to achieve—and is even, in a sense, inconceivable—cannot be a necessary condition for *knowledge that we actually have*; analogously, an abundance that is impossible cannot be a necessary *ontological* condition of a structure that we can attribute, without incoherence, to the real world.)]

[7. The passage is quoted from a letter, Grünbaum to Stein, dated 5 March 1976, which proposed the insertion of a new sentence at the end of part C of Grünbaum's section *The Relationist's Account of . . . Space-Times*, and invited comment on this sentence. In Grünbaum's final version, the inserted material has been revised and considerably expanded: it now forms the concluding paragraph of that part C (pp. 355–6). Instead of adapting my comments to this new exposition, I have decided to leave them as they were first formulated: Grünbaum's paragraph can thus be considered as—at least in part—a reply to the criticism made here.]

8. I here use 'object' in the sense of the older—medieval and Cartesian, not Kantian—distinction of "objective" and "subjective"; or of Locke's explanation of "idea" as "whatsoever is the *object* of the understanding when a man thinks."

9. There is what seems to me a similar mere verbalism in your discussion of the issue of "infinite numbers" (pp. 305ff.): You repeatedly speak, here, of "infinite *cardinalities*." Now, the word 'cardinality' no more possesses an "intrinsic" virtue or import than (say) the word 'integer' (cf. just above). The first useful definition, and of course the only one now used, of a notion of "cardinality" applicable to infinite sets, is Cantor's. On p. 367 (n. 7), you indicate that the doctrine you are discussing was not associated (according to Maier) with "the modern Cantorean distinction between denumerably and super-denumerably infinite sets." Now, if infinite sets have different "cardinalities" (in *some* sense); and if, among these, some are *larger* than others (as seems clearly implied); and if the set of the natural numbers is not maximal (as also seems clearly implied); then some sets will have "cardinality greater than that of the set of the natural numbers." The only sense I can make of a claim that "Cantor's super-denumerably infinite sets" are not involved here is that Cantor's *criterion of comparison* was not adopted. So far, well and good; except that it is then unclear (a) what criterion *was* assumed—or whether *any* clear notion really existed (Answer: almost certainly not!); (b) why the term 'cardinality' has any appropriateness here (was it actually used in the scholastic tradition?); and (c) why, if an honest *concept* of cardinality (or number) was in fact lacking, you should find any comfort here for your theory of the "intrinsic," or should conjecture an affiliation with Newton: is it a purely *verbal* tradition that you think Newton may have been following? But beyond this, real trouble—indeed, utter confusion—sets in for me when you say (p. 307) that the theory that different-sized point-sets have different infinite numbers of points was *refuted* by *the exhibition of one-to-one correspondences* between such sets; for this *is*, after all, Cantor's criterion of comparison! In this discussion, concepts seem to be in a

state of continual fluctuation, and only words to be constrained into a degree of stability. I can quite well believe that such confusions were present in the historical situation; my complaint against you is that you seem to see no need to explicate the issues, or to resolve the confusions.

10. I here speak somewhat loosely, without care for the distinction between the Newtonian and Minkowski space-time structures.

11. Similarly, when I said, in the passage you quote on p. 329, that "I see no way to confront the former question"—namely, whether a certain relation is involved in the structure of "the space-time manifold itself, considered apart from all other entities"— "independently of the latter" (how to explicate the notion of "the space-time manifold itself"—to draw a line, so to speak, between it and "all other entities"); and that yet the converse may also seem to hold, etc.: I was not claiming to offer a *proof* of vicious circularity in the enterprise under discussion. So your reply, in the Uncle-Jack-and-President-Giscard passage, aimed at refuting in general terms a charge of "necessary circularity," is in my opinion not to the purpose: *I still see* no way—you have certainly not shown me one—to confront the first question independently of the second, or to answer the second without begging the first.

12. *De gravitatione et aequipondio fluidorum*, in Hall and Hall, ed., *Unpublished Scientific Papers of Isaac Newton* (Cambridge University Press, 1962), pp. 131–132, 136; Latin on pp. 99–100, 103. Hall and Hall most irritatingly render *affectio* throughout as "disposition"—one among many seriously misleading mistranslations in their English version of Newton's Latin text.

13. Again the Halls give a really terrible mistranslation: they have "*it is not among the proper dispositions that denote substance.*" But it should be plain to anyone with a rudiment of philosophic discrimination that when Newton writes "*Non est substantia tum quia non absolute per se . . . subsistit; tum quia non substat ejusmodi propriis affectionibus quae substantiam denominant,*" the two verbs in the two dependent clauses—*subsistit* and *substat*—are deliberately chosen for their association with *substantia*: substance is what is self-subsistent, and is also the substrate or supporter of properties. The meaning of *substat* has to be, not "stands *among*," but "stands *under*": extension is not a substance because it does not support—underlie—stand under—the "characteristic denominations" (what Frege would call the "*Merkmale*") of a substance.

14. The article may, of course, be disputed, since Latin possesses no articles. Newton's text reads: *Spatium est entis quatenus ens affectio*; and the Halls render: "Space is a disposition of being *qua* being." The expression "being *qua* being" has indisputable standing in the metaphysical tradition—but not, I think, as denoting an individual subject of attributes, something that has "affections" (or "dispositions"). The translation I have given seems to me consonant with the sentences that follow.

15. Not, as the Halls translate, "of the first existence of being"!—The "first existing thing," of course, according to Newton, is God (of whom he has previously characterized space as "an emanative effect (as it were)"; but it is noteworthy that the *reason* he gives for his statement that space is "an emanative effect of the first existing thing" is *quite independent of what thing that may be.*

16. In what you call Riemann's "DH," it is this *Wirkliche*, rather than the spatial manifold itself, whose "binding forces" are said to give rise to the "Massverhältnisse" in the manifold: that is, Riemann does not speak of binding forces as *acting upon the spatial manifold* (a notion it is hard to make any sense of), but as *acting upon "the real."*

Simultaneity in Newtonian Mechanics and Special Relativity

1. Introduction

Everyone will agree, I think, that the transition from Newtonian mechanics to special relativity taught us something of fundamental importance about time and simultaneity. Many philosophers have urged that there is a significant *semantic* lesson to be learned from this transition. For example, the following kinds of views have been expounded: Einstein was aided in his discovery of special relativity by an analysis of the "concepts" of time and simultaneity; the transition from Newtonian mechanics to special relativity resulted in a profound "change of meaning" of 'time' and 'simultaneous'—a change that was so extensive as to make any comparison of the two theories problematic; in a special-relativistic world the notion of simultaneity is in an important sense conventional—statements about distant simultaneity lack truth-value, they are mere "definitions."

Defenders of such views rarely provide explicit semantic theories within which their claims can be evaluated. There are, however, philosophical theories of meaning lurking in the background. Thus claims that Einstein analyzed the "concept" of simultaneity fit naturally into an operationalist account of meaning, since what Einstein did was to discuss ways of measuring time and distant simultaneity. Similarly, claims about the conventionality of distant simultaneity are supported (at least in Reichenbach's case) by a verificationist theory of meaning. Statements about distant simultaneity in a special-relativistic world lack truth-value, it is argued, because they are unverifiable in principle. On the other hand, views that make much of the noncomparability or "incommensurability" of the meanings of 'time' and 'simultaneous' in Newtonian mechanics and special relativity seem to involve some kind of "contextual" theory of meaning—meaning is to be identified with "role in theory" or the like.

In this paper I would like to see what light can be shed on these