

## HOW TO KEEP THE 'PHYSICAL' IN PHYSICALISM\*

Physicalism is roughly the thesis (1) that every entity is either itself a physical entity or is exhaustively composed, ultimately, of physical entities, and (2) that every property is either itself a physical property or is realized, ultimately, by physical properties.<sup>1</sup> Never mind whether exhaustive composition and realization are the best notions for a physicalist to employ in explicating the idea that everything is physical—better, for instance, than the notions of token identity and supervenience;<sup>2</sup> and never mind also how these notions are to be precisely understood. The problem I wish to discuss is what physicalists can and should mean by 'physical' in their formulations of physicalism, and this problem arises on any reasonable view as to the answers to these questions.

A physicalist's definition of 'physical' can perfectly well be stipulative—the problem is not one of conceptual analysis—but to be plausible it must at least meet the following two conditions: when plugged into an otherwise satisfactory formulation of physicalism, it must yield a thesis (1) that is not obviously false and (2) that possesses content determinable by us. (A physicalism whose content was not determinable by us would presumably be impossible for us to support empirically, and might, for all we know, not even exclude from existence the sort of paradigmatically nonphysical items—for example, souls, entelechies, ghosts—which physicalists have traditionally refused to countenance.) There is a dilemma, however, apparently owed to Carl G. Hempel,<sup>3</sup> that is sometimes thought to show that no physicalist definition of 'physical' can

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<sup>1</sup> See, for example, my "Being a Physicalist: How and (More Importantly) Why," *Philosophical Studies*, LXXIV (1994): 221-41.

<sup>2</sup> See, for example, John F. Post, *The Faces of Existence* (Ithaca: Cornell, 1987).

<sup>3</sup> "Comments on Goodman's *Ways of Worldmaking*," *Synthèse*, XLV (1980): 193-99, especially pp. 194-95. See also his "Reduction: Ontological and Linguistic Facets," in Sidney Morgenbesser, Patrick Suppes, and Morton White, eds., *Philosophy, Science, and Method: Essays In Honor of Ernest Nagel* (New York: St. Martin's, 1969), especially pp. 180-83. Compare J. J. C. Smart, "The Content of Physicalism," *Philosophical Quarterly*, xxviii (1978): 339-41; and Noam Chomsky, *Language and Mind* (New York: Harcourt Brace Jovanovich, 1972), p. 98.

meet these two conditions simultaneously.<sup>4</sup> It is nicely expressed by Geoffrey Hellman:<sup>5</sup>

...current physics is surely incomplete (even in its ontology) as well as inaccurate (in its laws). This poses a dilemma: either physicalist principles are based on current physics, in which case there is every reason to think they are false; or else they are not, in which case it is, at best, difficult to interpret them, since they are based on a "physics" that does not exist—yet we lack any general criterion of "physical object, property, or law" framed independently of existing physical theory (*ibid.*, p. 609).

So if "physical" entities and properties are those mentioned as such in the laws and theories of *current* physics, then physicalism is very probably false; but if they are those mentioned as such in the laws and theories of *completed* physics, then, since we have no idea what completed physics will look like, the resulting formulation of physicalism will lack content determinable by us. I shall argue here that the first horn of this Hempelian dilemma is blunt, and that therefore it remains open to a physicalist to understand by "physical" entities and properties those mentioned as such in the laws and theories of *current* physics.<sup>6</sup>

#### I

Here is what is supposed to be wrong with defining 'physical' in terms of current physics. Past theories in physics, when judged from the standpoint of current physics, have usually turned out to be both false and incomplete; it is therefore very likely (though not, of course, absolutely certain) that current physics is both false and incomplete.<sup>7</sup> But if so, and if physicalism formulated in terms of cur-

<sup>4</sup> See, for example, T. Crane and D. H. Mellor, "There is No Question of Physicalism," *Mind*, xc (1990): 185-206, especially p. 188; and Crane, "Why Indeed? Papineau on Supervenience," *Analysis*, LI (1991): 32-37, especially p. 34. See also Bas C. van Fraassen, "Science, Materialism, and False Consciousness," in Jonathan L. Kvanvig, ed., *Warrant In Contemporary Epistemology: Essays in Honor of Plantinga's Theory of Knowledge* (Lanham, MD: Rowman and Littlefield, 1996), especially pp. 163-70 and 173-74.

<sup>5</sup> "Determination and Logical Truth," this JOURNAL, LXXXII, 11 (November 1985): 607-16, p. 609.

<sup>6</sup> For a critical survey of other responses to the dilemma, plus a positive proposal very different from mine, see Jeffrey Poland, *Physicalism: The Philosophical Foundations* (New York: Oxford, 1994), ch. 3.

<sup>7</sup> It is not entirely clear what an advocate of Hempel's dilemma should or even could mean by 'incomplete' here; but I assume the intuitive idea is that current physics is incomplete if and only if it fails to mention some entity or property which (a) exists and which (b) would, if discovered, unhesitatingly be classified as physical—such a thing as a new particle with mass, charge, and spin. A difficulty with such an account, of course, is that it *presupposes*, contrary to the conclusion of the dilemma, that a viable conception of 'physical' is now available to us, but an advocate of the dilemma can no doubt get around this problem.

rent physics assumes the truth and completeness of current physics, then it is very likely (though not, of course, absolutely certain) that physicalism is false, too<sup>8</sup>—which requires one to cease to be a physicalist.

My reply to this argument is to challenge its final step, that is, the inference that a physicalist should abandon physicalism just because physicalism is very likely false. The argument assumes that a physicalist is someone who must assign a high, or even very high, probability to the thesis of physicalism; wherefore it is unreasonable for a physicalist to define 'physical' in terms of current physics if doing so implies that physicalism is improbable. But I deny this assumption, claiming that a physicalist need *not* assign a high probability to physicalism, and can therefore comfortably live with the result that physicalism has a very low probability. There are different ways of developing such a reply to the argument, however, ways that vary in their positive account of what attitude to physicalism a physicalist should take. For instance, one could say that a physicalist is someone who merely treats the thesis of physicalism as some sort of regulative ideal for science, a role it could play while being literally false; and, indeed, Hellman seems to say something just like this in his own response to Hempel's dilemma (*op. cit.*, p. 610). Alternatively, one could say that a physicalist is someone who holds that physicalism, while literally false, is nevertheless closer to the truth, a better approximation to the truth, than its rivals. But both these suggestions have drawbacks. The first requires us to abandon the intuition that a physicalist is someone who takes some sort of attitude toward the *truth* of physicalism. The second can only be as good as the account of verisimilitude or approximation to the truth on which it relies, and these notions are notoriously hard to explicate satisfactorily. By contrast, the account I shall give here respects the intuition that a physicalist is someone who takes some sort of attitude toward the truth of physicalism; and it has no dependence on the concept of verisimilitude.

<sup>8</sup> Of course, the past history of physical theorizing is not the only evidence relevant to current physics' likelihood of being true, since there is also independent (that is, nonhistorical, observational) evidence for the claim that current physics is true to take into account; and when this other evidence is taken into account, it is far from obvious that current physics will emerge on balance as very unlikely (see Michael Levin, "On Theory-Change and Meaning-Change," *Philosophy of Science*, XLVI (1979): 407-24, especially pp. 420-21). But I waive Levin's point in what follows. One reason for doing so is that there seems to be no independent evidence for the claim that physics is *complete* which could outweigh the historical evidence for the claim that it is *incomplete*; so Levin's point could not be the whole of a response to the first horn of Hempel's dilemma.

My development of a different basis for the claim that a physicalist need not regard physicalism as more probable than not will involve giving an account, not of the thesis of physicalism, but of what it is to be a physicalist. Here is my argument in outline.

- (P1) To be a physicalist is to take the same attitude—whatever that attitude is—toward the hypothesis of physicalism which those who have broadly scientific realist and antirealist intuitions take toward what they regard as the best of current scientific hypotheses.
- (P2) The attitude that those who have broadly scientific realist and antirealist intuitions take toward what they regard as the best of current scientific hypotheses is identical with an attitude (to be defined later) that I shall call the *SR attitude*.
- (C1) Therefore, to be a physicalist is to take the SR attitude toward physicalism.
- (P3) But to take the SR attitude toward a hypothesis does not require regarding it as likely to be true (let alone very likely to be true).
- (C2) Therefore, to be a physicalist does not require regarding physicalism as likely to be true (let alone very likely to be true).

(P1) is very plausible, once physicalism is viewed as no more and no less than a scientific hypothesis, albeit one with unusual features. Surely, it can be so viewed. It does not, admittedly, assert the holding of any law, nor does it belong to any *one* science. But it does make a claim concerning the constitution/realization of the world which, like the claim that genes are made of DNA (though obviously on a larger scale), is entirely contingent and which is surely scientific rather than, say, commonsensical or religious. As I shall show at the end of my final section, physicalism can be exhibited as being explanatory in just the way we expect scientific hypotheses to be. At any rate, if physicalism is not a scientific hypothesis, then it resembles one in every respect relevant to current purposes. (P3) will turn out to be a triviality, once the nature of the SR attitude is made clear, as will shortly be done. (P2), however, which urges the identification of attitudes not obviously identical, is far from trivial and will require much elucidation and defense.

## II

The elucidation and defense of both (P2) and (P3) must begin with an explanation of what I mean by the 'SR attitude' toward a hypothesis, however. Here is a *stipulative* definition:

- (SR) To take the SR attitude toward a hypothesis is (1) to regard the hypothesis as true or false in virtue of the way the mind-independent world is, and (2) to assign the hypothesis a higher probability than that of its *relevant rivals*.

But what are the relevant rivals to a hypothesis? Here is another stipulative definition:

- (RR) Hypothesis H1 is a relevant rival to H2 if and only if (a) H1 is sensibly intended to achieve a significant number of H2's theoretical goals; (b) the hypotheses, H1 and H2, fail to supervene on one another; and (c) H1 has actually been formulated.

Clauses (a) through (c) require some unpacking and motivation. Take clause (a) first. The theoretical goals of a hypothesis will include such things as the satisfactory explanation of certain phenomena and the solution of certain problems. The reference to a "significant number" of such shared goals is supposed to do (rough) justice to the fact that advocates of rival hypotheses almost never completely agree what theoretical goals their respective hypotheses can reasonably be expected to achieve; insisting that *all* theoretical goals be shared for two hypotheses to count as rivals would therefore leave almost no pairs of hypotheses as rivals. The reference to goals that are "sensibly" intended for a hypothesis to achieve is supposed to ensure that the hypothesis that, say, the moon is made of green cheese does not qualify as a relevant rival to Darwinism just because some lunatic thinks it can account for the origin of species. Let us now turn to clause (b). It is included so that hypotheses at different levels of explanation (for example, folk psychology and scientific psychology), whose theoretical goals arguably overlap considerably, are not mistakenly classified as rivals; presumably folk psychology does supervene upon scientific psychology. It also serves to exclude hypotheses that are merely notational variants of one another from counting as relevant rivals. Finally, clause (c). I have no full account of what it is for a hypothesis to be formulated, but two points are crucial. First, to count as formulated, a hypothesis need not have been formulated in any great detail, but we must have been told something about its basic principles. So, for instance, I would count creationism as having been formulated, on the grounds that we are told *something* about the basic mechanism it hypothesizes to account for life, even though creationists are notoriously stingy with suggestions as to the details of God's plans for flora and fauna. Secondly, we must distinguish formulating a hypothesis from referring to it. The expression 'the set of laws accepted by people at Harvard who call themselves "physicists" in 2097' very probably refers to a hypothesis, but it does not formulate one; for it tells us nothing about the basic principles of the hypothesis (if any) referred to, which hypothesis therefore fails to count as a relevant rival to any current hypothesis.

One especially important consequence of (RR) is that the sheer negation of a hypothesis, unsupplemented by any other claims, does *not* count as a relevant rival to the hypothesis, since the unsupplemented negation of a hypothesis fails to meet condition (a): it cannot sensibly be intended to achieve the theoretical goals of the hypothesis. Simply denying the existence of electrons, for instance, goes no way toward accounting for the phenomena electrons are introduced to explain. Of course, the negation of a hypothesis can certainly be *part of*—one conjunct of—a relevant rival to the hypothesis; and I suspect that this is typically so when it appears that a hypothesis has a pure negation as a rival. So, for example, the denial of electrons' existence conjoined with appropriate phenomenological generalizations can perfectly well be a relevant rival to electron theory. Similarly, while (RR) implies that atheism unadorned is not a relevant rival to theism, it can allow that atheism plus the findings of contemporary science is a relevant rival to theism.

According to (RR), the relevant rivals to a hypothesis will be (1) certain predecessors in the history of the branch of science to which the hypothesis belongs, (2) certain current, actually formulated hypotheses in the branch of science to which the hypothesis belongs, and (3) certain current, actually formulated hypotheses—call them *crackpot hypotheses*—which in some sociological sense do not belong to the branch of science to which the hypothesis belongs (though they may once have done so). So, for instance, to take the SR attitude toward the hypothesis of evolution by gradualist natural selection is (a) to regard the hypothesis as true or false in virtue of the way the mind-independent world is, and (b) to regard it as likelier to be true than (at least) Lamarckianism, the punctuated equilibrium model, and creationism.

In the light of the stipulative definitions (SR) and (RR), it is now tolerably clear at least what (P2) and (P3) are claiming. But are they true? Take (P3) first. Since, according to clause (2) of (SR), taking the SR attitude toward a hypothesis requires only regarding it as more likely to be true than its relevant rivals,<sup>9</sup> and since the relevant rivals to a hypothesis do not include the sheer negation of that hypothesis, it is possible to take the SR attitude toward a hypothesis

<sup>9</sup> Notice that although regarding a hypothesis as more likely than its relevant rivals may not require being able to estimate *how* likely the hypotheses in question are, it is quite consistent with being able to do so; nothing here assumes or implies that "the testing of theories yields only a comparative warrant"—see Peter Lipton, "Is the Best Good Enough?" in David Papineau, ed., *The Philosophy of Science* (New York: Oxford, 1996), p. 93.

without regarding it as likely, still less very likely, to be true: a hypothesis might be unlikely, and yet still more likely than its relevant rivals.<sup>10</sup> What about (P2)? Is it true, as (P2) asserts, that the SR attitude, as stipulatively defined, can be identified with the attitude that those who have broadly scientific realist and antirealist intuitions take toward what they regard as the best of current scientific hypotheses? I shall now argue that it can.

What is the deepest intuitive commitment of those who would call themselves scientific realists and antirealists? It consists, I suggest, in the respectful way in which they regard (certain) current scientific hypotheses. By and large, they regard these current scientific hypotheses as:

- (1) true or false in virtue of the way the mind-independent world is
- (2) objectively superior, in some truth-connected sense, to earlier hypotheses in the field, so that science has, in this sense, progressed
- (3) objectively superior, in the same sense, to current rival scientific hypotheses
- (4) objectively superior, in the same sense again, to current rival hypotheses advocated by people outside the scientific establishment
- (5) such that whether the regard for a hypothesis embodied in (1)-(4) is appropriate is generally independent of whether or not the hypothesis postulates entities and properties that cannot be observed

Someone with these attitudes to the best of current scientific hypotheses will therefore find repugnant each of the following three ideas: (a) the (allegedly) neo-Kantian idea that the postulates of a scientific hypothesis are somehow conjured into existence by the widespread acceptance of the hypothesis; (b) the epistemologically egalitarian idea that all hypotheses, past or present, scientific or fringe, are more or less on a cognitive par with one another (even if their *political* influence is unequal); and (c) the empiricist idea that the distinction between observable and unobservable marks a distinction of great epistemological significance.

The argument for (P2)—that is, for identifying the stipulatively-defined SR attitude with the attitude that those who have broadly scientific realist and antirealist intuitions take toward what they regard as the best of current scientific hypotheses—has two premises. The first premise is that the attitude that those who have broadly scientific realist and antirealist intuitions take toward what they re-

<sup>10</sup> If the relevant rivals of a hypothesis did include the sheer negation of the hypothesis, then taking the SR attitude toward a hypothesis—regarding it as likelier than all its relevant rivals—would entail regarding it as likelier than its negation, and hence regarding it as likelier than not, and hence as likely.

gard as the best of current scientific hypotheses is adequately characterized by (1)-(5). (One who denies this premise has only to specify something that has been left out or wrongly included.) The second premise is that the SR attitude, as stipulatively defined, should be identified with the attitude characterized by (1)-(5). Five observations together provide evidence for this identification. First, clause (1) of (SR) says exactly what (1) above says. Second, to take the SR attitude toward a hypothesis is surely *one* way of taking a truth-connected attitude (as mentioned in (2)-(4)) toward it. It is not, of course, to have an all-or-nothing belief that the hypothesis is true; nor is it even to believe that the hypothesis is closer to the truth than—enjoys greater verisimilitude than—other false hypotheses. But it is, in part, to assign to the hypothesis a higher probability of being *true* than is assigned to its relevant rivals. Third, to take the SR attitude toward a hypothesis, and in particular to assign it a higher probability than any of its relevant rivals, is surely *one* way of regarding it as objectively superior (as mentioned in (2)-(4)) to certain rivals, at least on the assumption that assignments of probability are answerable to objective constraints, such as would be supplied by a reliabilist theory of confirmation (or by a Bayesian theory, appropriately supplemented by some account of objective constraints on assigning prior probabilities). Fourth, the relevant rivals that the SR attitude concerns include just the sort of rivals that (2)-(4) concern. Finally, nothing in the SR attitude rules it out that one could perfectly well take the SR attitude toward a hypothesis postulating unobservables.

(P2)'s identification of the SR attitude with the attitude that those with broadly scientific realist and antirealist sympathies take toward the best of current hypotheses evidently rests upon the idea that nothing compels a scientific realist/antirealist to assign a high probability to the theories she picks out as best. Yet it is hard, I admit, to dislodge the intuition that *something* requires the assignment of a high probability. But what? Not the need to reject the egalitarian suggestion that all theories, past or present, scientific or fringe, are really on a cognitive par with one another, since that suggestion can be rejected merely by adopting an attitude that, like the SR attitude, only assigns a higher, not a high, probability to favored hypotheses. Here is a better suggestion: "A scientific realist/antirealist must *believe* the theories she picks out as best, and since to believe a theory just is to assign it a high probability, the high probability requirement swiftly follows." Indeed it does, but neither premise is very plausible. To begin with, it is not at all clear why a scientific realist/antirealist must



*believe* the theories she picks out as best; certainly, belief is not required to explain practical reliance on the theories; for a sufficient basis for action can be the assignment of only a low probability, as when I carry a spare tire, even though I certainly do not believe that I will have a flat. Whereas it might plausibly be claimed that scientific realists/antirelativists must in *some* sense accept the theories they favor, argument is needed to show that this attitude of acceptance amounts to anything more than the SR attitude. More seriously, the identification of belief with the assignment of a high probability runs into the problem of the Lottery Paradox, in which, given the identification, an apparently quite rational person must be regarded as holding contradictory beliefs: a person assigns a high probability to each proposition saying, of one lottery ticket, that it will lose, and also to the proposition that some ticket will not lose; but if belief just is the assignment of high probability, then the person must *believe* of each ticket that it will lose, and also that some ticket will not lose—which commits her to a contradiction.<sup>11</sup> Nor is it true that the assignment of a high probability is even a logically necessary condition of belief: surely, it is logically possible for someone who believes ten (probabilistically independent) propositions, to each of which he assigns a probability of 0.9, to believe also the conjunction of those propositions, even though the probability of the conjunction is low, being the product of the probabilities of each conjunct.<sup>12</sup>

Here is a second suggestion for supporting a high-probability requirement: “Even if believing a theory does not logically require assigning it a high probability, *rationally* believing it surely does. So let us assume some analysis of belief which makes belief quite independent of probability assignments.<sup>13</sup> Then, since a scientific realist/antirealist must believe the theories she picks out as best, and since *rationally* believing a theory requires assigning it a high probability, a scientific realist/antirealist must on pain of irrationality assign a high probability to her favored theories.” But this second argument for a high-probability requirement is also inconclusive. It retains the nonobvious assumption of its predecessor that a scientific realist/antirealist must believe the theories she picks out as best. But the premise that rational belief requires assigning a high probability seems clearly wrong; for surely it is always rational to believe the im-

<sup>11</sup> For elaboration and defense, see Mark Kaplan, *Decision Theory as Philosophy* (New York: Cambridge, 1996), pp. 93-98; see also Patrick Maher, *Betting on Theories* (New York: Cambridge, 1993), pp. 134-35.

<sup>12</sup> For the argument as applied to acceptance, see Maher, pp. 137-39 and 152-55.

<sup>13</sup> For such analyses of a belief-like notion of acceptance, see, for example, Kaplan, ch. 4, and Maher, ch. 6.

mediate logical consequences of what one rationally believes. But then the person who rationally believes ten (probabilistically independent) propositions, to each of which he assigns a probability of 0.9, is rational in believing their conjunction, even though, as already noted, this conjunction has a low probability.<sup>14</sup> Let me also note that there are theories of rational acceptance which leave open the possibility of accepting hypotheses that are improbable, and they could presumably be modified to cover rational belief, non-probabilistically-construed.<sup>15</sup> Neither Mark Kaplan's nor Patrick Maher's theory is at all reliabilist in spirit, but a reliabilist theory which also yields the same result is easy to imagine in vague outline: if one's cognitive goal is not just truths, but truths that, say, provide a basis for prediction and explanation, then a belief-forming method could be rational to adopt, and its products count as rational, even though unlikely to be true, just so long as they made up for their improbability, as it were, by constituting a superior basis for prediction and explanation. Unformulated theories, or the sheer negations of formulated theories, might be likelier to be true, but cannot provide such a basis.<sup>16</sup>

In the absence of any further reason for insisting on a high-probability requirement, (P2)'s identification of the SR attitude with the attitude that those who have broadly scientific realist and antirealist intuitions take toward what they regard as the best of current scientific hypotheses can stand.<sup>17</sup> It may be possible to go on the offensive here,

<sup>14</sup> Again, see Maher, pp. 137-39 and 152-55.

<sup>15</sup> See Kaplan, ch. 4, and Maher, ch. 6.3. This is a good point at which to respond to the complaint that my answer to the Hempelian dilemma offers no solace to the physicalist who *believes* physicalism (rather than just taking the SR attitude toward it). In one way, of course, the complaint is misconceived: the thrust of the entire paper is that a physicalist does not have to *believe* physicalism. A more substantial reaction is to note that, even if it is insisted that a physicalist is one who believes physicalism, Hempel's dilemma can be avoided so long as belief in physicalism can be rational despite physicalism's improbability, which would be true if some general thesis held that belief in an improbable hypothesis can still be rational.

<sup>16</sup> Perhaps scientific realist/antirealist intuitions include the idea that the best of current scientific hypotheses are things we *know*. Perhaps; but it is far from clear that one knows that *p* only if it is likelier to be true than false that *p*. These are accounts of knowledge (for example, reliabilist ones) that do not imply that this is so, and a staunch defender of scientific knowledge might favor such an account—indeed, might be expected to do so.

<sup>17</sup> A different kind of objection to (P2) claims that, if a hypothesis were very, very bad but still better than its relevant rivals, one could take the SR attitude toward it without also favoring it in the manner distinctive of a scientific realist/antirealist. But, naturally, I doubt that this could happen, and I know of no example. I conjecture that any putative example would either tacitly assume that favoring a hypothesis in the distinctive scientific realist/antirealist manner requires believing it (which I have denied), or would be a case in which many relevant rivals no less likelier could easily be formulated (even though ordinarily no one would bother to).

however. Resisting a high-probability requirement offers scientific realists/antirelativists an attractive reply to the so-called pessimistic induction which claims that, since most past theories have turned out to be false, current theories are probably false, too.<sup>18</sup> Moreover, it is arguable that insisting on a high-probability requirement would be unreasonable, so that the SR attitude toward hypotheses (or something like it) is all we can decently hope for. Suppose a scientific realist/antirealist insists that we should regard our best current theories as more likely to be true than false. That implies regarding them as more likely to be true than the disjunction of their rivals, and hence more likely than each disjunct taken individually. But some of those disjuncts are unborn hypotheses, that is, hypotheses that have not yet been formulated and perhaps never will be.<sup>19</sup> How could our current evidence make it reasonable to regard a current hypothesis as likelier than an unformulated hypothesis? So we may be going beyond any attitude it could possibly be reasonable to take if we claim that a current hypothesis is likelier to be true than not.

### III

My response to Hempel's dilemma is now complete. Let physicalism be formulated in terms of current physics. Then, given that a physicalist is simply someone who takes the SR attitude toward physicalism, the mere fact that the history of physical theorizing makes physicalism unlikely to be true provides no reason by itself to abandon being a physicalist; one can remain a physicalist, just so long as physicalism, though unlikely, is still more likely than its relevant rivals. This is not simply special pleading on behalf of physicalism; for the SR attitude, as I have argued, is none other than the attitude scientific realists/antirelativists take toward their favored hypotheses in particular branches of science.

Of course, the history of physical theorizing still constitutes evidence against physicalism, in the sense of lowering the likelihood that it is true, and presumably it does not in the same way constitute evidence against physicalism's relevant rivals. So it still threatens physicalism. But any evidence against physicalism must obviously be weighed against evidence for it, and the balance of probabilities may yet leave physicalism likelier than its relevant rivals. Naturally, physicalists insist that there *is* strong evidence for physicalism, evidence capable in principle of giving the probability of physicalism the nec-

<sup>18</sup> As noted earlier, I discovered, in Maher, p. 137.

<sup>19</sup> See Lawrence Sklar, "Do Unborn Hypotheses Have Rights?" *Pacific Philosophical Quarterly*, LXII (1979): 17-29.

essary boost. Such counteracting evidence, moreover, does not have to raise physicalism's probability to 0.5 or higher; given my proposed account of what it is to be a physicalist, it need only lift it above that of its closest relevant rival, an easier requirement to fulfill.

In fact, and contrary to the presumption of the last paragraph, the history of physical theorizing may be as damaging to physicalism's relevant rivals as it is to physicalism. Relevant rivals to physicalism are actually formulated hypotheses that are sensibly intended to address the problem that it is the central theoretical goal of physicalism to address: giving an account of the relations among the ontologies that the many sciences (including folk psychology and folk physics) respectively postulate, in light of such cross-scientific regularities as have been discovered empirically.<sup>20</sup> An example of one sort of crude cross-scientific regularity might be that nothing is ever in a mental state unless it is in some simultaneous brain state; but the sciences (for example, the neurosciences) present more refined regularities. To identify physicalism's relevant rivals, it is helpful to view physicalism as the conjunctive thesis that:

- (1) There is some science, *S*, distinct from the totality of all the sciences, such that every entity (property) is either itself mentioned as such in the laws and theories of *S* or is ultimately constituted (realized) by entities (properties) mentioned as such in the laws and theories of *S*.

and that:

- (2) *S* is current physics.<sup>21</sup>

<sup>20</sup> See my *op. cit.*, pp. 222-24.

<sup>21</sup> Understanding physicalism in this way, as the conjunction of (1) and (2), has the possibly unsettling consequence that, say, Thomas Hobbes was not a physicalist, since he had no notion of current physics. But, surely, Hobbesian materialism nevertheless has much in common with physicalism. The most important commonality is the belief that some science distinct from the bare conjunction of the many sciences is basic in some metaphysical sense. But there is also the fact that Hobbes picked as the basic science something that turned out to be, sociologically and historically speaking, the ancestor of current physics. Similarly, if tomorrow new evidence forces a revision of physics, it will be possible to formulate another relevant rival to physicalism in category (A), by letting tomorrow's (superior) physics take the place of current physics in (2); and if the evidence for taking tomorrow's physics to be basic is as good as the evidence for taking current physics to be basic, which is highly likely, the resulting doctrine will be superior. But it will not, strictly speaking, be the doctrine I have called physicalism, though it will, however, have much in common with it. Specifically, it will implement the generic idea that there is a basic science distinct from the bare conjunction of the many sciences, and it will do so by appeal to a science that is an immediate descendent of current physics. So if we are looking for something to serve as "the spirit of physicalism" which transcends particular formulations, then commitment to (1) might be the heart of a good candidate, though the spirit of physicalism would then be cognitive and not attitudinal (see van Fraassen, pp. 169-70).

Relevant rivals to physicalism therefore fall into two categories: (A) those which endorse (1) without endorsing (2), by agreeing that there is a basic science to which all the other sciences stand in some special relation, while proposing that this basic science is something other than physics; and (B) those which deny (1), maintaining that no science is basic, since all sciences are on an ontological par, linked to one another merely by various fundamental laws.<sup>22</sup> Do relevant rivals of either sort gain any advantage over physicalism from evidence constituted solely by the history of physical theorizing?

Take rivals in category (A). Suppose one such rival asserts that there is a basic science, but that it is, say, biology. Such a view is evidentially quite untouched by the history of physical theorizing. On the other hand, the track record of biological theorizing is arguably no better than that of physical theorizing, which evens things out, and obviously there is other evidence decisive against taking biology to be the basic science. But the best-known relevant rival in category (A) is traditional dualism, which I interpret as the view that, to put it very crudely, physicalism is true of everything except the mind: there is a basic science, but it is the *conjunction* of physics and folk psychology (presumably linked to one another by fundamental psychophysical laws). The impact of the history of physical theorizing on traditional dualism, relative to that on physicalism, is trickier to assess. To the extent that the history of physical theorizing makes it likely that current physics is *false*, there is exactly the same evidence against traditional dualism as there is against physicalism, given my interpretation of traditional dualism as the view that the conjunction of physics and folk psychology constitutes the basic science. It might appear, though, that historical evidence that current physics is *incomplete* would leave traditional dualism unharmed, since traditional dualism, unlike physicalism, is not committed to regarding current physics as complete. But, in fact, what the history of physical theorizing makes likely is that current physics has left out something like a new kind of particle, with mass and charge, and traditional dualism *is* committed to regarding current physics as complete in respect of that sort of thing.

What about relevant rivals in category (B)? The egalitarian and pluralist view that there is no basic science, the view, in effect, that cross-scientific regularities should be treated as fundamental laws, appears initially to be committed neither to the truth nor to the completeness of current physics; so on evidence constituted solely by

<sup>22</sup> See Crane and Mellor.

the history of physical theorizing, physicalism appears less likely than antiphysicalist pluralism. Actually, though, on pain of simply saying nothing at all about interscientific relations, antiphysicalist pluralism is committed to law statements connecting events as characterized by current physics to events as characterized by each of the special sciences; and if current physics is probably false, then so, surely, are those law statements. Surprisingly, then, antiphysicalist pluralism may derive no advantage over physicalism from evidence constituted solely by the history of physical theorizing.

Finally, let me give the briefest outline—no more than a hint—of the evidence *for* physicalism that I mentioned a few paragraphs back. The evidence for physicalism, as I understand it, consists in empirically discovered regularities of various sorts, the best explanation of which is physicalism. One important sort of regularity is that which links physical conditions with simultaneous nonphysical (for example, mental, biological, geological) conditions; so, for instance, there are physical conditions of a person's brain and environment which are regularly accompanied by a certain simultaneous mental condition.<sup>23</sup> Now, physicalism can explain a regularity of this kind by hypothesizing that the mental condition is identical with a functional condition, and that this functional condition is physically realized, and physically realized, in particular, by the physical condition of the person's brain and environment empirically found to be sufficient for it. On this hypothesis, we would expect to observe that whenever the physical condition obtains, so, too, does the mental condition: the physical condition suffices for the playing of a certain functional role, and that something plays that role suffices for the obtaining of the mental condition, so that the physical condition suffices for the mental condition. (Evidently, the physical condition must be *capable* of playing the relevant role, and whether it is must be determined empirically, or else the explanation must be rejected.) But if physicalism is not just an explanation but also the best explanation of such a regularity, then its power to explain the regularity is some evidence for it.<sup>24</sup>

<sup>23</sup> There are other empirically-discovered regularities that can arguably be explained by physicalism but not otherwise; for instance, I believe the argument from overdetermination for physicalism (see, for example, my *op. cit.*, pp. 226-31) points us toward some. Yet other regularities potentially providing evidence for physicalism are the laws of the special sciences; see my "Two Cheers for Reductionism; Or, The Dim Prospects For Non-Reductive Materialism," *Philosophy Of Science*, LXII (1995): 370-88.

<sup>24</sup> For elaboration, see my "Being a Physicalist: How and (More Importantly) Why," pp. 231-35.

Further light can be shed on how regularities of this kind provide evidence for physicalism by considering the egalitarian and pluralist rival to physicalism which maintains that there is no basic science. Since this rival asserts the holding of, and hence (trivially) entails, the very regularities that I claim provide evidence for physicalism, it might be objected that these regularities therefore fail to provide evidence that favors physicalism over its rival; the evidence is neutral between physicalism and its rival. But there are obvious replies to this objection. The first is that the rival is still inferior to physicalism in respect of *explanatory power*. Although the egalitarian rival indeed entails the regularities that physicalism can explain, it entails them quite trivially, by explicitly asserting that they hold; so it does not *explain* them at all, but instead treats them as brute, fundamental laws, on a par with the fundamental laws of physics. A second reply is that, both because the egalitarian rival treats these regularities as brute laws to be accepted *in addition to* the fundamental laws of physics, and because it postulates so many properties neither identical with nor even realized by physical properties, it is less *economical* than physicalism. Certainly, antiphysicalists are aware of these replies, but they tend to disparage them. So here is a reason to take them seriously. On my account of the evidence for physicalism, the empirical case for physicalism is strongly analogous to that for any physical theory aiming to give an account of what underlies some (relatively) observable regularity (for example, a physical theory aiming to explain why water boils at 100° C). Now, a physical theory will always have an empirically equivalent rival, namely, the view that the observable regularities it aims to explain should be treated as brute laws that simply have no explanation. But many of us suppose that the theory is nevertheless to be preferred, on the grounds of some superempirical virtue it enjoys, like explanatory power or economy; and whereas the philosophical understanding and justification of appeals to such virtues is obviously contested, that we make such appeals, and take ourselves to be reasonable in doing so, seems hard to deny.<sup>25</sup> But if these appeals are legitimate when used to break the tie between a physical theory and its empirically equivalent phenomenological rival, then why not also in the case of physicalism and its pluralist and

<sup>25</sup> I suspect that hostility to taking the superempirical virtues of a theory as counting in favor of its truth stems from the unstated assumption that methodological principles must be a priori in character; for it must be conceded that it is no necessary truth that the world exhibits the sort of economy and connectedness of structure that would make the explanatory power and economy of a theory count in favor of its truth. But from the perspective of an a posteriori, reliabilist epistemology, surely the matter looks quite different.

egalitarian rival? For the case is simply another instance of a theory and its empirically equivalent rival.<sup>26</sup>

At first sight, Hempel's dilemma seems to guarantee a swift and decisive victory to the enemies of physicalism. But closer examination reveals that it is merely one offensive in a campaign whose outcome is at best undecided.

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<sup>26</sup> See Smart, "Sensations and Brain Processes," *Philosophical Review*, LXVIII (1959): 141-56, especially pp. 155-56.