11 Scientific Results and the Mind–Brain Issue: Some Afterthoughts

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Rereading the transcript of the preceding "Conversation among Philosophers and Scientists" left me feeling unsatisfied or, rather, unfulfilled. It was not so much the old familiar feeling that, I suppose, most of us in the academic game have had more than once—the feeling that, "When he said *so and so*, I should have said *such and such*. That would have been a good lick!" On the contrary, it left me with the impression that all of us, including even me, did pretty well—pretty well, that is, given the material that we had and had had before us at the conference. My regret was that, in view of the interest in the matter and its crucial importance, we had not had more formal presentations on the relevance of scientific knowledge for the mind–brain issue.

This is one excuse for inserting *this* paper on the subject here. Another is that the "Conversation . . . " was triggered by some discussion remarks of mine in response to a paper by Karl Pribram (this volume). The present essay *is* intended as a general one, and I have made every effort not to reply either directly or by implication to any of the points "made against" me, indeed, not to "make points" against

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any of the remarks contained in the "Conversation . . . " Whether or not I have succeeded in this resolve is, no doubt, difficult to decide, for the "Conversation . . . " certainly has been stimulating and suggestive and has played a large role in generating the material for this paper.

Since there are some readers who are not (professional) philosophers, I have tried to keep jargon at a minimum and have explained at some length matters which the philosophers will find unnecessarily elementary. I beg the indulgence of the latter group for these two breaches of professional mores.

The concern of this essay *is* the mind–body problem, and, if it were possible, I would stick to *it* and pass over general issues about the relationship between science and philosophy. But the view that scientific results can have little or no relevance for philosophical problems is currently so popular and so firmly held that it must be dealt with if one wishes to argue that science is not only relevant but indeed that it provides the means for untying the world knot (the mind–body puzzle). I *have* dealt with it in detail elsewhere (Maxwell 1970*a*, 1972, and, esp., 1975, 1976), so I shall be brief and thus, I fear, somewhat dogmatic here.

Before making any critical remarks, I should like to give a short statement of what I take the nature, the task, and the method of philosophy to be. The concern of philosophy is with basic, fundamental, foundational principles (assumptions, beliefs, etc.). Such principles may sometimes be explicit, but often they are implicit, tacit, and, perhaps, unconsciously held. (Some of them may be missing altogether. This might be the case in an area about which we would say that its "foundations" are incomplete or unsatisfactory—for example, confirmation theory today [or, as some would call it, "inductive logic"], or, in physics, quantum theory.) The belief in the existence of the "external" (mind-independent) world—the belief that, say, my desk continues to exist when no one is aware of it—may be said, not too inaccurately, to be an example of an assumption or a belief that is *tacit* and, perhaps, *unconscious* (until, or course, one becomes initiated into philosophical circles). (Some would have it that such philosophical musings are misguided, oversophisticated [or sophistical], contrived, unnatural, pathological, and otherwise naughty. I disagree. I have encountered more than one happy, healthy, well-adjusted four- or five-year-old who invented, quite independently, and struggled with puzzles very similar to Kant's antinomies on the finitude of space and time. This, of course, doesn't *prove* my point, but, *prima facie*, it seems to favor it.)

How do such foundational principles (and/or beliefs, etc.)

differ from less exalted ones, ones for which we might try to collect (observational) evidence, perhaps designing and performing experiments, so that they might be *confirmed* or *disconfirmed*? The answer, I am firmly convinced, is that they differ only in degree and not in kind. And sometimes the difference in degree is quite small: there is a continuum (with no singularities) from the most basic foundational principle to the most lowly "empirical" generalization. In other words, *philosophy* and *science* differ from each other only in degree and not in kind. (By "philosophy" here, I am referring to epistemology and metaphysics, in general, and philosophy of science, in particular. I wish to leave entirely aside the question as to whether very similar or very different considerations apply to the realm of *values*.)

Before giving arguments for this contention, it should be useful to consider some of the reasons that it sounds so outrageous to many contemporary ears. (For more details than can be included here, see Maxwell, *ops. cit.*) The central objection to my position might run as follows: "*Scientific* statements, even the most highly theoretical ones, must be such that they can, in principle, be confirmed or disconfirmed by experimental, observational evidence. It must at least be the case for a *scientific* proposition that there *could* exist evidence that would count either in favor of it or against it. *Scientific* disagreements can, in principle, always be settled by a proper assessment of an appropriate kind and amount of evidence. (What holds for scientific knowledge also holds for virtually all of our [legitimate] common sense knowledge.) Philosophical propositions," the objection continues, "are radically different. They can not only be 'saved' *from* and made consistent *with* any conceivable evidence; many of them are such that their proponents, by means of ingenious and convoluted machinations, can make them *explain* or *account for* any conceivable evidence. This should generate healthy suspicion and distrust of (traditional) philosophical propositions and should, as it indeed does, lead us to an agonizing reappraisal of the entire philosophical enterprise. As a result, we see that the only function of a *legitimate* philosophical statement is to *convey*[1] information about the *language, the concepts,* and the *logic* that we use to express our knowledge, beliefs, etc., about the world and ourselves. (Legitimate) philosophical activity can consist only of the analysis of language, conceptual analysis, or logical analysis. For, in what other area could philosophy stake a defensible claim? We have

[1] According to some, philosophical statements can only *show* or otherwise "express" information, they cannot (explicitly) *assert* it It is now recognized by most, however, that statements made in an appropriate metalanguage can explicitly *assert* the (linguistic, conceptual, or logical) information that is "conveyed "

seen above," the argument proceeds, "that statements and issues of a factual—a contingent, an *empirical*—nature are in the realm of science (or, sometimes, of good old everyday common sense) and are to be settled by collecting and weighing evidence and that philosophical issues are not amenable to this kind of settlement. Therefore, philosophical statements, views, positions, theories, etc., must be void of any factual, contingent, 'empirical' content and, at best, can only be about linguistic, conceptual, or logical matters. Q. E. D. "

As congenial as this view about the nature of philosophy (and of science) may be to contemporary philosophers (and, no doubt, to many scientists) and as much as I used to extol it myself, I believe that it is grossly mistaken and that the argument, sketched above, in its favor is drastically unsound. However, I do agree emphatically with one of its apparently damning premises; it is true that almost any philosophical theory can be "saved" in the face of any conceivable evidence and can, indeed, be made to explain or account for any evidence whatever. But, unfortunately for the argument in question, *the same is true for any scientific theory* that is of any appreciable degree of interest and importance and which goes beyond the lowest "empirical generalization." So that, if the premise in question is the ground for the intermediate conclusion that philosophical propositions are devoid of factual, contingent, or "empirical" content, it follows that every scientific proposition that is of much interest or importance is possessed of the same kind of distressing vacuity. What is wrong here? The obvious but not very helpful answer is that neither (intermediate) conclusion follows from the premise and that some propositions that are such that they can be "saved" in the face of all conceivable evidence, etc., etc., can nevertheless have factual, contingent content. (Omission of the word "empirical" from the preceding sentence, which, in a way, is the key to the whole matter, as well as my using it in "shudder quotes" in this essay, will be discussed presently.)

The discomforting facts about the rather extreme tenuousness and deviousness of any connections that lead from evidence to scientific theories have been revealed clearly and forcefully by recent studies in confirmation theory—by uniformly unsuccessful attempts to "justify induction" and/or to develop an "inductive logic," by attempts such as Popper's, again unsuccessful, to "save empiricism" by utilizing *falsifications* (actual *or* possible) of theories by observational data in order to circumvent the "problem of induction," by Russell's devastating negative results and, at best, indifferently successful constructive efforts to deal with problems of confirmation, and by many others. The reasons for such failures were first elaborated, as far as I have been able to tell, partly by Russell and partly by Duhem, although Hume had long before provided the central theme with impeccable clarity and force. Russell emphasized the (deductively demonstrable!) fact that commonly employed "inductive inferences" or "inductive arguments" yield, in principle, false conclusions from true premises infinitely more often than they yield true ones. (Included under "commonly employed 'inductive inferences' " are induction by simple enumeration, Mill's Canons, and most kinds of statistical inferences that are presented in textbooks.) For, it is easy to demonstrate that, for every such inductive argument with true premises and a true conclusion, there exists an infinite number of arguments, each of which has the *same form* as the first, has premises that are all true, and has a *false* conclusion. (The conclusions of these arguments are also mutually incompatible in addition to being incompatible with the true conclusion.) As Russell [1948] remarks, this shows that induction leads infinitely more often to incorrect, unacceptable results than it does to correct ones *unless* it is bolstered by (extremely strong) contingent but unconfirmed[2] assumptions. As we shall soon see, an entirely analogous result can be demonstrated for the only other kind of procedure that seems to be available for confirmation, or for nondeductive inference, or for relating evidence to theories (the *hypothetico-deductive* or, better, *hypothetico-inferential* method). These considerations show, quite conclusively, I think, that all attempts to justify or vindicate induction, any kind of nondeductive inference, or any kind of confirmation methods in a manner acceptable to empiricism—and, thus, to the vast majority of contemporary philosophers—that all such attempts are bound to fail (for reasons, ironically enough, that can be established by "logical analysis" alone). It follows that, except for statements that report direct observations of the moment, *there are no empirical statements* in the sense of "empirical" that is used in contemporary circles. There are no statements of much scope, interest, or importance that are decidable or, even, confirmable or disconfirmable on the basis of only the data plus logic (including "inductive logic," if there were such a thing).

Popper's valiant attempts to "save empiricism"[3] in the face of all of this, most of which he would grant and, indeed, enthusiastically endorse, must now be considered briefly. His views are so well known that I shall omit any exposition of them and proceed directly to criticism. Unfortunately for Popper, statements that go any way at all

[2] Not confirmed by any means acceptable to empiricists (or to rationalists, for that matter) and, thus, not confirmed by means acceptable to most contemporary philosophers For discussion of the nature of such assumptions, see Maxwell (1975, and in press)

[3] He puts the matter in this manner in Popper (1962)

beyond the lowest level empirical generalization—statements that are of any appreciable scope, interest, or importance—are no more falsifiable by observational evidence than they are verifiable (or confirmable).[4] The reasons are, mostly, the familiar Duhemian ones: such a theory alone will not, as a rule, yield any evidence statement (any observation statement or its denial) as a *deductive consequence* and, thus, in isolation, it cannot be falsified by any conceivable observational evidence. In practice, that which *does* deductively imply observation statements is a conjunction of the theory of interest with other, *"auxiliary" theories* ("background knowledge") together with (singular) statements of initial conditions *a large portion of which are usually about unobservables.* Thus, if such a conjuction entails a certain observation statement and, after we have done our best to see that all of the initial conditions are fulfilled, the entailed observation statement turns out to be false (the predicted result does not occur), what has been *falsified,* of course, is the conjunction; the culprit may be one or more of the auxiliary theories and/or one or more of the assumptions about unobservable initial conditions, and the theory being tested (the "theory of interest") may, for all we know, be true.[5]

[4] Popper (1959) recognized this, but goes on to say that it's all right, because scientists ought to use certain "methodological rules" which *do* "make" theories falsifiable Crudely, but not, I think, unfairly, Popper's "rules" may be summarized· Assume that all of the operative auxiliary theories and other "background knowledge" are true so that if the predicted observation does not occur, it must be the theory of interest that is false I have given detailed arguments elsewhere (Maxwell, 1974, 1975) that use of such rules, unless it is completely arbitrary and *ad hoc,* rests on strong presuppositions that are unconfirmed (and "uncorroborated") and that, therefore, stand just as much in need of justification or vindication—if they are to "save empiricism"—as do induction or other confirmation procedures. Moreover, I have argued that some of the presuppositions are false and that the rules *neither are nor ought to be* taken too seriously by practicing scientists To cite just one argument, quite often the auxiliary theories that are used to relate the theory of interest to the evidence are less well confirmed (or "corroborated") than the theory being tested A striking example from recent history of science is the "detection" of the neutrino (see Maxwell, 1974); so that if the results *had* been negative, no one would have thought that the "neutrino hypothesis" had been falsified.

[5] Popper (1974) contends that such conjunctions do not need to contain statements about initial conditions in order to be inconsistent with (and, thus, falsifiable by) the special kind of observation statements that he calls "basic statements " Presumably, this is because his basic statements contain assertions to the effect that the initial conditions do hold *and* that the predicted outcome does not transpire. But his contention can stand only if all the initial conditions, in a given case, are observable Consider a case where the initial conditions required, say, that a certain system be at thermodynamic equilibrium or that a given planet was not being acted upon by any unobserved bodies or by other undetected forces The potential falsifiers would be (something like), "When a system [such as this] is at thermodynamic equilibrium and when [the other initial conditions] are fulfilled, then, [nevertheless], such-and-such a result does not transpire" (and an analogous one for the astronomical case) But these potential falsifiers are *not,* of course, observation statements, and Popper (1959) clearly specifies that his *basic statements are* observation statements. It is true that he was not very happy about this, even at the time, and said that he could have just as well said that they were statements about macrophysical objects. But, of course, the latter condition does not hold, in general, for unobservables He may want to change his mind about the observability–macro requirement and allow some basic statements to be (partially) about unobservables. But this surely would wreck his program to "save empiricism," in particular, and his efforts to provide any kind of deductive link (indeed, any viable link at all) between observable evidence and scientific theories And, it would, I should think, transform his conventionalism about the acceptance of observation

I have answered Popper's replies to these kinds of objections in the footnote just designated, in the one preceding it, and, at more length, in Maxwell (1974, 1975). I have used footnotes for this, not because of any underestimation of its importance—on the contrary—but in an effort to preserve unity and simplicity of presentation. For its purpose, I shall assume that the Duhemian position, as I have outlined it here, is correct and urge the reader to study the footnotes at a convenient time. I have discussed the elementary and rather well

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statements (already too bitter a pill for most empiricists to swallow) into a full-fledged conventionalist view of science

I grant that these *particular* unwelcome results can always be circumvented by patching up theories by adding to them postulates of a certain kind (They are a kind of "correspondence postulates," in Carnap's (1963) terminology) Such postulates could quite conveniently be considered to be among the auxiliary theories and, thus, subject to the kind of "methodological rules" discussed in the preceding footnote For example, in the thermodynamics case, we could add a postulate to the effect that all systems that have been isolated as carefully as possible for such-and-such period of time and in which measurements of temperature, pressure, etc , at various places in the system and at various times agree with each other and remain constant, and etc , etc —that all such systems are at equilibrium *This* particular postulate is not too bad, although we do know from contemporary thermodynamic theory that, at best, it only holds statistically (I shall let pass, here, Popper's provisions for "making" statistical theories "falsifiable," although I think that they are as ad hoc and unsatisfactory as his other *methodological rules*) For the astronomical example, the postulate added would be something to the effect that, whenever we have pointed our telescopes, etc in all directions and have made all other plausible efforts to detect extraneous bodies and forces, no such bodies or forces are present This would be a very questionable postulate or auxiliary theory (quite apart from the fact that it is, at best, a statistical one—for, almost certainly, we would sometimes fail to detect extraneous forces that are nevertheless present, no matter how assiduously we looked for them), and a methodological rule that abjures us to accept it as true for the purpose of "making" the theory of interest falsifiable would be a bad rule For example, its adoption, at a certain stage of inquiry, would have resulted in the "falsification" of celestial mechanics and have prevented, at least for a time, the discovery of the planet Uranus Popper could quite correctly reply that, on the contrary, it was careful attention to this "postulate" or "auxiliary theory" that *resulted*, eventually, in the discovery of Uranus However, this shows, it seems to me, that there is only a verbal—or rather a formal—nonsubstantive difference between dealing with, e g , extraneous forces as unobservable (perhaps in some cases, as in this one, merely unobserved-at-the-time) initial conditions, on the one hand, and incorporating them into postulates or auxiliary theories, on the other The substantive point is that, either way, such factors amount to one more obstacle in the way of falsifying theories of interest and provide one more reason for rejecting Popper's methodological rules Recall that the "rules" direct us to turn our fury towards the theory of interest so that it "becomes" falsifiable and, thus, to be gentle with the auxiliary theories rather than subjecting them to the same kind of severe scrutiny Such a policy *might well* have overlooked Uranus and "falsified" celestial mechanics (Or, if unobservables *are* accepted into initial conditions and, thus, into *basic statements*, why not accept, *by convention*, the basic statement to the effect that [the other] initial conditions obtain *and* there are no extraneous bodies or forces present, and the orbit of Neptune does not coincide with the one predicted by our theory of celestial mechanics? In which case, the same unhappy chapter in the history of science would have had to be written.)

At this point, some defenders of Popper might be tempted to abandon his "methodological rules" but to claim that, even so, the conjunction consisting of the theory of interest and the auxiliary theories, etc (including those that incorporate what I prefer to consider unobservable initial conditions)—that this (rather enormous) conjunction *is* falsifiable by observational evidence This is true, *provided that* neither the theory of interest nor any of the auxiliary theories are statistical ones, a condition which, I believe, will almost never hold, since those "auxiliary theories" that incorporate initial conditions will almost always be statistical ones, but let this pass, for the moment, and assume that such conjunctions *are* falsifiable Such a "defense," however, unless something else is said (I don't know what), renders the Popperian position indistinguishable from that of the Duhemians and brings us back to exactly the same point in the text at which we had arrived when we took time out for this footnote known details of the logical structure of the relations among theories of interest, auxiliary theories, initial conditions, and (observational) evidence at some length here because they, together with a point to be emphasized soon, not only undermine Popper's program but also take us to the heart of the confirmation predicament.

We have already noted not only the Duhemian point, that a theory of interest can be saved in the face of any data—any evidence—whatever by means of making appropriate changes and exchanges among the auxiliary theories (and the other necessary conjuncts), but also that by using similar manipulations, the theory can actually be made to *account for* (to *explain*, to yield [together with the other conjuncts] as a deductive [or, sometimes, as a statistical] consequence) any such evidence. This is always possible, moreover, when it is required, as of course it *should* be, that the theory of interest function nonvacuously in accounting for (explaining, entailing or implying statistically) the evidence. It is also easy to show (again, indeed, to demonstrate [deductively]) that, *given any conceivable evidence in any amount whatever, there will always be an infinite number of mutually incompatible theories each of which will account for (explain,* [*deductively*] *entail* or, if statistical theories are appropriate, *statistically imply) the evidence*. And this result holds not only for "theories of interest" but also when we designate as *theories* the entire, enormous conjuncts that are required to account for (entail, etc.) the evidence. This delivers, I believe, the *coup de grace* to Popper's program, already mortally wounded by the Duhemians. For, even if falsification *were* possible, it would get us nowhere. No matter how many theories we falsified, there would always, in principle, remain an infinite number of theories that entail (would account for, etc.) the evidence at hand, only one of which is true.

However, Popper's program is no worse off (and no better off) than any of the other confirmationist (or "corroborationist") procedures that most philosophers (and, I suppose, most scientists who are at all self-conscious about their methodology) accept as articles of faith today. What we must accept is the abject impotence—total, chronic, and permanent—of evidence so long as it is paired exclusively with logic (even with Inductive Logic—whoever she may be). And since Empiricism refuses to recognize as legitimate anything other than knowledge that issues from the union of these two, she must join Hume, perhaps the first and the last consistent empiricist, and embrace total skepticism. The so-called "paradoxes of confirmation" and other popularly discussed difficulties fade into insignificance beside—indeed, they are just special cases of—these general and elementary logical considerations that, we have seen, seal the doom of empiricism.

If anyone has remained with me this far, he may very well be losing what patience he has left. For example, I can just imagine someone (in particular, I imagine Sir John Eccles!) bristling and coming up with a rejoinder such as, "Come, come now, Maxwell! I am neither intimidated by nor very much impressed with your logico-philosophical bag of tricks. As a practicing scientist I am not interested in your infinitude of logically possible, mutually incompatible competing theories. Surely most of them are so silly, contrived, implausible, and convoluted that they aren't worth considering. Anyway, in any *one* real scientific context, no one is going to think up or, at any rate, propose seriously more than one or two of them. And let us suppose, even, that someone does. We could always use our good old scientific horse sense to decide which of them are too silly to be considered. Moreover, we can use the same kind of horse sense to see when the evidence counts heavily enough against a theory for us to consider it falsified, even though it could be 'saved' by substituting complicated, far-fetched, silly, or *ad hoc* auxiliary theories, etc." Bravo! Something sort of like this *is* what happens in scientific *and* in common sense contexts. But before proceeding, let us pause to note that using "good scientific horse sense" is using something in addition to evidence plus logic and that *silliness* (or the absence thereof), *plausibility*, far-fetchedness, etc.,[6] are not logical properties. This calls attention to the central principle of epistemology, which is that if knowledge (or, even, a significant amount of true belief) is possible and if any reliable assessment or confirmation of our knowledge claims is possible, humans must possess two remarkable (extralogical) kinds of abilities.[7] They must be able to make guesses, have hunches, in other words, to *propose theories* that have a much greater chance of being true (or close to true)

[6] It might be thought that *ad hocness* and perhaps *simplicity* (low degree of complication or convolution) can be characterized by purely logical terms and, thus, can be used as a basis for eliminating all but one member of a family of competing theories, providing a way out of the confirmation muddle, rescuing empiricism, etc But it is easy to show that, given any of the usual meanings of *ad hoc*, no theory (or singular hypothesis, even) is *ad hoc* unless it is *logically equivalent* to all or a portion of the evidence (see Maxwell, 1974) And I believe that I have shown (Maxwell, 1975) that *simplicity* offers no hope as far as accomplishing the hoped-for miracle is concerned

[7] It may well be true that we have no good *reasons* to believe that knowledge, confirmation, true belief, etc , are possible, but surely it is also true that we have no good reasons for believing in their impossibility (provided we are willing to abandon *un*reasonably stringent requirements for certification of knowledge, confirmation, etc) It is perhaps, then, moot whether it is "rational" to hope, or *believe*, or have faith that knowledge, etc , is possible However, it seems obvious that it is not *irrational* to so hope and believe, although I have no desire to begin any debate about the words "rational" and "irrational " It even seems to me that we would be well advised to act as if knowledge, etc , *were* possible I am not quite sure what this means, but I think that it might be explicated by means of decision-theoretic considerations Whether or not this, if true, amounts to a "vindication" of such beliefs or modes of action, I do not know Ironically, it is somewhat similar to one part of Reichenbach's attempted justification of induction I say "ironically" because he had set himself what I have tried to show is the impossible task of justifying an evidence-plus-logic-alone method

than would be the case for random selection among the possible theories, each of which is equally well supported by the evidence-plus-logic, and they must be able to make not-too-hopeless (subjective) estimates of the *prior probabilities* of those theories that *are* proposed and considered. This restates the imaginary contention of Eccles in a less elegant, less colorful but somewhat more precise manner. Since the data-plus-logic are always neutral toward a bewildering multitude of competitors, we must use our horse sense to discover ("think up") and propose a small subset of these theories and to choose among those proposed, selecting those kinds that, we believe, turn out most often to be true.

For the purposes of this essay, the point to be emphasized that has emerged from these considerations is that scientific theories (of appreciable importance, interest, etc.) and many "philosphical" theories, *including theories about mind and brain,* are all in pretty much the same boat. Why has this not been noted, indeed, why has the contrary been so strongly maintained? Well, scientists, blissfully unaware of the finer points of logic chopping, have gone right along using their horse sense to propose theories and to (subjectively) estimate their *prior* probablities (at least they have done so tacitly or implicitly) in order to eliminate some of those proposed and to calculate (again, perhaps tacitly and implicitly) *posterior* probabilities or degrees of confirmation of those not eliminated. Philosophers and "inductive logicians," not entirely aware that scientists were doing this (or believing that they ought *not* be doing it even if they were), maintained the comfortable faith that, although Hume's problem ("of induction") remained unsolved, somewhere (maybe in Plato's Heaven) a solution existed, and that evidence-plus-logic-alone could decide among—confirm or falsify—*scientific* theories. Being more familiar, or course, with "philosophical" theories, they were aware that this did not hold for them and, thus, went on to infer that philsophy and science must be forever disjoint. Only Russell, Duhem, and a few others have noted that scientific theories (of appreciable important, etc.) are little, if any, better off.[8] Before continuing, let me admit and insist that there are extenuating circumstances. *Some* philosophical problems *are* mostly logical, conceptual, or linguistic in character,[9] and *most* philosophical problems have important, sometimes crucial, logical, and conceptual

[8] Quine has noted it but has drawn therefrom, I believe, some mistaken conclusions Unlike Quine's, my refusal to draw a sharp line between science and philosophy depends in no way upon a rejection of a sharp analytic–synthetic distinction Nor does the confirmation predicament, at least part of which Quine correctly recognizes and insists on, give any viable support to rejection of the latter distinction For a detailed discussion, see Maxwell (1976).

[9] As are *some* chemical, psychological, physical, etc , problems Recall, for example, what Einstein accomplished by a logico–conceptual analysis of the notion of *simultaneity*

components (as do many scientific problems). For example, most of the negative results vis-a-vis confirmation theory upon which I have drawn so heavily herein have been obtained almost entirely by logical and mathematical means; and I believe that the "free-will" problem is mostly a linguistic and conceptual one (although I am not entirely comfortable with any of the solutions that have been proposed for it). Moreover, problems that are popularly called "philosophical" problems are, generally speaking, perhaps even a tiny bit more loosely connected with evidence than are those that are considered to be *scientific*. (What this means will be discussed presently. For a more detailed discussion, see Maxwell [1976]).

As a case study, let us now take some of the principles that we have been considering and use them to dispose of *one* mind–body theory, the kind of mentalistic monism that has been called "subjective idealism." The "case study" will be a long one, but much of the material that is developed in it will be used later when we are more substantively concerned with the mind–brain problem. For the benefit of nonphilosophers (if such persons exist), subjective idealism is the contention that nothing exists except minds and their contents, or, at any rate, that we can have knowledge of nothing else and, therefore, have no right to assume that anything else exists. For those offended by the phrase, "minds and their contents," there are many alternative ways of stating the position. We could, for example, speak of *sense experience, sense contents, items of direct (private) experience,* etc. No way of expressing the view will please everyone, and some contend that it cannot be meaningfully stated at all. We must brave their wrath and proceed. The position, in my opinion, is very similar to most varieties of phenomenalism. At any rate, most of what I shall have to say about it will also apply to phenomenalism.

The battles against this view, I believe, have been all too frequently "won" too easily. Many of the defects, for example, in Berkeley's already very clever and rather compelling exposition of it are quite easily removed. In particular, there are no insurmountable logical, linguistic, or conceptual obstacles in the way of its acceptance. (I know how heretical this sounds but hope that the reader will bear with me, for arguments' sake if for nothing else.) It is true that the view presupposes that we can meaningully talk about mental events (or their "ingredients"—items of private experience, "sense contents," feelings of joy and sorrow, etc.) This rather modest assumption is challenged today by many philosophers, who subscribe to the Wittgensteinian "argument against private languages," or to something similar to it. There is neither the space nor the patience, here, to deal with such hangovers from narrow positivist verificationism—with the epistemologistic fallacy of conflating the meaning of a statement with its "method of verification" (or confirmation). I have treated the matter (with the help of an apostate positivist—he was never a very devout one) in Feigl and Maxwell (1961) and in more detail in Maxwell (1970*a*). I shall only say now that the objection is completely demolished, I believe, by applications of the considerations about confirmation theory and their implications for science and philosophy that are given above. However, the argument that I shall give *against* subjective idealism is general in character and can be applied, *mutatis mutandis*, against a number of other philosophical views. For example, I should hope that anyone who still balks at "private languages," etc. and who holds that our talk is mainly (though not necessarily exclusively) about medium-sized material objects will see how the argument, whatever its defects may be, would be used against an instrumentalist view of scientific theories. (Instrumentalism is the view, crudely put, that "ordinary" material objects [or some other kind of observables] exist but *not* the unobservables [electrons, fields of force, etc.] that some [misguided noninstrumentalists] contend many scientific theories are about. What *are* they about? "Nothing," says the instrumentalist, "they are *mere* instruments, calculating devices, cognitively meaningless sounds or marks on paper or blackboards").[10] It is, at the least, meaningful I have been arguing, to claim as does the subjective idealist, that we are directly aware of (are *acquainted with*, have direct knowledge of, etc.) items in our direct (private) experience, whether they are (or are called) sensations, sense contents, feelings, thoughts, emotions, etc., that we are directly aware of (are acquainted with, directly observe, etc.) nothing else and, finally, that these items are the only *direct referents* of the descriptive (nonlogical) terms of our language. I want to claim not only that this is *meaningful*, but I should like to grant that it is *true*—for the time being and for the sake of argument, if for no more and nothing else.[11]

The subjective idealist now plays one of his two aces. "If we can only observe sense contents, items in our own minds (call then what you will), if the only properties, objects, etc. with which we are acquainted or (directly) know anything about are mental in nature, how, then, can we even form any idea or concept of other properties or

[10] For arguments against contentions such as those of Nagel (1961) and Carnap (1963) to the effect that the differences between instrumentalism and realism (or between phenomenalism and realism—*or* between subjective idealism and realism) are mere linguistic differences see, e.g., Maxwell (1970*a*), to say nothing of the material that has preceded and that is to follow here

[11] It is easy enough for *me* to grant these preliminary contentions of subjective idealism because, as a matter of fact, I am firmly convinced that they are not only true but that they are extremely important, especially for the mind–body problem and that they are virtually forced upon us by contemporary physics, physiology, psychophysiology, etc. But more of this later

entities of any sort? Obviously we cannot," he continues, "and if we cannot form any concept of nonmental items, certainly we cannot talk about them, we cannot refer to them directly *or* indirectly and, *a fortiori,* we cannot have any knowledge (direct *or* indirect) of them or any right to suppose that they exist." A great many contemporary philosophers, including myself, have considerable respect for this argument, but few of us accept its conclusion. Most of these philosophers, apparently, feel that the inference is valid or, rather, could easily be made so by supplying relatively unproblematic missing premises. Believing that the conclusion is absurd, they take the argument to provide a kind of *reductio* proof of the falsity of the premises. I contend that the argument is invalid and remains so after addition of any reasonable missing premises, so that it is perfectly consistent to reject the conclusion and maintain the premises. In so doing, however, I agree that I am obliged to explain how we can have a right to believe in the existence of unobservable properties and other entities and how we can "talk about them" or refer to them (indirectly). But it will be convenient to wait until the subjective idealist has played his other ace. "Suppose that, *per impossibile,* we *could* form meaningful concepts of mind-independent entities, talk about them—refer to them indirectly, whatever this may mean. Even so, " continues the attack, "there would not be the slightest reason to believe that they existed. There would not be—could not be—any evidence from which we could reasonably infer any thing about the nature of such entities or, again, even that they exist." Many seem to be even more impressed by this argument than by the former one. However, it is grossly defective and is plausible only when an extremely naive view of confirmation is held, the view that the only legitimate modes of nondeductive inference are simple inductive ones. Hypothetico–deductive (or, better, hypothetico–inferential) explanations of events of which we are directly aware by assumptions (theories) about mind-independent entities provide excellent confirmation of such assumptions provided the prior probabilities of the assumptions and of the (directly experienced) evidence fall within certain (very wide) intervals. Let us return, then, to the first argument.

Long ago, Bertrand Russell (see, e.g., Russell, 1905 and 1912) explicated the important distinction between *knowledge by acquaintance* and *knowledge by description* and explained, with his theory of description (definite *and* indefinite), how it is that we are able to refer, *indirectly* to (or to *denote*) entities with which we are not acquainted. When we say, "The author of *Waverly* was knowledgeable" ("$(\exists x)(Wxw.(y)(Wyw \equiv x = y).Kx)$"), then, provided one and only one person did write *Waverly,* we have managed to denote that person or to refer (indirectly) to him (or her), in one perfectly good sense of "refer" and, moreover, to say something about the author, even though we have not observed him, are not acquainted with him, and may not have any idea as to *who* he was (or is). This indirect reference is accomplished by using an existentially quantified (individual) variable[12] and words whose *direct* referents *are* items (things, properties, etc.) with which we are aquainted.

In what amounts to a development of Russell's theory[13] Frank Ramsey (1931) provided the formal apparatus for clarifying our indirect reference to unobserved and unobservable *properties, sets,* etc. The details need not concern us here. They are similar to those of Russell's method for referring indirectly to individuals; in the "Ramsey sentence," existenially quantified *predicate* variables (or other variables of higher logical type) are used to refer indirectly to properties, sets, sets of sets, etc. (The resulting descriptions are always indefinite ones.)

Applying these results to the subjective idealist's first argument, we see that we have the means for talking about, referring (indirectly) to, expressing (possible) knowledge about, etc., items with which we are not acquainted, which are unobserved and, even, unobservable, whatever one may take such unobservables to be. For the subjective idealist, everything is unobservable except sense contents and other mental entities. However, Russell's theory of descriptions, in general, and Ramsey sentences, in particular, make it clear that there is no difficulty at all in talking about, hypothesizing, and theorizing about entities, which, for the subjective idealist as well as for others, are unobservables. Whether or not this enables us to form *concepts* of mind-independent entities is, perhaps, moot. What *is* a *concept*? It is true that, in our new language that we have *reformed* to accommodate the subjective idealist and the rest of us who hold that only sense contents, emotions, etc. are observable, there are no descriptive terms that refer directly to mind-independent objects. This however, is no obstacle to our saying all that we need to say about them (using indirect reference) nor to our *knowing* a great deal about them—by confirming and disconfirming hypothetico-inferentially our theories and hypotheses about them, as explained above.

What I have done so far is to defend the subjective idealist's claim that there are no logical, linguistic, or conceptual barriers that prevent his position from being meaningfully asserted. Then, I count-

[12] In English, words like "some," "something," and other "logical" words perform the function of such variables

[13] That Ramsey's device is an extension of Russell's theory of descriptions does not seem to have been generally recognized

ered his claim that *only* his position can be asserted, showing how, even when one grants (as I *do*) that the only observables are mental entities, there are no logical, conceptual, or linguistic barriers to talking about and having knowledge about mind-independent entities. So far, our work on this issue *has* been purely logical, linguistic, and conceptual. The subjective idealist has failed to *establish* his position by logical, linguistic, and conceptual means, but, we have seen, his position cannot be *refuted* by such means either.

This, in my opinion, is entirely as it should be. Subjective idealism is a *contingent* theory, true in some possible worlds and false in others (which include the actual world, I believe). To evaluate it and its alternatives we must go beyond the realm of the logical, the conceptual, and the linguistic and consider the evidence and other factors necessary for confirmation or disconfirmation. The evidence, of course, is the whole of our experience—our sense experience, our experienced emotions, thoughts, etc., etc., including any regularities, irregularites, anomalies, etc. in it that we remember or have recorded (or that we *think* we remember or *think* that we have recorded).

Let us suppose, first, that the worst possible situation exists—the situation in which confirmation or disconfirmation would be the most difficult. Let us suppose that subjective idealism and whatever alternative to it we are considering—some kind of realism—account for the evidence (*explain* our experience) equally well. Even in such a case, I claim that, given the evidence, *realism would be much better confirmed than subjective idealism*. For, we have seen above that one indispensable factor in the calculation of degree of confirmation is our estimate[14] of the prior probability of the theory of interest. I would estimate the prior probability of realism to be *much* higher than that of subjective idealism. Are the readers appalled that, in such a case, I would stake my defense of realism and my rejection of subjective idealism on a pure guess, a (subjective) hunch about prior probabilities? Dear friends, as I have asked elsewhere (Maxwell, 1976), what is the alternative? We have to do pretty much the same thing,[15] as was already so clear to Hume, when we "bet" that our next mouthful of bread will nourish rather than poison us.

The actual situation is not quite as bad, although it will,

[14] Although we must depend on our *subjective* estimates of prior probabilities, the prior probabilities themselves (as well as the posterior ones) are objectively existing relative frequencies (Roughly, they are the relative frequency with which theories that *resemble* the theory of interest in certain relevant respects are true or close to the truth For details see Maxwell [1975, 1976])

[15] Not exactly, it is true Remember, however, that both hypotheses *can* be "made" to account for all of the evidence and, crucially, that we have to make a guess about the prior probabilities before we can say that the "nourish" hypothesis is the better confirmed

doubtless, leave many just as unhappy. The auxiliary theories that are needed in order for subjective idealism to account for all of the evidence (our experience) are different from those that are required in order for realism to do so. In computing the degree of confirmation or the posterior probability of a theory on the basis of the evidence at hand, there are a number of equivalent ways to handle the prior probabilities (prior to the evidence in question) of the auxiliary theories. For our purposes, it will be convenient to consider, first, the posterior *and* the prior probabilities of the entire conjunction (of the theory of interest, the auxiliary theories and other "background knowledge"). Now I maintain that the auxiliary theories, etc., that must be conjoined with subjective idealism in order that the evidence be accounted for are much more convoluted, complicated, contrived, etc. and, therefore, are much more *implausible* than those that are required in order that realism do so. We should, then, estimate their prior probability to be much lower and, thus, that the prior probability of the entire conjunction in the case of subjective idealism to be much lower than for the case for realism. Since the evidence is the same for both cases, the degree of confirmation or the posterior probability of the conjunction containing subjective idealism will be much less than that for the one containing realism. It is true that it does not follow, deductively, from this that the degree of confirmation for subjective idealism is less than that for realism. What does follow is that in order for subjective idealism to account for the evidence, that is, *in order for the evidence to be relevant, to give any support at all to subjective idealism, we must assume that theories with extremely low probabilities are true, while this is not the case for realism*. We are, thus, entitled to conclude that the evidence gives very little support to subjective idealism but, given our estimates of the relevant prior probabilities, that it gives strong support to realism.[16] We should, therefore, conclude that, on the basis of the evidence, subjective idealism is, in all probability, (contingently!) false and that (some variety of) realism is (contingently) true.

Fortunately, now that these lengthy, general considerations are completed, we can make short shrift of the mind–body problem. It

[16] I cannot take the space, in an article already becoming too long, to go into details about what these various auxiliary hypotheses might be or why I consider the ones necessary for subjective idealism so much less plausible than those for realism. To mention only one, subjective idealism must either account for our ability to communicate with "other minds" or go on to embrace what many claim is its "logical conclusion," solipsism. I would certainly estimate the prior probability of the latter position to be very low, and any auxiliary (nonrealist) hypothesis that I can imagine that would account for the communication with other minds seems fantastic and unlikely indeed

is not the purpose of *this* essay to *solve* it but, rather, to argue for the crucial relevance for it of experimental and theoretical scientific results. It is true that in doing I shall indicate what I think its "solution" to be, but I shall reserve for another occasion detailed arguments in its favor.

There are a number of obvious but often forgotten ways in which scientific or, even, common-sense knowledge (actual and/or conceivable) can have important implications for the mind–body problem. For example, Aristotle, with his usual scientific acumen[17] announced, so I am told, that the function of the brain was to cool the blood, the seat of the soul being, I suppose, in the heart. If this were true, we would have a mind–heart or soul–heart problem but no mind–brain one.

More seriously, there can be no doubt that contemporary developments in neurophysiology and psychophysiology, rudimentary as they may be, count heavily against a radical interactionist dualism. (By "radical interactionist dualism," I refer to the contention that *some* mental processes are radically autonomous, being neither [directly] caused by nor regularly correlated with [or "paralleled by"] brain processess.) On the other hand, if the evidence were very different from what it is, if, for example, there were evidence of the existence of disembodied minds, evidence that people really can "leave their bodies" as some do claim to do, or evidence of survival after the destruction of the body, etc.—if there were evidence of this kind, it would be reasonable to conclude that radical interactionism is true. Of course, it would *not* thereby be proved, nor would alternatives such as, even, materialistic monism be falsified. However, the auxiliary theories that would be necessary to "save" materialism in the face of such evidence would be so bizarre as to be given a prior probability near zero. The evidence being what it actually is, the prior probabilities of the auxiliary theories that would have to be assumed to save radical interactionism should be estimated to be very low.

Recalling that we have already, on a scientific or, at any rate, a contingent basis, disposed of radical mentalistic monism (subjective idealism[18]), we see that the viable alternatives that seem to remain are (nonidealist) mind–body (mind–brain) monism, psychophysical paral-

[17] Other examples Women have fewer teeth than men, and elephants can be cured of insommia by rubbing salt into their hides (see Russell, 1946) He also revived the naive realist view of perception and knowledge of the world, a view that had already been demolished by the pre-Socratics, especially by Democritus and the other early atomists (see, e.g , Anderson, 1974)

[18] I am not competent to deal with *absolute* or *objective* idealism However, from what I am told, I would guess that its prior probability is low

lelism, epiphenomenalism, and a view, let us call it, "*mild* interactionism."[19]

Many philosophers contend that there is no "difference that makes a difference" among the last three (dualist) positions. I have a considerable amount of sympathy with their contention, especially in view of the unsatisfactory state of our knowledge about the nature of causation. Nevertheless, I believe that the contention is wrong. It is true that the three positions (as well as mind–body monism) can all be made to account for all actual and possible evidence, assuming that they are logically, linguistically, and conceptually acceptable. But as we know, *ad nauseum*, by now, this is very unexciting and by no means entails the no-difference-that-makes-a-difference contention.

Both parallelism and epiphenomenalism are extremely implausible intuitively. This, of course, does not *entail* that their prior probability is low, but, often, even in science *and* in everyday life we have to base our (sometimes vital and crucial) estimates of prior probabilities and, thus, our eventual selection or rejection of theories or hypotheses on nothing more than intuitive plausibility. In cases where there is more to go on, well and good! But all that we can do, in a given case, is the best that we can. These two positions are implausible, apart from the feeling of mystery engendered by dualism, in general, and the *preestablished harmony* required by parallelism, in particular, because of the strange network of causal relations that they involve. Whatever the meaning of "cause" may be, it seems as certain as anything is certain that, if there *is* any such thing as causation, then my getting pricked by the pin *caused* an intense feeling of pain, which, in turn, caused a quick flash of anger. My guess is that the prior probability of this is so high that I must assign to parallelism, which it contradicts, a very low one. And it seems equally certain that the pain and the anger *caused* me to slap away at the arm of the person who pricked me. This, in turn, causes me to estimate a very low prior probability for epiphenomenalism. (Also, as Smart [1963] and others have noted, there does not seem to be anywhere else in the universe the kind of total causal impotence that epiphenomenalism requires mental events to be possessed of. It

[19] *Mind–body monism* is the denial that "the mental" and "the physical" are drastically different from each other The following three positions deny this denial According to *parallelism* each kind of mental event (or mental state, etc.) is invariably accompanied by its own peculiar kind of physical correlate, but there is no causal interaction in either direction between the mental entity and its parallel physical event (or state, etc.). *Epiphenomenalism* (not to be confused with the unrelated (perceptual–epistemological) theory called "phenomenalism") assumes that the same kind of correlation exists but that the physical events *cause* the mental ones *and* that the mental events, however, have no causal efficacy at all. *Mild* interactionism, assuming the same kind of correlation, holds that there is causation in both direction These crude characterizations are sufficient for our purposes here They may be ignored by professional philosophers

seems unlikely [contingently unlikely, *I* would add] that such "nomological danglers" exist.[20])

Mild interactionism, although *prima facie* more plausible, is also beset with causal anomalies. There are danglers of a sort here, too. For many mental events would have *two* causes, and not in the usual innocuous senses of there being a (large) number of serial events in the same causal chain *or* of there being two or more necessary events in the *set* of events that is sufficient to produce the effect. It is rather a queer kind of "overdetermination." Each cause, the mental one and physical one, it appears intuitively, is *sufficient*, quite independently of the "other" cause, to produce the effect. For example, the feeling of pain alone seems sufficient to produce the brain state or event that initiated the efferent neural impulse that was responsible for my slapping the hand, as does *also* the brain state or event that neurophysiology assures us preceded *it* and, indeed, caused it (also?). Our reflections lend, for example, considerable plausibility to the contention of parallelism that the physical realm is *causally closed*. All of this produces the inclination to estimate a quite low prior probability for mild interactionism.

The only possibility that remains seems to be some kind of mind–brain monism, and, since we have rejected radical mentalism, we seem to be left with some variety of mind–brain identity. But, as we shall see in a moment, this seems, *prima facie*, not only implausible and improbable but downright absurd; indeed, when a few facts that seem trivially obvious are taken into account, it seems logically or, at best, conceptually absurd. If mental events really are just brain events—just physical events—then *all* events are physical events. Is this not just plain old materialism[21]—nothing more? Materialism denies the existence of *bona fide* mental events (states, etc.) altogether or, at best, tries somehow to sweep them under the rug (by holding, for example, that mental entities are not at all like what we think they are—indeed not even like what we *think* that we directly experience them to be—that when we *do* come to understand what they *really* are like, we shall see that indeed they are *nothing but* neurons firing, electrons jumping from one energy level to another, etc.).

It is difficult to know how to give *arguments* against material-

[20] Unlikely that nomological danglers exist, *not* unlikely that mental events exist, even though Smart and other materialists *do* seem to want to infer *something* like the nonexistence of the latter from the nonexistence of the former Nonmaterialists, such as Feigl (1960), infer, correctly I believe, simply that mental events are not nomological danglers

[21] Sometimes nonphilosophers take materialism to be simply either parallelism or epiphenomenalism (See, e g , Gunderson [1970]) This is reasonable because these positions make mental entities second class citizens, or lower However, they do acknowledge the (full-fledged) existence of the mental, and they are not usually classified as materialisms by professional philosophers

ism, just as it would be difficult to give arguments against someone who maintains that I am feeling no pain and that my flesh is not being seared while my bare foot is being held firmly against red hot coals. I can only protest that I am more certain of the existence of my sense experience and emotions, and I am more certain about their nature, as I live through all of their qualitative richness than I am of anything else, including the most "firmly established" scientific principles, to say nothing of any philosophical view. I, therefore, estimate the prior probability of materialism to be very near zero. Again, admittedly, this is not much of an argument and is, perhaps, not a very exemplary attitude. But let us recall once more that in science and in everyday life, we often have to make crucial decisions on the basis of prior probabilities that have been estimated on just such bases. Moreover, I do not believe that there are any viable arguments for abandoning *these particular* beliefs about our experience so intuitively congenial and so strongly held. However, somewhat ironically, I shall argue presently that some of our beliefs that are almost as intuitively certain as these must be abandoned in the face of contemporary scientific findings. However, these will, in fact, turn out to be the beliefs that start the materialist on the wrong track. The rejection of them will make clear why I contended above that there is no scientific sanction for abandoning the beliefs that the materialist wants us to give up.

We have rejected both (radical) mentalism and materialism. Does this leave hope for *any* kind of monism and for a mind–brain identity theory, in particular? It might seem that we are worse off than ever. We have seen that we know intimately and quite fully what our experience and the items in it are like. And, we common-sensically believe, we also know almost as intimately and in considerable detail what physical objects, events, etc. are like, whether they be tables, chairs or brains that we can see (that we *believe* that we can see) or whether they be neuron firings, electrons that go from one energy level to another, etc. We know, then, what *the mental* is like and what *the physical* is like, and is it not obvious that they cannot be identical? As I once heard Benson Mates remark, it makes about as much sense to identify a mental state with a brain state as it does to identify a billy goat with a quadratic equation. And, indeed, given these obvious "facts" that we know about the mental and the physical, it isn't just that we estimate the probability of their identity to be near zero, but there seem to be genuine conceptual obstacles to identifying them with each other. Here is another point at which scientific results come to our rescue. They imply that we must surrender some of the beliefs so dear to materialists and, evidently, to just about everybody else. (These beliefs *are* so deeply embedded in common sense that they usually operate only at the tacit, implicit, or, perhaps, unconscious level.) It turns out, however, that, when we do accept the necessity of abandoning them, the materialist has the matter entirely reversed. It is not our concepts or, better put, our *beliefs* about the nature of the *mental* that must be radically altered but, rather, our notions about the nature of the *physical* that must be drastically revised.

Before proceeding, let us recall that we defended the conceptual legitimacy (the "meaningfulness") of the subjective idealist's contention that everything of which one is directly aware is an ingredient of (or occurs in, or is a content of, etc.) his own private experience. Although we expressed sympathy, we left open the question as to whether or not the claim is true. I now want to argue briefly that it *is* true. However, I do not believe that its truth can be established by purely logical, linguistic, or conceptual means, nor by appeals to "epistemological primacy," and certainly not by the (alleged) certainty, incorrigibility, etc., of judgments about one's own experience. The crucial argument is, again, from science. Physics, physiology, and psychophysiology virtually force us to conclude that, for example, a blue patch of color of which we are visually aware is exemplified in and only in our minds (or in our brains—to leave *this* issue open for the time being). (Considerations from these sciences also corroborate what should be pretty obvious already—that each one's experience is indeed private unto oneself.) It is, of course, very fashionable today to maintain the philosophical irrelevance of "the causal theory of perception." But this is just one more instance of the general prejudice that scientific results are irrelevant for philosophical issues. (For a detailed discussion of the relevance of the "causal theory of perception," see Russell [1948] and Maxwell [e.g., 1972].)

The conclusion reached above about the blue patch of color holds for all of the sensory qualities. Admittedly scientific theory does not *prove* (deductively) that these properties are exemplified only in our experience and that, for example, the table top that we think we perceive to be brown is not brown because it is not colored—color exists only in our private experience. What it does show is that we have no good reason for supposing that it is brown. For, a complete physical, neurophysiological, and psychophysiological account of everything that happens when I do what we commonsensically (and, strictly speaking, somewhat mistakenly) call "looking at a brown tabletop"—such an account would not mention anything brown[22] until the psycho-

[22] In the usual, primary sense of "brown"—*brownness* as we are directly aware of it in visual experience

physiologist mentioned the visual *experience* of brownness. Again, this does not *prove* that table tops, in particular, and, indeed, "mind-independent, external" objects, in general, are *not* brown, but it does make the assumption that they *are* brown as gratuitous and unwarranted as the assumption that there are now exactly 5481 imperceptible demons dancing on this page.

If, then, naive realism is to be rejected—if physical objects are not at all like we have believed them to be—believed, indeed, that we *perceived* them to be, what *are* they like? How do we know that they exist? How can we formulate and confirm or disconfirm knowledge claims about them? Here, the material developed in our "case study" above again stands us in good stead. Insofar as we know anything about physical objects (and we have reason to believe that we know a great deal), they are like what our well-confirmed theories (reformulated so that all of their "observational terms" refer to items of our [private] experience) say that they are. If we want to be strictly correct—for most purposes we do not need to be—the theories are formulated as Ramsey sentences. This kind of indirect reference to the entities, the properties, individuals, sets, sets of sets, etc. of the mind-independent, "external," or *physical* (in *one* sense of the term) realm makes explicit our ignorance about *what* these properties, etc. *are*. What is expressed is our knowledge *that* they are (they exist) and our knowledge about their higher type (higher *logical* type) properties, relations, etc.—in other words about what Russell terms their *structural* properties. (The lower type properties of the physical realm are what he calls *intrinsic* properties, and our ignorance as to *what* these *are* is indicated by our referring to them only by description—by indirect reference, as described in our "case study.")

I have discussed all of this in detail elsewhere (e.g., Maxwell, 1970*a*, 1970*b*, 1972), giving arguments where appropriate, etc. Here, I am concerned mainly to reemphasize that *it is mainly science* rather than the traditional philosophical arguments *that makes the rejection of naive realism*[23] *mandatory* and to go on to use this result in our "solution" of the mind–brain problem (with which we have almost finished).

Since physical objects, states, and events are not at all like we common-sensically believe them to be, or, at any rate, we have no good reasons for believing that they are, since our best scientific results—up to *this* point—provide knowledge only about their structural properties and leave entirely open the question as to what are their *type-one*, or *intrinsic* properties, then, *the possibility is entirely open that some of these*

[23] The realism that we accept is similar to though not identical with so-called *representative realism*. Our "case study" above showed that the usual arguments against representative realism are grossly unsound.

properties just are the ones that are exemplified in the events that constitute our own private experience. If this is true, then *mental events are* indeed *one kind*—perhaps a rather special kind—*of physical events.*

Let us pause to reflect on the important step that we have just taken. Science urged upon us the rejection of naive realism (as well as the acceptance of an alternative kind of realism). We see, then, that we are not directly aware of mind-independent ("physical") entities; we do not really observe them. They provide (crucially important) links in the causal chains that produce our perceptive experiences, and something (structurally) similar to our common sense perceptual "knowledge" can be *rescued* (and confirmed) by reformulating it as (theoretical and/or hypothetical) *knowledge by description* (using Ramsey sentences or other, equivalent devices). "Biting the bullet" and admitting that we do not observe ("external") physical objects and, thus, do not have observational knowledge of what their intrinsic properties are and, moreover, that those intrinsic properties that we mistakenly attributed to them are, in all probability, exemplified only in our ("internal") private experience—all of this removes completely, in the manner explained above, the enormous intuitive and conceptual obstacles to identifying *the mental* with (a portion of) *the physical.* It emerges from this that scientific results not only often have crucial implications for those (numerous) philosophical propositions that are *contingent* in nature but, also, they can rescue us from seemingly hopeless *conceptual* quagmires such as the one that detained us after we have rejected, above, subjective idealism, parallelism, etc. Therefore, even those who (mistakenly!) hold that all philosophical problems are conceptual, linguistic, or logical in nature, cannot validly infer from this that scientific (or other contingent) knowledge is irrelevant for philosophical issues. (For a general [non-Quinian] discussion of how contingent knowledge can have a [usually indirect] bearing on "purely" conceptual or linguistic matters, see Maxwell [1961].)

It is true that giving up our common sense beliefs about perception, the nature of physical objects, etc., is a pill that goes down hard and certainly not a step to be taken lightly or hastily. It might seem, using the jargon that we have developed, that the prior probability of these beliefs is so high that they should stand in the face of any evidence whatever. However, as Russell (1959) noted, we are faced with a painful choice, a choice, moreover, that cannot be avoided by pleading the "philosophical" irrelevance of "the causal theory of perception." We are faced with the choice no matter what the nature of philosophy may be; whether one chooses to call it a "philosophical choice" or not, it will not go away. Russell's choice concerns the kind of considerations given above about the situation that we commonly call, "looking at a brown tabletop." We must choose between admitting (1) We do not see a tabletop, (2) We do not see anything brown (i.e., we do not have brownness exemplified in our visual experience[24]), or (3) physics, neurophysiology, and pychophysiology are grossly false. We have chosen (1), but, as Russell points out (in different words), choosing either (2) or (3) would have resulted in even more violence to the evidence and to our intuitions and would have involved rejecting beliefs for which we should estimate even higher prior probabilities than we do for those that we must abandon as a result of choosing (1). It could be pointed out that we did not consider a fourth alternative. We could have chosen to interpret physics, neurophysiology, etc., instrumentalistically—in which case the theories of these sciences would be neither true nor false and therefore not true. I am not sure, however, that this would free us from choosing among the first three alternatives. For these theories, althought they had become mere calculating devices, would still yield

[24] Quite a few philosophers assure me, in all apparent seriousness, that they do not know what this means (meaning, of course, that they deny that it *has* any meaning). Philosophy, being the study of basic, often implicit and unconscious, beliefs, *should* be full of surprises including *prima facie* absurd avowals. We should, therefore, in general, learn to be tolerant and at least go through the motions of having an open mind (as Herbert Feigl says) in the face of such strange denials. I must admit, however, that I am tempted to reply to this particular instance of know-nothingism with "You know perfectly well what I mean." Such a denial may (or may not) be based partially on something like Ryle's (1954) ruminations about "success words," according to which the word "see" is used appropriately only when we have successfully *seen* an external, mind-independent object. (Rylians will, no doubt, also object to the use of the last three or four words in the preceding sentence. However, I cannot remember exactly how he put it, and it is impossible for me to obtain a copy of his work at present.) If Ryle were correct, this would seem to make my use of the phrase, "brownness exemplified in . . . visual experience" as an alternative to "see anything brown" even more admissible instead of rendering it naughty. But, I am told, the meaninglessness of my expression is also partly due to inappropriate (nonordinary) use of the word "experience" (or, perhaps, also of "visual," "exemplified," "brownness," etc.) This illustrates, in my opinion, a very common fault of ordinary-language philosophy. It refuses to recognize (and at times seems to be *based* on such a refusal) the myriad of (very useful and desirable) ambiguities and vaguenesses that make "ordinary language" such a flexible and expressive instrument. *Contra* this school of philosophy and other old-time positivists, we can use it to say almost anything we want to say, including things philosophical.

Let us suppose that words like "see" *are* most commonly used in "success" situations of the kind *required* by Ryle. But they can also be used in different (though related) ways even, I should say, while retaining virtually the same meaning (*intension*)—or, if the meaning does change, a large amount of "core meaning" remains common to the different uses. For example, we talk of "seeing in our mind's eye," or when remembering a *vivid visual* experience (!) (forgive me! How else should I put it?), we say, "I can just see it (him, her) now!" And surely we, quite properly, speak of *seeing* things in dreams (or should we speak rather, of *visual experiences*?) There is just no good reason that we cannot see something (or have a visual experience of something) that is not an ordinary material object. In a still more different though still closely related use of "see," we can, with perfect propriety, speak, as Russell (1948) does, of *seeing our own brains* (every time we have a visual experience).

Finally, we must recall that we actually never succeed in seeing a mind-independent, physical object, although we usually mistakenly think that we do. If this allegedly basic use of "see" were the only legitimate one, then we would never *see* anything. (This is consistent with the (attempted) "success" use being the basic or, even, the first-learned use [or attempted use]. We can learn from false beliefs [and unsuccessful though *apparently* successful attempts] provided that they are not too hopelessly off the mark. How this might be done, I have discussed in Maxwell [1970*a*].)

the same observation statements one of which, we have argued, is something like, "If mind-independent objects *are* observable, then they are *not* observable," which entails, of course, that mind-independent objects are not observable. (See Maxwell [1960] for the details of a similar argument as well as for general arguments against instrumentalism.) This argument depends, however, on whether or not statements like, "So-and-so's are observable" are, themselves, observation statements, so I shall not press it here. The main thing is that instrumentalism seems completely unacceptable. We saw in the "case study" above that there are no viable logical, linguistic, or conceptual obstacles to realism, and it seems undeniable that the preponderance of the evidence, when reasonable estimates of the requisite prior probabilities are used, is overwhelmingly in favor of realism (vis-a-vis scientific theories as well as elsewhere).

Let us return to the point at which we had rejected naive realism in favor of an ontology and epistemology that acknowledge our ignorance of the intrinsic properties of physical entities but recognize and avow their existence, as well as the legitimacy of our knowledge claims about their structural properties. To best utilize this result for the mind–brain problem we need to replace (common sense) *substance* metaphysics with an *event ontology* (in the manner of Russell [1927, 1948] and Whitehead, e.g. [1925]). *An event* is, roughly speaking, the instancing or the exemplification of a property, for example the occurrence of a blue expanse in the visual field or a twinge of pain (not to be taken to imply that all events are necessarily experiential). Substance ontology is objectionable on conceptual—perhaps logical—grounds (e.g., the absurdity of "bare" [propertyless] particulars). On the scientific side, contemporary physics is best formulated, in my opinion (again following Russell [1927, 1948]), using an event ontology. In such an ontology, *things* or *objects*, etc., are replaced by families of events, families of families of events, etc., causally related to each other in certain intimate ways.

The brain (perhaps it would be better, strictly speaking, to abandon the word "brain" with its naive–realist and substance–metaphysics connotations and say something like, "the entity that takes the place of the brain"), according to our event ontology, is a huge family of families of families, etc., of events. We now rephrase our earlier statement of mind–brain identity by saying that, in view of the considerations cited above, the possibility is entirely open that some of the events that *are among the constituents of the brain are mental events*, our sense experiences, our feelings of joy, our thoughts about Nirvana (as we live through them and know them in all of their qualitative richness).

At this stage, there are no reasons for not giving free range to our strong intuitions that mental events are both causally efficacious (vis-a-vis other mental events and [other!] physical events) and susceptible to (causal) production by (other!) physical events. Now if we follow Russell (1927, 1948), as I believe that physics and, perhaps, neurophysiology and psychophysiology will eventually indicate we should, and regard (physical) space–time as a construction out of the causal relations among events, we can say that mental events are just as much in time *and in space* as are other (!) physical events—since they are, we hold, just as much in the causal network. We see, then, that replacing our old notions about *the physical*, including (physical) space, with scientifically more adequate ones removes any conceptual obstacles to saying that mental events are in space. In fact, I am confident that it removes virtually all of the stock objections to a genuine mind–brain identity theory. All mental events are physical and some physical events are mental. As Russell (1959) notes, there is no more difficulty about saying that a given event can be both mental and physical[25] than there is in saying that a given man can be both a baker and a father.

The last point makes it evident that this identity theory is *not* a variety of materialism. Mental entities remain entirely mental and none the less so for being physical as well. It can be called, quite fairly, a kind of "physicalism." All events are physical and none the less so just because some of them are mental. Therefore any laws expressing regularities among them can be called, quite fairly, physical laws; whether some of the laws involving brain events are quite similar to or quite different from other physical laws is, of course, an open question. Certainly it is a kind of "identity theory;" mental events are identified with (though not replaced by) physical events. Whether or not it is a monistic theory is, perhaps, moot; Russell says that we should remain agnostic as to whether the events in the rest of the world are (intrinsically) similar to or very different from that subset of brain events that constitute the mental. If they are radically different, then a *kind* of dualism, but *not* a mind–brain dualism, remains; while if they differ only in degree (assuming this means much of anything) a kind of monism—a much watered-down panpyschism—would seem to be true. Fortunately, this is not an issue that has to be settled in this paper.

We must note here that Russell's construction of (physical) space–time out of the causal relations among events was sketchy and programatic and left important problems unsolved. Once someone

[25] This is *not*, however, *neutral monism* Although Russell never explicitly disavowed the term, it definitely does not appropriately signify, in any of its usual (phenomenalistic) senses, his later views on mind and body

completes it or a similar program, a number of difficulties, among them the "grain" problem (see, e.g., Sellars [1965] and Meehl [1966]) will, I believe, disappear. The "grain" objection, put very crudely, asks, "How can a 'mental' event such as the exemplification of a color patch in the visual field, which is smooth, continuous, and nongappy, be identical with a physical state or event, which, according to physics, has a gappy, discontinuous structure (an array of elementary particles, or of quantum transitions, or of singularities in a field, etc.)?" Well, in Russell's construction, a point in physical space–time is a family of events, and, while of course a point, by definition, is not extended, the events that are its members can be extended (*and* continuous) in space–time in two respects. An event, or rather its "ingredients" such as a color patch can be extended in *visual* space(–time), and an event can also be extended in physical space–time in that it can be a *member* of more than one point. It might be thought that this can't help much; for would it not be implausible to *over*extend an event, *a member of a point*, to cover a large region of the brain; and is it not likely that in, e.g., seeing a colored expanse, a large region of the brain is involved? I believe that part of this difficulty results from picturing "a large region of the brain," etc., in terms of common-sense space, which is a mixture of "phenomenal" space and physical space and which we have *replaced* with a network of causal relations among events. Whether this is true or not, I do not believe that this objection can be evaluated properly without proposing and analyzing in detail a number of possible brain models for perceiving, sensing, etc. In a holographic model, for example (see, e.g., Pribram, 1971), a relatively large region of the brain could be involved and yet each of a large number of very small regions within it would provide for (contain events that would be identical with—how *should* one express it?) sensing a patch of color. This is crude, merely suggestive, and, no doubt, would have to rejected or drastically modified if it were developed in detail. However, I do not believe that we should be discouraged by the "grain" argument until we have proposed and examined a large number of psychophysiological theories.

In this paper I have tried to show that scientific results have great relevance for the mind–body problem and, indeed, that a scientific approach to the problem seems to offer the most promising results. This is mainly because, as I have argued at length, there is at most a difference in degree and not a difference in kind between scientific problems and philosophical ones. The mind–body problem is very near the middle of the continuum; in fact I believe that it is nearer the scientific end (if we want to insist on retaining such a distinction even though there isn't too much point to doing so—save for academico-administrative convenience—but as Quine astutely reminds us: The universe is not the University).

We have seen that the "scientific" procedure of weighing the (observational) evidence—with proper use of the requisite prior probabilities—is appropriate and vital for a considerable portion of philosophical problems, in general, and for the mind–body problem, in particular. Another often unnoticed but crucially important way in which science has implications for philosophy is from the *theoretical* direction. Theory (admirably well confirmed) leads us to abandon naive realism. This in turn, we saw, enables us to formulate a viable mind–body identity theory. Theory also indicates, I claimed, that we should adopt an event ontology and then proceed to base our theory of (physical) space–time on the network of causal relations among events. This also greatly facilitates the development of and lends added credence to our mind–body view. We also indicated how neurological and psychophysiological theories coupled with this space–time theory might point the way to a solution of the "grain" problem. Finally, we saw how scientific developments can even point towards ways of solving "purely conceptual" components of a problem. Theory, in refuting naive realism, removed conceptual obstacles to an identity theory as does also the space–time theory just mentioned.

BIBLIOGRAPHICAL ADDENDUM

In presenting the mind–body theory sketched and defended here, I have referred most often to the work of (the later) Bertrand Russell. I have studied his work on the subject more recently and have it more at my fingertips than material from other sources. Moreover, I believe that his is the most nearly complete theory (although, admittedly, one must piece it together from various parts of his writings). Especially vital to the mind–brain issue is his event ontology and his views about space and time. However, several important thinkers have arrived, each more or less independently of the others, at positions very similar to Russell's. First of all, there is the justly renowned work of Herbert Feigl (e.g., 1960, 1971, 1975), to whom I am indebted in this and a number of other areas even more than to Russell. Unfortunately, many who are superficially acquainted with his mind–body views classify him as a more or less old-fashioned materialist. This is entirely mistaken. In fact, I can bring this addendum to a speedy completion by referring to his recent account of the independent development of the mind–body positions of Russell and of Moritz Schlick (Feigl, 1975). Not only does Feigl include a fascinating comparison of the views of these two philosophers, but he mentions a number of others who developed and defended similar views. Among them are Kant, Durant Drake, Roy Wood Sellars and other American "Critical Realists," and the late Stephen Pepper.[26]

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