

PROBLEMS AND PROSPECTS FOR THE
PHYSICALIST PROGRAM IN SCIENCE

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ABSTRACT

In this dissertation, a version of physicalism is formulated and evaluated. A number of potentially devastating objections are considered and strategies of defense are suggested.

In Chapter 1, the underlying philosophical and scientific motivations for pursuing the physicalist program are explored and three areas of critical concern are identified: ontology, objectivity and explanation. Criteria of adequacy and acceptability for formulations of the physicalist doctrine are proposed.

In Chapter 2, a taxonomy of physicalist theses is developed in terms of general categories of entity (i.e., linguistic and non-linguistic) and types of relation between classes of entities (i.e., reductive and non-reductive). A number of extant formulations of physicalism are located within the taxonomy and evaluated with respect to the criteria of adequacy. It is concluded that none of the formulations considered is adequate; and, it is argued that, in general, for a formulation to be adequate it must include both linguistic and non-linguistic theses and must not consist of only non-reductive theses.

In Chapter 3, the problem of identifying the physical bases is addressed. Past strategies are reviewed and their shortcomings identified. An alternative strategy is suggested which is based upon a principle for circumscribing the subject matter and research questions studied in physics. Three presuppositions of this strategy are isolated and defended against objections. It is concluded that the physical bases do not suffer from any pernicious indeterminacy and that they can be developed in a way which does not trivialize the theses or deprive them of their intended content. However, it is not clear that paradigmatically non-physical entities (e.g., mental states) can be definitively excluded from the bases, a prospect which threatens basic physicalist motivations.

In Chapter 4, a set of theses is developed which satisfies the criteria of adequacy. Two objections are formulated: (1) that the theses are "utopian" and (2) that they suffer from physicalist indeterminacy and, hence, are not acceptable on physicalist grounds.

In Chapter 5, three standard metatheses are formulated that characterize the scope of application, the empirical status and the methodological roles of the theses. It is argued against this "Received View" that the scope of the theses is not well defined and that the theses have a dubious empirical content. It is suggested that the theses be viewed, not as empirical hypotheses, but as "regulative ideals" for natural science. This revision provides a means for responding to the objections that the theses are "utopian", that they suffer from physicalist indeterminacy, that their scope is ill-defined and that they have dubious empirical content. Finally, strategies of defense against the objection that the bases could include non-physical entities are suggested and the outlook for the program is briefly surveyed.

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1. THE PROBLEM OF FORMULATION

The doctrine of physicalism consists of a set of claims concerning, roughly, the nature of reality and the structure of scientific knowledge. Such theses as "Everything is physical", "No difference without a physical difference",¹ "The physical truth determines the whole truth about nature",² and "Theories in science are reducible to physical theory" have been entertained, developed and endorsed by leading philosophers for over forty years. Such claims hold forth the promise of solving a multitude of philosophical and scientific problems in addition to giving expression to a number of deeply entrenched philosophical beliefs; further, the success of the physicalist program in science is thought by its proponents to yield substantial gains in scientific understanding; and, the doctrine is believed to play a major role in the ongoing practice of science and philosophy. In short, if physicalism is a tenable doctrine, then it is an extremely important one.³

¹ The principle of the indiscernibility of physical identicals.

² The principle of physical truth determination.

³ In this work, the following distinctions will be honored:

1. The doctrine of physicalism consists of a set of three types of claim as follows: (a) theses, which are explicit views concerning nature and science, (b) presuppositions, which are claims that are presupposed by the theses, and (c) metatheses, which are claims concerning the theses (e.g., that the theses are empirical).
2. The program of physicalism is the enterprise consisting of:
(a) formulating the doctrine, (b) defending the doctrine

The history of physicalism has not been smooth sailing. Despite its attractiveness, the doctrine has proven difficult to formulate rigorously in such a way as to both avoid the objections of critics and retain its significant content.⁴ Original physicalist formulations (e.g., those of Carnap, Feigl, Hempel) were typically very strong definitional and derivational theses concerning relations between terms, laws and theories of the special sciences and those of physics; and, they were motivated by, inter alia, the verificationist theory of meaning and the view that all substantive claims in philosophy can be couched in linguistic terms. This latter view we shall refer to as "the replacement thesis".

During the time since the original presentation of physicalist doctrine, a number of developments have shown that (1) the original formulations of the theses are untenable and (2) the program as a whole may be doomed to failure.

First, the original formulations are untenable because (a) they are too strong as constraints on the structure of science and (b) they are inadequate as expressions of physicalist views. They are too strong because there are branches of science which fail to satisfy them but

against objections, (c) verifying the doctrine by assessing relevant evidence, and (d) the actual construction in science of a system of knowledge which exhibits the properties described by the theses (this will be referred to as "the working out of the program in science").

⁴ See Field (1972), p. 357, fn. 13, for explicit recognition of this difficulty.

which are, nonetheless, legitimate branches of natural science. Clearly, the strategy behind this objection presupposes that there are ways, independent of specific formulations of physicalism, for distinguishing legitimate from illegitimate branches of science. This is essential for any construal of physicalist theses as empirical; as we shall see below, the supposed methodological role of the theses complicates this situation since many physicalists (e.g., Quine) take their doctrine to formulate criteria for assessing the legitimacy of theories in different branches of science. Despite this complication, it is generally agreed that the strong reductionist claims of the early physicalists are too strong for either correctly describing or constraining theories in science. This is so because, roughly, if an independent criterion of scientific objectivity does exist, then classical reductionism is too strong if there could exist theories in science that are objective in accordance with such a criterion but which fail to satisfy the reductionist theses. Essentially, this has been shown by proponents of a "functionalist" metatheory for current cognitive psychology.⁵

Strong reductionist theses are inadequate expressions of the physicalist position because they could be satisfied while the basic physicalist concerns of a unified ontology and a unified explanatory system would not be satisfied. As we shall see,⁶ liberation from the strong

⁵ See Putnam (1960), Fodor (1965).

⁶ See Chapter 2.

forms of the verificationist theory of meaning, the replacement thesis and classical reductionism has opened up considerable latitude in the kinds of thesis that are available for the expression of physicalism. In particular, adequate formulation of the doctrine will be seen to require linguistic and non-linguistic theses.⁷ Further, less stringent theses, admitting a variety of relations between linguistic or non-linguistic objects in the different branches of science, will be seen to provide more accurate and more plausible formulations of physicalist belief.⁸

Second, and more critical with respect to the survival of the program, there are a number of objections to physicalism which are designed to show that, even if the formulations of the doctrine are modified to accommodate the above described developments, the program is doomed to failure. Chomsky and others have been concerned with diffi-

⁷ A linguistic thesis here is one which quantifies over linguistic objects, as classical definitional and derivational reductive theses do.

⁸ Relaxation of the criteria for relating the terms of the special sciences to those of physics as well as for relating the theories and the objects in the domains of the theories of the special sciences to the theories and objects of physics, will be seen to yield more accurate formulations of the physicalist position as well as more plausible ones; apparently, early physicalists opted for implausibly strong formulations that, even if they had succeeded, would not have expressed what the physicalist wants to say. We note that early physicalists were not entirely in agreement concerning the proper formulation of their position. For example, Quine (1966) and Oppenheim and Putnam (1958) presented versions of the doctrine that signaled the difficulties of the original formulations both in terms of the implausibility of the reductionist theses and the need for explicitly non-linguistic theses.

culties involving the principled identification of "the physical" that is presupposed by any non-trivial formulation of the doctrine: to date, there have been no adequate responses to these concerns.⁹ Further, Goodman and Putnam have contended that physicalism is, at best, in a position comparable to that of the phenomenalist program that preceded it: that is, physicalism is, at best, a program with too many promises of success and too few actual successes.

Despite these developments and difficulties, physicalism, vaguely construed, continues to exert a powerful command over intuition and a strong directive force in science. As a consequence, physicalists are reluctant to abandon their doctrine and, hence, are faced with the task of formulating a version which adequately expresses their views and, yet, which avoids the objections of critics and does not hold out promises that are not likely of fulfillment. Opponents of physicalism, of course, believe that the prospects of successfully completing this task are poor.

A number of questions emerge from this rough overview as very much in need of being answered:

⁹ As we shall see below, it is a presupposition of the theses of physicalism that there be distinctions between physical and non-physical branches of natural science, theories and ontologies that are based upon considerations relevant to the physicalist program (cf., Chapter 3). Opponents hold that any such divisions are based upon factors that are arbitrary with respect to the physicalist program (e.g., socio-historical factors); hence, the theses are thought to be devoid of substantial content.

1. What does an adequate formulation of physicalist doctrine consist in?
2. Can such a formulation survive the objections of the critics?
3. If so, what is the outlook for the physicalist program with respect to (i) the verification of the theses and (ii) the working out of the program in science?

It is the objective of this project to provide answers to these questions.

The plan for proceeding toward this objective is as follows: in the remainder of this chapter, the principle motivations of physicalists will be clarified; it will be seen that physicalists are guided by powerful convictions and by the promise of significant cognitive and non-cognitive gains. Toward the end of the chapter, criteria of adequacy for formulations of physicalism will be stated in terms of how well a formulation expresses physicalist motivations. In Chapter 2, a review of major physicalist proposals of past and present physicalists will be made; it will be seen that all formulations fail to satisfy our criteria of adequacy, and we shall summarize the central reasons for such failures as an aid to avoiding such a problem for our own formulation. In Chapters 3 to 5 we shall put forward a full statement of physicalist doctrine including presuppositions (Ch. 3), theses (Ch. 4) and metatheses (Ch. 5); it will be seen that the theses are adequate by our criteria and that most of the standard objections to the program can be fended off. Finally, strategies of defence against the

most difficult of the objections will be considered and the prospects of the program will be considered. It will be seen that physicalism is vulnerable to the objection that the theses are not acceptable empirical theses of natural science and to the objection mentioned above that the doctrine is hampered by many promisory notes that are unlikely of fulfillment (i.e., physicalism is a "utopian" doctrine.) Proposals will be made for how the metatheory can be revised so as to avoid these objections while retaining the significant content of the theses as well as their important methodological role in science. We will conclude with some remarks concerning the future of the program.

1.1 MOTIVATIONS FOR THE PHYSICALIST PROGRAM

Preliminary to our discussion of the motivations of the physicalist program, two distinctions must be made. First, we distinguish between issues concerning the growth of scientific knowledge and issues concerning the structure of scientific knowledge. The first are highly important issues that have figured centrally in many discussions of physicalist reduction as a form of scientific development. Such issues are not of direct interest to us in the current project; they are orthogonal to the second class of issues that concern the logical and epistemological structure of scientific knowledge at any given stage of development. It is this class of issues with which we shall be directly concerned. For example, we shall be concerned with views about what logical and epistemological relations

between theories in different branches of science ought, ideally, to exist; "ideally" in the sense that a corpus of knowledge exhibiting such structural relations ought to be a goal of scientific activity. Thus, characterization of such an ideal can be viewed as a characterization of a goal state toward which scientific progress ought to be aimed; for someone working within a program accepting such a characterization, the goal state will play a major methodological role in directing inquiry and evaluating developments. In this way, growth issues and structural issues, though distinct, are clearly relevant to each other. In what follows, we shall be concerned primarily with physicalism taken as a doctrine about the structure of science; in the final chapter, we shall discuss how such a doctrine bears on scientific growth.

Second, we distinguish between "unitary science" and "unified science".¹⁰ A view of science as unitary will be taken as a view of science as ideally consisting of a single total theory in one language and embodying a set of basic explanatory principles that are sufficient for explaining all laws and all specific events within the domain of scientific study (i.e., a unitary explanatory system). For a physicalist, unitary science is usually conceived of as physics constituting the total explanatory system of science; on such a view, the physical vocabulary and the explanatory principles couched in that vo-

¹⁰ See Feigl (1963) for a discussion of this distinction; our development differs somewhat from his.

cabulary suffice for all scientific description and explanation. What is essential to this view is that science does not consist of any branch of science other than physics.¹¹ All "special sciences" serve only heuristic purposes that "in principle" could be served by an ideally completed physics. On this view, the idea of structural relations between branches of science has no place; it is the expression of a strong, eliminative physicalist position: i.e., that every special science is eliminable in favor of physics. As a structural position, the ideal of unitary science expresses the view that science is monolithic and embodied completely by physics; as this pertains to growth issues, scientific activity ought to be directed toward the absorption of all explanation into physics and the gradual elimination of the special sciences.¹²

In contrast to unitary science is unified science, the main features of which are: (i) there are principled divisions between branches of science and (ii) there are principles which specify relations that sciences must bear to some basic science (e.g., physics, psychology). Thus, the unification consists, not in the eliminability of all branches in favor of one, but in the kinds of relations that the branches bear to the basic branch. Physicalism, in the spirit of uni-

¹¹ Where branches of science are individuated by such considerations as: canonical vocabulary, problems and questions addressed, patterns of explanation, etc.

¹² See Quine (unpublished), Davidson (1970) and Fodor (1978).

fied, not unitary, science is the topic of the current project.¹³ Thus, in what follows, we shall be concerned with the physicalist program conceived of as a program for unified science and as constituted by a set of structural principles that characterize the unifying relations in scientific knowledge. We now turn to a discussion of the underlying motivations for such a program.

There are, at least, three such kinds of motivation:

1. The doctrine of physicalism serves to express certain deeply entrenched ontological and epistemological convictions of its proponents.
2. The success of the program would result in substantial cognitive gains.
3. The success of the program would result in substantial non-cognitive gains.

We shall consider these in turn.¹⁴

¹³ Such a view does not preclude the possibility that a given branch of science might be eliminable in favor of physics; what the view affirms is that this is not the paramount goal of scientific activity. Rather, as we shall see, such elimination could, in many cases, only result in an undermining of "the aims of inquiry." Further, this view may be better conceived in terms of relations between levels of organization and of theory rather than relations between branches of science; for this to succeed however, the relevant notion of levels must be clarified.

¹⁴ Another motivation would be that there is strong reason to believe that the program will be successful. However, this project will be concerned primarily with the a priori features of the physicalist program; questions of evidence for specific claims or for the overall success of the program will only be briefly touched upon at the end.

Typical formulations of physicalist theses have, either implicitly or explicitly, been motivated to express a certain view of science: viz., that physics occupies a special position relative to all other branches of science. Physicalists who have been explicit in this write as follows:

Mathematical physics, as the most basic and comprehensive of the sciences, occupies a special position with respect to the overall scientific framework. In its loosest sense, physicalism is a recognition of this special position.¹⁵

It [the claim that there is no mental difference without a physical difference] is a way of saying that the fundamental objects are the physical objects. It accords physics its rightful place as the basic natural science without venturing any dubious hopes of reduction of other disciplines...¹⁶

This second thesis of physicalism claims that the facts and laws of the natural and social sciences can all be derived - at least in principle - from the theoretical assumptions of physics. We may formulate this second thesis as the belief in the possibility of a unitary explanatory system.¹⁷

A review of physicalist writings reveals that there is considerable variety in the kinds of relations which are thought to hold between physics and other scientific disciplines in virtue of this special place of physics in the structure of science. Physicalist theses have typically characterized those relations and have been concerned with, at least, three areas: ontology, objectivity, and explanation.¹⁸

¹⁵ See Hellman and Thompson (1975), p. 551.

¹⁶ See Quine (1979), p. 163.

¹⁷ See Feigl (1963), p. 227.

¹⁸ Physicalist theses have often been formulated in terms of formal relations between linguistic objects in different branches of science. But, in most cases, it has clearly been the intent of the

First, with respect to ontology, the physicalist holds that the ontology of physical theory is sufficient to incorporate the ontology of any other branch of science. As a result, physicalism rules out "ontologically independent" objects or attributes (i.e., objects or attributes that fall outside of the physicalist ontology). Such entities as spirits and entelechies or vital forces are unacceptable posits to the physicalist because they are entities introduced for an explanatory purpose but do not fall within the physical ontology. All physicalists appear to be in agreement with respect to the fundamental ontological concerns of the program, although we shall see that there are many different ways of construing these concerns.

Second, several physicalists¹⁹ have been concerned with the conditions of objective knowledge; that is, they have attempted to formulate principles which characterize conditions under which there is an objective fact of the matter in some domain of inquiry.²⁰ The theses

proponents of such theses to be expressing claims (or the equivalent of claims) concerning the above areas. Our concern in this section is with the basic interests of physicalism rather than with issues of how best to express theses that serve those interests. See Feigl (1963), Nagel (1961), Hellman & Thompson (1975), Boyd (unpublished), Fodor (1975) for explicit discussion about how best to express physicalist concerns.

¹⁹ For example Quine, Friedman, Hellman and Thompson.

²⁰ The notion of "objectivity" within science is, at best, a partially analyzed one. Although emphasis in the philosophical literature has focused upon inter-subjective testability as a condition of objective knowledge, this is not sufficient. Physicalists have focused on the "factual basis" required for objective knowledge and have made proposals concerning necessary conditions for objective knowledge in terms of relations to a physical basis of fact or truth.

of physical truth determination and of the indiscernibility of physical identicals formulate, respectively, a condition for objective truth which all claims to objective knowledge must satisfy and a condition for objective difference in some respect between any two objects. Within a physicalist view of science, all claims to objective truth and all putative similarities and differences must be correctly related to physical truth and physical similarities and differences.²¹ Although not all physicalists have been explicitly concerned with questions of objectivity, no physicalist would deny either of the principles mentioned above.

Finally, physicalists have, until recently, been quite explicitly concerned with explanation in science. The physicalist's view has been that science should be a unified explanatory system in which branches of science are organized hierarchically with physics as the basic science, and, in which the laws and individual phenomena at each level in the hierarchy are explainable in terms of the laws and individual phenomena at lower levels.²² With the rejection of classical reduction-

Our approach assumes that there are, at least, these two components (i.e., inter-subjectivity and factuality) to an adequate analysis of objectivity. We observe that there are notorious difficulties involved in making such an assumption, since the idea of a "factual basis" needs clarification. It is this that physicalists concerned with objectivity have sought to provide.

²¹ An example of employment of such principles is Quine's discussions of "indeterminacy" in linguistics and psychology.

²² Versions of this picture have been provided by Oppenheim & Putnam (1958), Causey (1977), Feigl (1963), Hempel & Oppenheim (1953), and Nagel (1961).

ism,²³ these explanatory concerns of physicalism have tended to be minimized or overlooked or to be judged as "utopian." In the interest of presenting a plausible doctrine many physicalists²⁴ have advanced versions of the doctrine that eschew explanatory concerns and are primarily concerned with ontology or objectivity. An emerging issue which we mention now and discuss later in this section is whether or not explanatory concerns are an integral part of the physicalist program, and hence, whether or not any adequate formulation of physicalist doctrine must address itself to such concerns.²⁵

So far, we have seen that the physicalist view of science involves acknowledgement of physics as occupying a special position in the structure of scientific knowledge; that special position consists in being most basic and comprehensive with respect to ontology, objectivity and explanation. As a result, formulations of physicalist theses have been typically designed to accord physics its special position by characterizing the relations that ought to hold between it and the other sciences in virtue of that position. Now, bypassing for the moment questions of how best to formulate these theses, it might be

²³ See Fodor (1975, 1978), Hellman & Thompson (1975), Boyd (unpublished), for arguments. See below, Chapter 2.

²⁴ For example, Davidson, Boyd, Quine, Fodor, and Hellman & Thompson.

²⁵ This issue is significant with respect to a number of questions: (1) what formulations are adequate?, (2) how much room for retreat does physicalism have when faced with objections?, (3) when studying inter-theoretic relations in science, what questions ought to be addressed and what are the constraints on answers to those questions?

asked, "Why should physics be accorded this special position in science?", or alternatively, "What motivates the physicalist view of science?".

We note that some motivation for the physicalist view of science originates from within science itself and will be discussed later in the sections on the cognitive and non-cognitive gains of the physicalist program. But, in addition to explicitly scientific concerns, physicalists have revealed a philosophical bias in their program which influences their conception of science. At a minimum, an understanding of this bias will give us some insight into the powerful grip that physicalism exerts on intuition.

The philosophical bias of the physicalist program involves a certain view of nature expressed in such passages as the following:

He [the physicalist] is content to declare bodies to be fundamental to nature in somewhat this sense: there is no difference in the world without a difference in the positions or states of bodies.²⁶

This view of nature, that bodies are fundamental to nature, requires some unpacking. There are two components of the view: the first concerns what exists and is an assertion of the classical materialist view that matter, or bodies, and states of matter are what exist in nature.²⁷ Further, this ontological picture involves a set of "basic"

²⁶ See Quine (1979), p. 162.

²⁷ Many contemporary physicalists have, with the developments in modern physics, dropped talk of matter or bodies in formal presentations of their doctrine. The ontological basis for physicalism is

material states and a set of "basic" material objects out of which all complex material states and objects are composed. The second component of this view of nature is a claim about determination: all attributes of matter (bodies) are determined by the basic material states. Once the basic material states are fixed all other states or attributes are fixed. The fundamental intuition lying behind this view of nature may be expressed as follows: it is in virtue of what something is made of (i.e., the fundamental constituents of matter and their attributes) and how it is put together (i.e., the arrangement of the basic constituents) that it has all of the other attributes it does.

The above described view of nature, or variants of it, is very much at the core of the physicalist program. We have described it as a philosophical bias of the program because, although it influences an approach to the conduct of science, its significance transcends science. More specifically, this view is an assertion of "the matter dependence of mind" and as such bears directly on the classical disputes between idealist and materialist forms of realism. An opposed view, "the mind dependence of matter", would undercut the primacy of matter in our view of nature and replace it with mind (e.g., mental activity) as the determinant of what matter is like.²⁸ For our current purposes,

not seen as being populated by objects, bodies or matter as we ordinarily conceive of these. In less formal discussions, the more familiar terms are still employed. See below, Chapter 4, for more formal development of the physicalist ontological position.

²⁸ An alternative to these positions is one advocated by Goodman which affirms the value of both approach while denying each its claim to

we must only see that the physicalist, in being committed to the matter dependence of mind, holds that whatever the properties of mind, especially whatever properties it has concerning knowing and representing nature, they are all determined by matter and its states. The strong alternative view would deny matter this special place by claiming that matter has that place only in a representation that is created and changeable by the mind; hence, mind occupies a position of primacy with respect to nature. The physicalist, of course, has a predictable reply to this.

The bearing of this dispute on the structure of science is clear: if one affirms the matter dependence of mind, then fairly rigid constraints on the scientific representation of mind and its place in nature (especially its relations to physical phenomena) are imposed; if one denies such dependence, then the structure of a scientific representation is constrained in a quite different way (e.g., the converse of the principle that there can be no mental difference without a physical difference might be endorsed). It should be clear that one cannot resolve the philosophical dispute by studiously watching the

primacy. Essentially, the recent dispute between Quine and Goodman involves the "monopolistic" character of Quine's physicalism. See Quine (1978), Goodman (1979), and Putnam (1979). It should be kept in mind that a denial of the matter dependence of mind need not consist in an affirmation of the mind dependence of matter; if, for example, some mental states do not depend on matter, that is sufficient for rejection of the matter dependence of mind as a general view about the relation between mind and matter. The physicalist, of course, denies that there are any mental states that are independent of the physical (matter).

progress of science, since how science is conducted depends directly upon what position one takes with respect to the question at issue. Insofar as science currently proceeds in accordance with an assumption of the matter dependence of mind, it is exhibiting a philosophical bias; and, the physicalistic view of science is an explicit affirmation of that bias.²⁹

At this point, one might ask, "Why accept this view of nature?". As we have suggested, the evidence for this view to be gleaned from the progress of science is of questionable value. The philosophical arguments in support of either side of the dispute are stunningly unconvincing to opponents. Perhaps the situation is, as Goodman might put it, "less like arguing than like selling". That is, to adopt one or the other of the two philosophical positions is to be justified more in terms of what such an adoption produces by way of achieving certain goals than by explicit arguments for its truth. It was essentially this approach that we adopted in this project when we proposed to evaluate formulations of physicalism in terms of how well they realize the motivations of the program.

²⁹ This way of putting things glosses over the complex and important issues concerning the empirical status of physicalist principles and their methodological role in science. That the principles are often conceived of in both of these ways complicates the construal of the physicalist's view of nature as a bias. Our view is that, ultimately, physicalism does embody such a bias, but that such a bias is not arbitrarily adopted and that empirical considerations bear on such an adoption. See Chapter 5 for further discussion.

The impact of the above described view of nature upon science is fairly straightforward. Insofar as it is a goal of science to provide an adequate representation of nature, then the materialist conception of nature leads to a view of science in which the ontology is a material (or, physical) one and all the attributes of material objects are determined by the basic attributes of matter: hence, the physical determination of (i) truth and (ii) sameness and difference. Further, the view of nature suggests the idea that explanation of any phenomenon, if possible at all, should be possible in terms of the basic constituents and their attributes. In short, for the physicalist, his view of science (i.e., that physics occupies a special position in science with respect to ontology, objectivity and explanation) is influenced by his view of nature.

To summarize, the first motivation of the physicalist program is to express two related views: (1) a view of nature which constitutes the adoption of a fundamental philosophical stance concerning the primacy of matter and (2) a view of science which is partly informed by the view of nature and which asserts the special position of physics in relation to all other sciences. As we have noted above, however, the physicalist's view of science is not only motivated by his philosophy; rather, there are independent reasons originating from within science that, at least, reaffirm that view. Further, those same considerations constitute additional justification for the physicalist program in the sense of characterizing the gains of adopting such a program. It is to those "cognitive" and "non-cognitive" gains that we now turn.

The second type of motivation for the physicalist program involves the "cognitive gains" that would result if the program were to be successful. By a cognitive gain is meant some improvement in our corpus of knowledge and consequent understanding; and, there are at least two ways in which the success of the physicalist program would result in cognitive gain: the first involves solution of specific philosophical and scientific problems, and the second involves improvements in some of the global features of our knowledge (e.g., consistency, parsimony, simplicity, explanatory power).

Physicalists have always seen advantages in their approach resulting from solutions it would provide to problems in such areas as: (i) mental phenomena, (ii) moral and aesthetic phenomena, (iii) biological phenomena, (iv) semantical phenomena, (v) social phenomena, and (vi) the theories of each of these. The physicalist program, if successful, would show how each of (i)-(v), insofar as they are real and objective, fit into a scientific conception of the natural order; and, it would provide a basis for resolving foundational questions concerning theories of each. An additional problem that would be partially solved concerns the delimitation of the boundaries of objective knowledge within science; that is, boundaries between knowledge that concerns matters of fact and knowledge that involves a component of subjectivity, or relativity to the knowing subject. The principles of physical truth determination and of the indiscernibility of physical identicals are candidates for principles that would serve this purpose. As we

shall see below, they have played a central role in recent physicalist thought concerning the distinction between objective and non-objective branches of knowledge (e.g., Quine's concern about the lack of objective status of linguistics and psychology.)

The second kind of cognitive gain that would result if the physicalist program were to be successful involves global improvements in our corpus of knowledge. There are three such kinds of improvement that have interested physicalists: (i) ontological parsimony, (ii) unification of the overall scientific explanatory system and a resulting increase in simplicity and consistency of our knowledge, and (iii) a substantial increase in our understanding of nature.

There appears to be a consensus among physicalists that the program is motivated by ontological concerns; and, inter alia, those concerns involve a certain kind of parsimony: viz., that of not exceeding the ontology of physical theory for any scientific purpose. That is, with respect to the ontology of science, nothing more need be posited than what is included in the domain of physics. From an ontological point of view, therefore, there is only one kind of entity in nature: physicalism is a brand of monism. Essentially, this monism requires that every individual in nature is either a basic physical constituent or a complex physical entity composed of basic constituents. Further, every class of entities in nature is a class of physical entities. And finally, every attribute that occurs in nature is an attribute of a

physical entity and is realized³⁰ by the physical attributes of such entities. In short, everything in nature is physical.³¹

To better understand what kind of parsimony this is, it is important to see what it is not: it is neither a parsimony of theoretical kinds of entity nor a parsimony of kinds of attribute nor a parsimony of the individuals in nature. That is, ontological parsimony does not put any restrictions on the groupings of individuals for the purposes of theoretical inquiry; nor does it entail that entities in nature must only have physical attributes rather than possessing more abstract attributes as well.³² Hence, this ontological parsimony does not involve any restrictions on what we can say about objects in nature; it only requires that the things we say something about be physical things.³³ Finally, there is no suggestion in this parsimony that

³⁰ Below (Chapter 4) we shall discuss what "realization" is. We use this notion because (1) there are good reasons for not adopting the very strong physicalist position that every attribute is a physical attribute and (2) it captures a significant sense in which the ontology of nature does not exceed the physical ontology.

³¹ Not every physicalist would state his position this way of course; stronger versions would have it that all attributes are physical attributes as well. Because, on our view, this is patently false, weaker forms of physicalism must be explored.

³² Such attributes must, according to the physicalist, be related in specific ways (e.g., by a realization relation) to physical attributes however.

³³ See Quine (1951) and Hellman and Thompson (1977) concerning the distinction between ideology and ontology; also, see Chapter 3 below. The amount of independence between what the physicalist believes to exist and what he believes to be constraints upon what we can say about what exists has been a critical issue throughout the history of the program.

there be a reduction in the number of individuals in nature, except insofar as is entailed by the inclusion of the individual domains of each branch of science within the domain of physics.

Having said what ontological parsimony does and does not consist in for the physicalist, we now ask, "Why is this ontological parsimony a cognitive gain?". Of what value is ontological parsimony that it should be a positive gain if realized by the physicalist program?

Let us begin by pointing out that ontological parsimony is not intrinsically a significant cognitive goal; alone, it is at best an aesthetic property of a system. However, ontological parsimony is an important property of a system insofar as it contributes to an increase in the understanding provided by the system. There are at least two ways that ontological parsimony can contribute to understanding: first, by leading to a decrease in the number of mysteries in nature; such reduction can come about by there being fewer "fundamental" mysteries (i.e., those which are not explainable within a given explanatory system because they are basic) and by there being a basis for explaining higher level phenomena not previously understood. In the case of physicalism, one of the attractive features it shares with its ancestors (i.e., materialism and mechanism) is that it promises to provide a basis for solving the problem of mind-body causal interaction. If interaction at the level of physics is well understood in terms of some class of basic processes and interactions, and if all

interaction at other levels can be related to physical interaction, then there are no mysterious interactions in nature in addition to the basic ones.³⁴ Such an approach decreases the number of mysteries because it avoids positing "special" interactive forces between mind and body and it avoids leaving the interaction unexplained or denied.³⁵

A second way in which ontological parsimony can contribute to increasing understanding is by leading to integration of an associated explanatory system. Essentially, a parsimonious basis creates the possibility for explaining more and more phenomena with a single class of fundamental objects, processes and attributes. Notice that, although ontological parsimony creates such a possibility, by itself it cannot guarantee actualization: the existence of an explanatory system with certain properties is an independent requirement for attaining such increases in understanding. Ontological parsimony of the kind advocated here need not lead to increases in understanding if, for example, non-physical attributes of physical systems cannot be explained in terms of physical attributes of those systems. In such a case, the de-

³⁴ Two proposals consonant with physicalism for solving this problem are (1) the token identity of mental events, states and processes with physical events, states and processes and (2) the "supervenience" of the mental upon the physical. See Kim (1979) for a lucid discussion of this latter proposal as it bears on issues of causation.

³⁵ A burden of the physicalist position involves showing that it is not trivially satisfied by locating all mysteries in the domain of physics; that is, showing that it does not avoid proliferation of ontological kinds by simply increasing the number of theoretical kinds in physics.

crease in fundamental mysteries secured by a parsimonious basis is offset by the existence of higher level mysteries comparable to the mystery of mind-body interaction. This is possible even if it is claimed that all instances of non-physical attributes are realized by instances of physical attributes.³⁶

The second type of global improvement that has been sought by physicalists is unification and simplification of the overall explanatory system in science. Because such a goal has always been associated with classical definitional and derivational reductionist programs, it is important to see that those programs were only one way of attempting to unify and simplify the scientific explanatory system. With the demise of those programs, it is incumbent upon physicalists to understand what the goals of those programs were and to seek new ways of achieving those goals. In the case of unification and simplification (as with parsimony) the fundamental motivation for trying to achieve them is to effect increases in understanding by reducing the number of unexplained phenomena and laws in science and by increasing the explanatory power of explaining laws. By subsuming more and more phenomena and explanatory principles under quite general principles, the explanatory power of the latter is, in general, thereby increased.³⁷ It is

³⁶ One of the key explanatory questions involved here, which drives much physicalist thought is, roughly, "In virtue of what do certain physical attributes realize certain non-physical attributes?". See below (Chapter 4) for further discussion.

³⁷ See Fodor (1978) for an important discussion of how this strategy can fail. Also, see Kemeny and Oppenheim (1956) for a discussion of

an important goal of the physicalist program to subsume as many phenomena and explanatory laws as possible under basic physical laws; the problem besetting contemporary physicalists is how to conceive of such a pursuit of unification in science without running into the difficulties encountered by classical reductionism.³⁸

Parsimony of ontology and simplicity and unity of the scientific explanatory system have been seen to be cognitive gains of the physicalist program because they contribute to increases in the understanding provided by that system. We now turn to explicit discussion of increasing understanding as the fundamental cognitive gain that would result if the physicalist program were to be successful.

Essentially, 'understanding is gained in science, at least, insofar as (i) there is a decrease in the number of fundamental mysteries, (ii) there is a decrease in the number of total mysteries throughout the scientific system, and (iii) the explaining principles have more and more explanatory power. The physicalist program can lead to such understanding through ontological parsimony (which can have the effect of reducing fundamental and total mysteries) and through unification (which can lead to reductions in the total number of mysteries and in-

the notion of "explanatory power".

³⁸ Nancy Cartwright has further suggested that a principle motivation of derivational reductive programs was to ensure the consistency of the system of scientific knowledge; as with simplicity and unification, this goal can be achieved in other ways. See Cartwright (1979).

creases in the explanatory power of explaining principles). Further, physicalism leads to understanding by epitomizing a standard practice in science of seeking to explain the properties and behaviors of complex phenomena in terms of the properties and behaviors of simpler phenomena of which they are composed.³⁹ Such patterns of explanation can achieve increases in understanding based upon all of (i)-(iii) insofar as successes decrease the need for positing new basic phenomena, lead to answers to questions concerning complex phenomena (thereby reducing the total number of mysteries), and tend toward unification of the total system (thereby increasing the explanatory power of the explaining principles). Hence, proposals that physicalism be taken to require "microreduction" of one branch of science to another, or that physicalism involves explanation of abstract properties of entities in terms of their physical realization, or that physicalism involves explanation of regularities in one branch in terms of underlying physical mechanisms are all construals of the physicalist program explicitly involving explanatory concerns of the kind we are considering that lead to increases in understanding. The issue, mentioned earlier, to which we now turn, is whether formulations of the physicalist program that do not explicitly address these concerns are adequate formulations.

³⁹ Thus, Niels Bohr (1961) has described the essence of scientific explanation as consisting in explaining the complex in terms of the simple; and, Oppenheim and Putnam (1958) have assimilated their version of the physicalist program to "The Democritean Tendency" in science to explain a wide range of diverse phenomena in terms of a few basic phenomena.

By an adequate formulation of physicalist doctrine we mean one that is such that, if the program based upon that formulation is successful, then the motivations of the program will have been served. We shall argue that a formulation that does not explicitly address explanatory concerns is not adequate: i.e., that it is possible that a program based upon such a formulation could be successful and yet the motivations of the program not be served.⁴⁰

Of the three traditional areas of physicalist concern (i.e., ontology, objectivity and explanation), explanation has been neglected in recent physicalist writings in favor of the other two. Our present claim is that formulations of physicalist doctrine that are exclusively concerned with ontology and objectivity are not adequate because the success of a program based upon such formulations is compatible with a failure to increase understanding. Our argument for this claim is that formulations concerned with only ontology and objectivity provide no guarantee that the success of the program will not lead to a body of scientific knowledge that is permeated by mystery and lacking in unity. The point here is that success of a physicalist program concerned with only ontology and objectivity could make no substantial

⁴⁰ We remind the reader that rejection of classical reductionism (construed as definitional and derivational reduction of syntactic objects) does not entail the rejection of the explanatory concerns that motivated that program. Hence, one should not jump to any conclusions about the feasibility of a physicalist program with explicit explanatory concerns. What we now know is that, if we have explanatory concerns, we should not try to formulate them in strong, purely syntactic terms.

difference to the understanding provided by a system of scientific knowledge. This is because only by developing certain types of explanation is it possible to rid a scientific representation of nature of a host of "vertical" mysteries: such as mysterious non-physical attributes of physical objects and the existence of mysterious non-physical regularities and their exceptions. In addition, if objectivity is expressed in terms of the principle of physical truth determination, in the absence of explanatory connections between different branches of science, such determination is an additional mystery. Such a formulation of physicalism exerts little influence on the structure of the explanatory system in science.⁴¹ In a nutshell, for the success of the physicalist program to guarantee satisfaction of all of the fundamental motivations of physicalists, explanatory unification of scientific knowledge is required. Therefore, we shall adopt the view that explicit explanatory theses are required for an adequate formulation of physicalist doctrine. Such a requirement bears directly on the evaluation of recent moves made by physicalists to avoid objections; specifically, any reformulation of the doctrine designed to avoid objections but which weakens the formulation so as to not address explanatory

⁴¹ Hellman and Thompson are sensitive to the problem that a purely ontological formulation of physicalism is not adequate; the point here is that what they augment the doctrine with (i.e., physical truth and reference determination) does not substantially improve the situation regarding the explanatory motivations of the program. Classical reductionism, on the other hand, bolstered by the D-N model of explanation, purported to address the explanatory motivations of the program. As we shall see below, the shortcomings of the D-N model undermine this effort.

concerns will be avoiding the objections at the cost of the adequacy of the formulation; our view is that this is too high a price to pay.⁴²

The third class of motivations for the physicalist program involves "non-cognitive" gains that would result if the program were to be successful. By a non-cognitive gain is meant some advantage that would result for the behavior of humans; and, the primary advantage of this kind involves the role of physicalist doctrine in the conduct of scientific inquiry. As we shall discuss below, physicalist principles can potentially contribute to scientific practice in two ways: (i) by suggesting and providing some definition to a wide class of research questions, and (ii) by providing a basis for evaluating the acceptability of scientific theories (e.g., by placing constraints upon formulations in physics and the special sciences).⁴³

⁴² The failure of adequacy is, of course, relative to our criteria; it is surely appropriate to formulate weaker versions of the program. Our main point is that, in doing so, one should be aware of the differences in motivations achieved: our criteria serve the expository purpose of affirming a certain set of motivations as highly desirable. Weaker formulations fulfill fewer of these motivations thereby lessening the worth of the program. The general maxim we believe ought to be observed in the design of any system of knowledge is to opt for the maximum criteria of adequacy possible; if unavoidable difficulties are encountered, then (and only then) ought the criteria be weakened.

⁴³ As we shall see, there is some difficulty in determining what degree of abstraction is required in formulating such constraints. History has taught us that strong syntactic constraints are not abstract enough in that they fail to allow for variability in the forms of scientific theories and in the kinds of relations between higher and lower level theories. As we hope to show, while syntactic constraints are not sufficiently abstract, explicit ontologi-

1.2 CRITERIA OF ADEQUACY AND ACCEPTABILITY

Given the discussion of physicalist motivations in the last section, the objective of this section is to formulate criteria of adequacy and acceptability for formulations of physicalist doctrine which will be employed in subsequent chapters as a basis for evaluating specific physicalist proposals. By the adequacy of a formulation we mean whether or not it expresses the motivations of the program; our approach to understanding the formulation problem confronting physicalists is that there are a number of reasonably well defined objectives which physicalists seek after and which the physicalist program should be designed to reach. Hence, any formulation of the doctrine should have the property of being such that if the program based upon that formulation is successful, then the principal objectives of the program will be reached.⁴⁴ Therefore, the criteria that a formulation of physicalism must satisfy include:

1. It must express the physicalist's view of nature and science.⁴⁵

cal, determinatist and explanatory constraints are. However, syntactic constraints had the virtue of being actually employable in practice; it is not so clear that more abstract constraints will have this property.

⁴⁴ This strategic approach has not been explicitly employed by many physicalists (much to the detriment of the program); exceptions include Fodor, Hellman and Thompson and the adamantly non-physicalist Nelson Goodman (in his discussion of systems building in general).

⁴⁵ I.e., that nature consists of physical systems and that all attributes of such systems are determined by basic physical attributes; and that science reflects this view of nature by according to physics a special place in the structure of scientific knowledge.

2. It must promise to realize the cognitive gains of ontological parsimony, theoretical unity, consistency, simplicity, increased scientific understanding and provision of solutions to specific scientific and philosophical problems.
3. It must provide usable principles for directing scientific inquiry.

These criteria will provide a measure of adequacy for formulations of physicalist doctrine; clearly, a formulation that satisfies all of them will be of more potential importance than one which does not. Further, failure to satisfy any one of them, on the view taken in this project, raises serious questions about the value the physicalist program as formulated; and hence, the justifiability of pursuing the program, especially in the face of objections, is suspect. Unless a formulation satisfies all of the criteria, we shall judge it to be inadequate and in need of replacement because, although such formulations may be easier to establish than stronger ones, they are not expressive of the deeper motivations behind the program.

Once the question of adequacy of a formulation is settled, the question of its acceptability arises. By an acceptable formulation is meant one which is (i) adequate, (ii) immune from the objections of critics, and (iii) plausible on the basis of relevant evidence. The current project will focus on the issues associated with formulating an adequate version of physicalism and on the objections of critics to

adequate formulations. The question of plausibility on the basis of relevant evidence will not be discussed because, on our view, the empirical situation is not sufficiently clear to make such an evaluation. In the last chapter, we shall briefly discuss a program of research which would eventually issue in a sufficiently clear body of relevant evidence to evaluate the plausibility of the program.

2. A REVIEW OF PAST FORMULATIONS

The objective of this chapter is to review past formulations of physicalist doctrine and to explore, relative to our criteria of adequacy, the reasons for their failure. Preliminary to this will be a discussion of the general formulation problem with respect to physicalist doctrine.

Essentially, the formulation problem facing us is to develop a set of theses which are jointly sufficient for (1) expressing the physicalist's views of nature and science, (2) realizing the cognitive gains of the program if it is successful, and (3) serving as usable and fruitful principles for guiding scientific inquiry. What, then, is a plausible strategy for solving this problem?

In answering this question, we shall begin by discussing the general character of physicalist theses and the ways in which they can vary. Typical formulations include the following:

1. All scientific terms are definable in physical terms.
2. All scientific theories are derivable from physical theory.
3. Every event is a physical event.
4. The physical truths determine all the truths about nature.
5. The laws of the special sciences are explainable by the laws of physics.

The general form of such theses may be construed as involving a relation between two classes of objects. Hence, there are, at least, two

ways in which such theses can vary: (i) they can vary with respect to the kinds of objects that are members of the two classes, and (ii) they can vary with respect to the character of the relation between the two classes.

Concerning the kinds of objects that are members of the classes, there are two general categories of interest: linguistic and non-linguistic. Each of these categories further sub-divides into the various kinds of linguistic object (e.g., terms, sentences, theories)¹ and the various kinds of non-linguistic object (e.g., events, states, properties). Further, the objects in the two classes may vary in what we shall call their "type" (e.g., mental, physical). It is a characteristic of all physicalist theses that for each kind of linguistic or non-linguistic object adverted to there is a special class of objects which is designated as being of the physical type and which occupies a special place in relation to all other objects of that kind. Each of these classes of physical object will be called a "base class" and their properties will be discussed more fully in the next chapter.

Concerning the kinds of relations between classes, we shall distinguish two general categories:

¹ The idea that a theory or an explanation is a linguistic object is controversial at best; to sidestep debate we shall assume that they are not and that it is only formulations of theories or explanations that are relevant to the physicalist; such formulations we take to be linguistic in character.

1. a reductive relation between two classes involves a mapping from the members of one class to the members of the other;
2. a non-reductive relation does not involve such a mapping, but rather, involves only preservation of global features of the two classes (e.g., degree of explanatory power, simplicity, unity).

For each of these two general categories of relation there is considerable variability possible concerning the specific relations that may be instances of them. Such variation depends upon the type of constraints that are imposed upon reductive and non-reductive relations between classes; for example, as Goodman has discussed at length,² the criteria of definition (a reductive relation between classes of terms) may vary between 'intensional equivalence, extensional equivalence, extensional isomorphism, etc.

Thus, there are two general dimensions along which physicalist theses can be arranged: kind of entity in the classes and kind of relation between the classes.³ Hence, there are, at least, four general

² See Goodman (1977), Chapter 1.

³ Our introduction of a taxonomy of physicalist theses based upon kinds of entity and kinds of relation is motivated by several considerations: (i) it will help in clarifying the logical relations between different theses, (ii) it provides a structure in which the wide diversity of physicalist views that have appeared in the literature can be located and assessed relative to one another, (iii) it highlights the degrees of freedom in the formulation of physicalist theses and emphasizes the fact that what choices are made at these points are crucial for formulating theses that will successfully serve the purposes motivating the physicalist. As we shall see, there is a history of poor choices that have led to inadequate for-

kinds of physicalist thesis that are possible:

1. linguistic-reductive: e.g., every theory in the special sciences is derivable from physical theory (plus bridge principles);
2. linguistic-non-reductive: e.g., every theory in the special sciences is reducible (in the Kemeny and Oppenheim sense⁴ of 'reducible') to physical theory;
3. non-linguistic-reductive: e.g., every event is a physical event;
4. non-linguistic-non-reductive: e.g., the physical facts determine all the facts of nature.⁵

With respect to the formulation problem for physicalism, the choice of what specific kinds of object and of what specific kinds of relation are to be employed in formulating physicalist theses is made on the basis of what will serve best for satisfying the criteria of adequacy, and with an eye to acceptability, what will serve best in avoiding certain objections and difficulties. That is, solution of the

mulations.

⁴ See Kemeny and Oppenheim (1956) for details of their view of theory reduction. Their use of 'reduction' and its cognates is different from that found in this project. The usage here will always be according to the technical sense described above unless otherwise indicated.

⁵ Jerry Samet has pointed out that the above four kinds of thesis are the "pure cases"; there are hybrid theses that are possible (e.g., the special sciences are about the same things that physics is about). Our discussion will be organized around the pure cases; hybrids will be discussed where appropriate.

formulation problem comes down to making a set of choices, based upon physicalist motivations and interests, among specific instances of the various kinds of physicalist theses: to arrive at an adequate formulation, what one wants to express and what goals one wants to achieve will play a major role in choosing the kind of objects and the kind of relations that constitute the theses that are formulated.⁶

We now shall review a number of past formulations of physicalist doctrine and exhibit their shortcomings. The following conclusion will be drawn from this review: that formulations of physicalist doctrine must not consist of only linguistic or only non-linguistic or only non-reductive theses (i.e., the doctrine must be expressed by a combination of linguistic, non-linguistic and reductive theses, although some non-reductive theses could also be included). The reasons for this will be explored as we proceed; roughly, the main reason is that physicalism is a doctrine concerning both the formal system of science and its intended interpretation; and, because most interesting theses concerning the one are not equivalent to nor entailed by theses concerning the other, separate theses are required if the full content of the doctrine is to be expressed.

⁶ Despite the obviousness of these remarks, they are in need of saying in order to partially arrest the drifting away from basic goals that has characterized recent physicalist thought.

To establish the claim that an adequate formulation of physicalism must include linguistic, non-linguistic and reductive theses, we shall proceed by cases and show that formulations that consist exclusively of linguistic or non-linguistic or non-reductive theses fail to satisfy the criteria of adequacy.

2.1 PURELY LINGUISTIC FORMULATIONS

Clearly, an exhaustive discussion of all possible formulations of physicalist theses is too formidable a task; hence, our approach will be to consider some central cases. For linguistic formulations we shall discuss: (i) theses concerning the definability of terms as expressions of physicalist ontological concerns, (ii) theses concerning semantic relations between theories or formal relations between terms as expressions of physicalist theses concerning objectivity, and (iii) theses concerning derivation or truth determination as expressions of physicalist explanatory concerns. It will be seen that in each of these cases the indicated physicalist concerns are not adequately expressed by the theses put forward. Further, a diagnosis of these failures will reveal that they are based upon properties shared by all members of the class of purely linguistic theses; hence, we shall conclude that purely linguistic theses are inevitably not adequate as expressions of the complete physicalist doctrine. It is, of course, left open whether linguistic theses are components of an adequate formulation.

The idea that a purely linguistic thesis is sufficient to express the physicalist's views about ontology dates back to Carnap's program of replacing all substantive philosophical claims with claims concerning language (the so-called "formal mode"). What we shall now see is that this program, at least as applied to the physicalist program in science, is seriously misguided.⁷

The physicalist ontological position is, roughly stated, that everything is physical; in science, this amounts to saying that the ontology of every branch is included in the ontology of physics. Since ontology concerns individuals, classes and attributes, the physicalist holds (roughly) that the ontology of physics includes all individuals, classes and attributes that are in the domains of all other branches of science. This has seemed to many physicalists as expressible by the claim that every term used in science is definable⁸ in the vocabulary of physics.⁹

⁷ See Carnap (1934, 1967a) for discussion of the proposal.

⁸ The criteria of definability can vary; we shall assume that the criterion relevant to the physicalist is nomological, extensional equivalence. If it were stronger than this, it would be hard to see how physicalism could provide a true definitional thesis. Observe that such a criterion is too weak for full expression of attribute identity.

⁹ What it means for the attributes adverted to in other branches of science to be included in the ontology of physics is not yet clear. The possibilities that are available include: (i) all attributes are physical attributes, (ii) all instances of attributes are identical to instances of physical attributes, (iii) all instances of a given attribute are realized by instances of one specific physical attribute, (iv) all instances of a given attribute are realized by instances of some physical attribute or other. The issue facing the

Now, this view that the physicalist ontological position can be expressed by a claim concerning the definability of terms in the physical vocabulary presupposes the following equivalence:

(R) Each thing is physical if and only if it is such that there is a term that refers to it and that term is definable in physical terms.

Because we are concerned to distinguish individuals, classes and attributes when considering the issues here, we shall resolve (R) into three separate claims as follows:

(RI) Each individual is physical if and only if it is such that there is a term that refers to it and that term is definable in physical terms.

(RC) Each class is physical if and only if it is such that there is a term that refers to it and that term is definable in physical terms.

(RA) Each attribute is physical if and only if it is such that there is a term that expresses it and that term is definable in physical terms.

Each of these claims has a form that can be represented in the following way:

(RF) $(x)[(Px) \text{ iff } (Et)(Rtx \ \& \ (Ep)(Dtp))]$

ontological physicalist with respect to attributes is serious; if, as is plausible, a strong identity thesis like (i) is false, how can the physicalist save face while acknowledging "property dualism"? However, this issue is orthogonal to the problems with linguistic formulations.

where

'x' is a variable ranging over individuals, classes, or
attributes

'P' stands for 'is physical'

't' is a variable ranging over terms

'p' is a variable ranging over physical terms

'D' stands for 'is definable by'

'R' stands for either 'refers to' or 'expresses'

depending upon whether the variable ranges over
individuals, classes or attributes.

To demonstrate the inadequacy of linguistic reductionism as an expression of the physicalist ontological position, it will be shown that the above equivalences fail to hold in both directions. That is, the following two claims will be shown:

(L/R) that everything (individuals, classes, attributes) could be physical and yet there not be a physically definable term referring to/expressing each thing

(R/L) that everything (individual, class, attribute) could be referred to/expressed by a physically definable term and yet not everything would be physical.

We shall consider arguments for these two claims in turn.

The strategy behind existing arguments for (L/R) has been to show that the ontology of physics cannot be specified by a system of defi-

nitions, and hence, the physicalist ontological claim that everything is physical cannot possibly be correctly expressed by a definitional thesis: (R), (RI), (RC), and (RA) do not hold even for the ontology of physics, let alone the ontology of all of science.¹⁰ Toward this conclusion, Hellman and Thompson argue as follows:

When it is contemplated, moreover, that no matter how sophisticated the list [of basic physical predicates] and the "defining machinery", there are bound to be entities composed of "randomly composed" parts of other entities which elude description in the physical language, then it is evident that something is wrong with the whole approach.¹¹

The argument, concerning individuals, appears to be that the defining power of the physical language is not rich enough, no matter how strong it is made, to build expressions that would be satisfied by entities composed of randomly selected parts of other entities. The force of this argument, however, is elusive. Why should randomly selecting parts of other entities inevitably lead to the composition of entities which "elude description in the physical language"? Hellman and Thompson's presentation of the argument leaves unstated the assumptions that it depends upon; we can only speculate as to their identity. First, the physical language, P, is finitary (i.e., it has a countable alphabet, finitely long wffs and finitely long proofs); and,

¹⁰ Boyd has observed that it is possible that the ontologies of other branches of science could in fact be captured by a system of physical definitions even though the ontology of physics cannot. Despite such a possibility, the fact concerning the physical ontology is sufficient to defeat the definitional thesis. See Boyd (unpublished) for discussion.

¹¹ See Hellman and Thompson (1975), p. 553.

second, the intended interpretation of P has an infinite domain (denumerable or non-denumerable).¹²

Given these assumptions, there are straightforward arguments to show that combining "randomly selected parts" of entities in the domain inevitably leads to the composition of more entities than there are descriptions in P.¹³ What is troubling about Hellman and Thompson's statement of the argument is that they chose to focus on "random selection of parts", something which leads to the desired conclusion only in the presence of assumptions at least as strong as those stated above. Nonetheless, their strategy of exhibiting the inadequacies of the physical language for describing the physical ontology constitutes a significant development in physicalist thought. In the context of the present discussion, their reconstructed argument delivers a counterexample to (RI) by showing that (L/R) is true for objects in the physical domain. We bypass for now the question of whether their argument can be generalized to other categories of individuals (e.g., events); answering such a question depends upon one's theory of such categories.¹⁴

¹² A third assumption might be that members of the domain have infinitely many parts although this can be relaxed without significant changes in the conclusion; it is not clear what Hellman and Thompson assume in this regard.

¹³ The argument turns on the differences in cardinality of the class of terms in a finitary language and the class of entities constructible from an infinite domain of individuals.

¹⁴ See J.J. Thomson (1977) for a theory of events that quite readily leads to the same conclusion as that just obtained for objects.

Another argument along these same lines but somewhat more explicitly stated has been offered by Boyd:

Briefly the problem about definability arises because there are a continuum of possible physical states (if the true laws of physics are anything like those we now accept) but only countably many possible "definitions" in the vocabulary of fundamental physics.¹⁵

The problem here is straightforward: the cardinality of the set of all physical states is greater than the cardinality of the set of all physical definitions; hence, there must be some physical states for which there is not an associated physical definition. Hence, the physical language is not adequate for describing the full physical ontology (in this case, physical states); hence, we have a counterexample to (RA) since (L/R) vis a vis states (a category of attributes) is true.¹⁶

Again, the argument is presented without explicit statement of all the premises upon which it is based; what, for example, is the feature of "the true laws of physics" that leads to the indicated conclusion? Presumably, it is that some such laws express continuous, monotonic real valued functions; thus, if for each value of the function there is a distinct physical state (type), then there are uncountably many physical states (one for each real number in the interval of the reals for which the function is defined). And, if the physical language is

¹⁵ See Boyd (unpublished), p. 15.

¹⁶ States are sometimes treated as individuals (i.e., dated and located particulars); the discussion here concerns only kinds of states, or "state types", which we include as a category of attribute.

finitary, then the set of physical descriptions available is countable; thus the argument goes through easily.

In reply, one might try the following argument: of course, it is true that taken collectively the class of all physical states outruns the class of all physical definitions; but, for any given physical state, a physical description can be constructed; the problem is not that there is a physical state that is not physically definable; it is that we cannot define them all at the same time. Hence, the linguistic construal of the physicalist ontological claim is immune from the objection (i.e., (L/R) is not shown to be true by the cardinality argument.)

What is wrong with this reply is that, even if its premises are true (something which is not clearly the case) they are beside the point and do not entail the desired conclusion that the alleged counterexample to (R) does not exist. Why does it fail?

Our reconstruction of Boyd's argument is supposed to show that a linguistic construal of the ontological thesis fails of its purpose because (i) the equivalence (R) cannot avoid the cardinality objection if the universal quantifier ranges over a non-denumerable domain and the existential quantifiers range over denumerable domains and 'Rtx' expresses a relation which is (at least) a many/one mapping from the domain of 't' into the domain of 'x'; (ii) physics and its intended interpretation provide such an interpretation for (R) at least with

respect to states; and (iii) if (R) is false for physics and its intended interpretation, then the physical ontology is not definable and thus the full content of the ontological thesis is not expressed by the linguistic thesis. The reply, however, says only that it is always possible to reinterpret terms to refer to something else. This is true but irrelevant since it doesn't eliminate the counterexample to (R): (R) must be false if the above described conditions hold. And, if (R) is false the linguistic and ontological theses are not equivalent, no matter that terms can be reinterpreted. Therefore, a claim about definitions cannot serve as a statement of the physicalist ontological claim.¹⁷

The above argument has general applicability to other ontological categories; it applies to any entity that is characterizable along the continuum (e.g., properties, relations, events, objects). Since it is plausible that such entities, in each of these categories, exist in the ontology of physics, numerous counterexamples to (R) are in the

¹⁷ A possible reply is that all the physicalist wants to say is that everything is physically describable, hence physical. Therefore, the cardinality objection is not appropriate to physicalist concerns. But, the problem is: can the full force of the ontological position be captured by a purely linguistic thesis? The answer to this is "no". The reply suggests that there is a hybrid thesis that captures the ontological position; however, relative to the purposes of the present discussion (i.e., the prospects of purely linguistic theses), the reply is beside the mark: (i) it is not purely linguistic, (ii) it has its own difficulties, as we shall see below, concerning its assumption that every state is describable, (iii) it doesn't save (R), and hence, any view that presupposes (R). It simply suggests an alternative claim that the physicalist might want to make instead of the ones being considered.

offing. Regarding classes, it is a commonplace that there are more classes than there are terms in a finitary language; hence, any linguistic thesis designed to express the ontological claim that every class is a class of physical objects (or a class of classes of physical objects, etc) must fail.

To sum up this first class of cardinality objections against purely linguistic formulations of physicalist ontological claims: the central difficulty in all instances (i.e., Hellman and Thompson's argument concerning individuals, Boyd's argument concerning attributes, and the argument concerning classes just mentioned) is that the formulation of a thesis that presupposes (R) must fail to adequately express the ontological claim if (R) is false; (R) must be false if the language adverted to by the linguistic thesis is finitary and the domain of the intended interpretation of the language is uncountable - the reason being that there are simply more entities in the domain than there are terms in the language. Since these claims and the claim that the intended interpretation of natural science is uncountable are not controversial, we conclude that if the language of natural science is finitary, then (R) is false and the linguistic construal of the physicalist ontological claim is inadequate. It appears that the only line of reply left open to proponents of purely linguistic construals of physicalism is to explore whether the introduction of an infinitary or higher order language would save (R) and hence the whole approach. We shall defer discussion of this issue until later.

A second class of cardinality arguments against the left/right direction of (R) has been considered; these arguments differ from the first kind in that, rather than arguing that there are more members of the domain of interpretation than there are terms, they argue that there are members of the domain that are not describable in a finitary language. A very quick argument to this effect is found in Earman's comments on Hellman and Thompson's 1975 paper. In discussing the relevance of infinitary languages to problems with physicalism, he writes:

To provide some motivation for focusing on such languages, suppose that we want to express the "state description" of the world as a sentence. Such a sentence may need to have an uncountable number of conjuncts, each of which specifies, say, the values of certain physical fields at some space-time point on a given "time-slice". And, to characterize the relevant features of such state descriptions, we may need an infinite string of quantifiers...¹⁸

For present purposes, the idea of a total state description of the world at a time requires that we work in a language rich enough to provide descriptions of every space-time point and to combine such descriptions to form a conjunction of uncountable length. Such a requirement is not met by a finitary language; hence, assuming that we are working with only finitary languages, we have an example of a single state which is not describable within those languages. Therefore, another counterexample to (R) vis a vis states is plausible. We leave aside for now the question of whether more powerful languages would help in sidestepping this objection.

¹⁸ See Earman (1975), p. 565.

It might be replied that it is easy to find finite expressions that refer to the total state of the world at some time: e.g., 'the total state of the world right now', or less trivially, some finite set of physical equations which are defined for every spacetime point on a given time slice. A key issue raised here concerns the limits of "acceptable physicalistic descriptions and definitions"; Earman's objection suggests that it is not possible to find finite expressions that are physicalistically acceptable and which represent a full state description of the world. It is clear that if the only such descriptions must involve uncountable conjunctions, then the resources of a finitary language will not suffice. The reply suggests that there are finitary expressions that suffice for such purposes. However, the first such description' (i.e., 'the total state of the world right now') is deficient: it does not provide a way of discriminating different current states and it provides no resources for characterizing theoretically relevant features of the present state. As a result, such a description can play no significant role in a physicalistic system: it is a trivial description which is not physicalistically acceptable.¹⁹

A related line of attack against the left/right direction of (R) is based upon the so-called "multiple realizability" of some kinds of states (e.g., some kinds of mental states). The functionalist view of

¹⁹ The second suggested description (i.e., a finite set of equations in a finitary language which characterize all relevant properties and are defined for each spacetime point) may be more promising, although it is too vaguely stated to be assessed.

mind, for example, holds that some mental states are definable in terms of their "causal role" in the mental life of individuals who can be in them; since a given causal role can be played by a diversity of physical mechanisms, even if each instance of a functional state is to be identified with an instance of some physical state, there is no physical state which is nomologically correlated with that functional state. Hence, predicates which express functional states are not definable in physical terms.²⁰ So far we don't have a counterexample to (R) (i.e., a non-definable physical state), although we have two claims that must give the physicalist pause:

1. That there is no physical state that is nomologically correlated with a given functional state;
2. That predicates that express functional states are not definable in physical terms.

To have a counterexample to the left/right direction of (R), we must have a state which is physical and which is such that there is no term that both expresses it and is physically definable. Let us see if a counterexample can be developed.

²⁰ Anticipating the following discussion a bit, we point out that these claims concern only first order, finitary physical states. We shall see below that the reasoning just stated is defective in that the premise that there is no physical state that is nomologically correlated with a given functional state is arguably false; there are both first and higher order physical states that are candidates for being so correlated.

To facilitate discussion, we shall grant that functional states need not be physical states (i.e., not every state is physical); the brand of physicalism we countenance allows that there are non-physical states (and other attributes) but requires that actual or nomologically possible realizations of such states be physical.²¹ Further, let us allow that the class of all nomologically possible realizations of a functional state, F, is a well defined class, although not one that is particularly easy to list; hence, the physical realization of every actual or nomologically possible instance of F is a member of that class.

Now, it may be supposed that, allowing certain constructive apparatus for taking states and forming new states out of them,²² there is a single physical state which corresponds to this class, and hence, which is nomologically correlated with F. This state is "the physical realization of F in nature": all and only the nomologically possible realizations of F are instances of it. And, now it may be asked whether or not that state is expressed by a physically definable predicate. If the answer is "no", then we have another counterexample to (R) (i.e., we will have a physical state that is not expressed by a physically definable term.) We shall consider two cases corresponding to different ways of construing the constructed physical state: (i) as a first order disjunctive physical state and (ii) as a second order

²¹ Below, in Chapter 4, we shall discuss what a realization is.

²² For example, disjunctions of states yield new states.

state.

In the first case, the correlated state is the physical state, P, which is either P1 or P2 or P3 or..., where the '...' continues until the class of all possible physical realizations of F is exhausted. The predicate expressing this state would then be of the form 'P1 v P2 v P3 v ...'. If the class of possible realizations of F is finite, then there is no problem; but, if, as is most probable, the class is infinite, then no expression in a finitary, first order language would express the physical state in question. Again, the question of whether adverting to an infinitary language would avoid the objection arises. Bypassing this question, we see again that if physicalist formulations are restricted to finitary languages, then the linguistic construal of the physicalist ontological claim is unacceptable. This time the counterexample to (R) depends upon there being a physical state that is constructible out of an infinite class of other states.²³ If such a construction is acceptable, then the counterexample follows from the difference of cardinality of that class and the finite length of expressions in finitary languages.²⁴

²³ The reply that this state can be simply named by a finite expression fails for the same reason that it failed earlier.

²⁴ The problem here is not the failure of nomologicality (cf., Fodor (1975), Davidson (1970)) but of expressibility; there is no first order physical expression in a finitary language that picks out the class of all nomologically possible realizations of F. Hence, there is a state that is not expressed by a physically definable term because it is disjunctive and has an infinite number of disjuncts. In reply to those who might think that the problem is one of failure to be nomological (e.g., because the predicates expressing such

The second case (i.e., the correlated physical state is a second order state) appears to be more promising. A second order physical state is one which is characterized in terms of a quantification over first order physical states. If we could find a condition which all and only the members of the class of physical realizations of F share, then we could specify a second order physical state P corresponding to the class as follows: P is the second order physical state of being in some first order physical state which ..., where the '...' stands for some condition which a physical state must have to count as a realization of F (e.g., a certain causal rôle).²⁵

The strategy then is to have in our language a stock of terms expressing second, (and higher) order physical states; such terms efficiently bypass the need for individually specifying each realization of higher order states. Notice that no guarantee is made that there will be enough terms for all such states; the current concern, however, is with individual states that are difficult to define. Here,

states are not natural kind predicates and, hence, don't occur in any laws), it is suggested that the most plausible way of construing nomologicality is in model theoretic terms; on such a construal, the states in question are seen to be nomologically correlated. The view that the failure is one of nomologicality is endorsed by Fodor and Davidson. However, they have adopted a different strategy from ours: viz., to restrict consideration to the actual realizations of a given state (or, to actual events) and to point out that a disjunctive state (event) built up from the instances is not likely to be lawfully correlated with the realized state (event). Our approach, however, starts with the assumption of a class of all possible realizations of a given state; hence, nomologicality of the correlation is assured.

²⁵ See Putnam (1970), Field (1975) for discussion of this proposal.

however, if cardinality problems don't threaten, then triviality problems do. That is, the problem facing the proponent of this approach is to find for each state (or other attribute) a condition which any physical state must satisfy to count as a realization of that state. To find such a condition is to sail between the Scylla of enumerating a non-enumerable class and the Charybdis of saying only that each physical state in the class is a realization of the non-physical state in question. The latter alternative, though expressible, is not clearly worth expressing: e.g., for F, the nomologically correlated physical state is the second order physical state of being in some first order physical state which is a realization of F. What is wrong here is not that we haven't deflected the alleged counterexample to (R), but that we have done so at the cost of creating mysteries. In the absence of an explanatory condition in other terms, what makes each physical realization a realization of F is unclear and apparently primitive. Now, such a specification serves well enough to avoid the counterexample to (R), but the physicalist should ask if the price is too high, given the goals of the program.

Although there have been proposals for how to avoid both problems and to specify the conditions on higher order physical states,²⁶ all such proposals have, to date, been vague. They constitute promises as yet unfulfilled; hence, for now, though we are sympathetic to such

²⁶ See Putnam (1970), Field (1975), Shoemaker (1979), Boyd (unpublished) for some general suggestions. Also see below (Chapter 4) for our discussion of "realization theories".

proposals, we cannot count them as an adequate buttress for the approach appealing to higher order physical states to avoid the objection that there are states not expressible in a finitary language. Thus, the second objection to the left/right direction of (R) stands firm.²⁷

In summary, the first class of cardinality arguments discussed above (i.e., those of Boyd and Hellman and Thompson) have shown that no first order, finitary language is adequate for describing all individuals, attributes or classes in the physical ontology; hence, (R), (RA), (RI), and (RC) all fail in the left/right direction. Therefore, if physicalist linguistic theses are restricted to such a language, then purely linguistic formulations are inadequate expressions of physicalist ontological concerns. The failure is clearly connected with the restricted expressive power of finitary languages; hence, the issue of whether appeal to infinitary or higher order languages can deflect the objections and preserve the linguistic approach is left open by this argument. Our consideration of the second class of objections to the left/right direction of (R) (i.e., Earman's objection and the objection based upon the multiple realizability of states) has further suggested that there are individual states that may not be ex-

²⁷ It might be objected that the proposals that have been made do not avoid the counterexample to (R) since the conditions suggested are not specified in "physical" terms, although they are conditions that specify a physical class. We shall bypass this objection, since the problem of identifying any non-trivial conditions is difficulty enough; at a later point, this further problem will have to be faced.

pressible in a finitary first order language. Thus, there is additional support for the failure of (R) for such languages. We also saw that higher order finitary languages might also be unacceptable for expressing such states, although such an avenue has not been completely closed off.²⁸ For now, modulo finitary first order languages, we conclude that the physicalist ontological position does not entail the linguistic theses thought to be equivalent to it.²⁹

We shall now turn to a discussion of the problems besetting the right/left direction of (R): i.e., if each thing is such that it is referred to by a physically definable term, then it is physical. Again, it will be seen that counterexamples plausibly exist; hence, every individual (or attribute) could be referred to by a term definable in physical terms and yet it not be the case that every individual (or attribute) is physical. Because the failure of the left/right direction is sufficient for rejecting the equivalence (R), our discussion of the problems with the right/left direction will be brief. We mention them to introduce ideas that will be useful later and to alert the reader to the fact of their existence.

²⁸ Pending the outcome of the search for non-trivial conditions occurring in the specifications of higher order physical states.

²⁹ With respect to a different and much weaker supposed entailment (viz., if everything is physical, then every term used to refer to a physical thing is physically definable), it is also clear that finitary languages are unacceptable and that higher order languages may be so as well. This is possibly what many physicalists have had in mind, although it fares no better than the stronger claim we have been considering.

Again, Hellman and Thompson have suggested a counterexample.

Against the claim that a strong form of reductionism³⁰ entails ontological physicalism they argue as follows:

To see this, consider a very simple theory, E, containing just two non-logical one-place predicates, P and Q, and the following non-logical axioms:

1. $\exists x \exists y (x \neq y \ \& \ (z)(z = x \vee z = y))$
2. $\exists x (Px \ \& \ (y)(Py \ \text{---} \rightarrow y = x))$
3. $\exists x (Qx \ \& \ (y)(Qy \ \text{---} \rightarrow y = x))$
4. $(x)(Px \vee Qx)$

That is, E asserts that there are exactly two objects and that exactly one object is a P and exactly one object is a Q and everything is a P or a Q. Now, in E, the following is provable: $(x)(Qx \ \text{---} \rightarrow \neg Px)$. In other words, Q is definable in terms of P. Yet, this doesn't guarantee that all objects are, or are exhausted by, P-type things. In fact, in every model of E, there are two disjoint subsets of entities, one P-type, the other Q-type.³¹

Strictly speaking, this line of argument provides us with a counterexample to the right/left direction of (R): we have a situation in which every object is referred to by a predicate that is either a P-term or a term definable by P-terms and yet not everything is a P-type thing.

³⁰ They construe physical reductionism to be the claim that:

[...]in the theory consisting of all the lawlike truths of science (stated in an adequate language), including, of course, physical theory, every scientific predicate is definable in physical terms. That is, for every n-place predicate P, the laws of science entail a formula of the form: $(x_1) \dots (x_n)(Px_1 \dots x_n \ \text{---} \rightarrow A)$, where A is a (finite) sentence containing only physical vocabulary as non-logical terms, and occurrences of n distinct variables x_1, \dots, x_n .

³¹ See Hellman and Thompson (1975), p. 557.

Despite the directness of this argument, it is not completely satisfying: first, how much does it depend on there being only two predicates in the formal system?; second, are negative definitions acceptable for the physicalist?; and third, does anything depend upon there being a finite domain?

In a footnote, Hellman and Thompson claim that nothing depends upon there being only two predicates:

If use is made of certain relative terms, clearly within physicalist vocabulary as conceived by traditional reductionist positions, e.g., predicates of location, then parallel arguments can be constructed for theories containing any finite number of predicates. (emphasis added)³²

But predicates of location, like negative definitions are suspect: the locatability of all phenomena is allowed for by some dualist positions. What the dualist denies and the physicalist asserts is that the phenomena themselves are physical; and being physical is not a simple matter of being located.³³

Although Hellman and Thompson have presented a counterexample to (R) as it was presented, the question arises as to whether the physical reductionist can't simply reply that the position refuted is not his; not just any definition is physicalistically acceptable. In par-

³² See Hellman and Thompson (1975), p. 557.

³³ Some physicalists would dispute this claim contending that being located in the space-time continuum is the only mark of the physical that withstands close scrutiny; being physical is just a matter of being located if being physical is anything at all. See below (Chapter 3) for discussion of this and other attempts to characterize "the physical".

particular, definitions that are negative or locational are not acceptable. Hence, Hellman and Thompson have failed to establish what they intended to because their argument depends upon physicalistically illicit definitions: there are restrictions on what an acceptable definition in physical terms is!

The question that now arises is: what are the restrictions? Although Hellman and Thompson have not provided a conclusive counterexample to a clarified formulation of (R), they have shown that the burden is on the physical reductionist to produce motivated restrictions on physical definitions which are sufficient to avoid the counterexamples. Hence, in the case of individuals, there is at least a question as to whether or, not the physicalist can restrict the definitions so as to preserve the entailment from definability to ontology.

In the case of attributes, there are also difficulties. That is, the physical definability of a predicate expressing an attribute does not guarantee that the attribute is a physical attribute. The point here is that nomological coextensiveness of predicates A and B is not sufficient for identification of the attributes expressed by A and B. So, even if all predicates in science are either physical primitives or definable in terms of such primitives, there is no entailment of the ontological claim that every attribute is a physical attribute: such definability is quite compatible with "property dualism". Hence,

the right/left direction of (RA) is false.³⁴

Many contemporary physicalists have abandoned this very strong ontological claim; and they have opted for weaker theses concerning either the instances of non-physical attributes (token identity) or, as mentioned above, the physical realizations of attributes. Neither of these views is a consequence of the physical definability of all terms in science. However, although the definability claim is not sufficient to express the ontological claims of the physicalist, it may still be of interest with respect to other goals (e.g., explanation).

Finally, we point out that, regarding classes, the arguments cited above concerning individuals defeat (RC) in the right/left direction. That is, it follows from the the claim that there could be non-physical individuals that had physical definitions that there could be classes of non-physical individuals that had physical definitions.

In this section, we have seen the general failure of equivalence between formal claims concerning the physical definability of terms in the language of science and ontological claims concerning the physical

³⁴ Of course, nothing we have said rules out the possibility that attributes expressed by some predicates used in "non-physical branches of science" are physical attributes. The point here is essentially that whether an attribute is physical or not does not depend on the specified relations between linguistic items; in particular, nomological co-extensiveness of terms is not a sufficient condition for attribute identity. What it does depend upon are relations between attributes. See Putnam (1970), Malinas (1973), Swinburne (1982), Sober (1982), Causey (1977), Enc (1976), Shoemaker (1979), and Achinstein (1974) for discussion of the myriad of issues and positions involved here.

nature of all individuals, classes and attributes. Proponents of purely linguistic construals of the physicalist ontological claim are committed to this equivalence; hence, we conclude that a purely linguistic construal is inadequate. This conclusion depends upon viewing the "language of science" as having the characteristics that were exploited in the arguments cited above.³⁵ At this point, we have not yet addressed the issue of whether this construal is acceptable; nor have we fully explored the generality of the arguments with respect to languages with different properties. Within these limitations, the moral is: to propose a thesis concerning the physical character of all phenomena, quantify over the phenomena themselves rather than the linguistic objects that are used to talk about those phenomena (i.e., formulate non-linguistic theses). A deeper moral, which we shall continue to develop throughout this project, is that an adequate physicalist doctrine, being concerned with both the formal system of scientific knowledge as well as the intended interpretation of that formal system (i.e., nature) had better honor the known results and difficulties concerning relations between formal systems and their intended interpretations: for example, incompleteness, the existence of non-standard models, the Lowenheim-Skolem theorem, cardinality considerations. As a consequence of such findings, and because the physicalist is concerned with both nature and the formal system of science, sepa-

³⁵ I.e., the characteristics of finitary, first order languages, in the case of the arguments against the left/right direction of (R). No such restriction was made in the discussion of the right/left direction.

rate theses concerning each may be required for adequate expression of the physicalist doctrine.

The second category of physicalist concern for which linguistic theses have been proposed is that of objectivity. Physicalists concerned with the theses of the indiscernibility of physical identicals and of the determination of all facts by the physical facts have expressed their concerns linguistically as follows:

(I) For every non-physical predicate and every distinction it makes, there is a physical predicate that makes that distinction.³⁶

(D) The physical truth determines the whole truth about nature.³⁷

These formulations of theses concerning objectivity are typical in the respects that we shall be considering: the appeal to linguistic objects (i.e., predicates in the case of (I) and truths formulable in a language in the case of (D).) And, it is these respects that we shall argue make the formulations inadequate expressions of the physicalist position that they are intended to express, although they do formulate theses of interest in their own right. We shall contend that (I) and (D) formulate theses concerning the formal system of science, but not theses concerning nature; two additional theses may be called for to fully express the physicalist position regarding objectivity in na-

³⁶ See Hellman and Thompson (1975) for their formalization of this claim.

³⁷ See Quine (1969b), Hellman and Thompson (1975), Friedman (1975) for discussion and ways of formalizing this claim.

ture.³⁸

What (I) fails to capture is the thesis that all distinctions between objects are associated with physical distinctions between objects. The reasons are by now familiar: quantification over predicates is limited to, at most, a denumerably infinite domain, whereas the class of all possible distinctions is non-denumerable. Hence, the thesis (I) fails to capture the full force of the "ontological" claim of indiscernibility. The strategy for demonstrating this follows along the lines employed above in showing that (R) was false; hence, it could be shown that it was possible for two objects to exist which differed in some physical or non-physical respect but for which there were not predicates available to mark the distinction. We shall bypass the details of this argument.³⁹

A similar, but more complicated, line of reasoning can be directed against (D). The notions of truth and of elementary equivalence⁴⁰ are

³⁸ Such objectivity is to be thought of in terms of determination of fact and indiscernibility of objects; at issue, is whether this is best thought of in purely linguistic terms or in terms of the entities and attributes that exist in nature and are referred to and expressed by linguistic objects. See, also, Horgan (1981), Haugland (1982), and Kim (1982a) for further discussion of how to express these claims.

³⁹ Quineans may reply that quantification over respects, differences, properties, etc is illicit and that the linguistic construal is the only one possible. See Quine (1978, 1979) himself for non-linguistic construals that avoid the problem at hand.

⁴⁰ In Hellman and Thompson (1975), the thesis of physical truth determination is developed in terms of elementarily equivalent models.

unavoidably tied to the notion of an interpreted language; and, the notion of an interpreted language is not necessarily adequate for discussing all of the features of structures that interpret languages. In general, theses about language are not equivalent to theses about structures; hence, with regard to the structures that interpret the language of science, the determination⁴¹ of "fact" should be expressed by a thesis that is about the structures themselves.⁴²

The problem in the case of both (I) and (D) is not that they fail to be true; but rather, it is that they fail to express the right theses: i.e., a thesis concerning all similarities and differences in nature and a thesis concerning all facts about nature. Alternatively put, they express theses that are simply about the wrong things and that are not equivalent to any relevant true theses about the right things. Again, the moral is: if you want to express a thesis concerning all phenomena in nature, quantify over those phenomena rather than over the linguistic objects that you use to describe those phenomena.⁴³

⁴¹ The notion of "determination" in theses about nature (e.g., The physical phenomena determine all the phenomena) may have a quite different content than the notion of "determination" used in (D).

⁴² Hellman and Thompson (1975, p. 558) appear to be aware of this shortcoming of their formulation of the thesis of truth determination. A formulation concerning structures might plausibly be couched in terms of isomorphism of structures.

⁴³ (I) and (D) may be perfectly good theses concerning the objectivity of scientific knowledge and predicates.

In addition to the inadequacies of linguistic construals of concerns about ontology and objectivity, past linguistic construals of explanatory concerns have also been deficient. Recall⁴⁴ that the explanatory concerns of the physicalist program involve (i) reduction of the total number of mysteries in nature, (ii) reduction of the number of fundamental mysteries, and (iii) increase in the power of explanatory principles. Classical physicalist formulations consisted of, in addition to definitional theses, theses concerning the derivability of the laws of every special science from the laws of physics plus bridge principles.⁴⁵ It was believed that such derivability theses were adequate expressions of the physicalist's explanatory concerns because such derivations were thought to entail (a) the explanation of the derived laws, (b) the explanation in physical terms of all phenomena subsumable under the derived laws, and (c) an increase in the explanatory power of the physical laws, as a consequence of their increased generality.

⁴⁴ See Chapter 1.

⁴⁵ There is difference of opinion about whether the physical laws involved in such derivations are the "basic laws of physics" or laws which are not necessarily consequences of the basic laws but which are obtained by substitution of physical definitions for defined non-physical terms in some "non-physical law". Our current concerns in no way depend upon how this dispute is resolved. See Kim (1978), Boyd (unpublished), Spector (1978), Hellman and Thompson (1975, 1977) for discussion of these issues.

That such a formulation is not an adequate expression of the explanatory concerns of physicalists is due to the fact that it depends crucially upon a shortcoming of the D-N model of explanation: that is, such a formulation presupposes that derivation from a set of laws is sufficient for explanation by those laws. As is well known, this presupposition is dubious.⁴⁶ At best, the view under consideration must be supplemented in some way to guarantee explanation; and, it is entirely possible that the manner of supplementation will not be linguistic in character.⁴⁷ If so, then a purely linguistic construal of explanatory concerns fails of its purpose. In our estimation, the idea that a purely linguistic feature of a derivation could be sufficient to sort out the explanatory derivations from the non-explanatory ones is most implausible. Hence, at this point, we reject purely linguistic theses as candidates for expression of physicalist explanatory concerns.

An important instance of this general problem has been discussed by Fodor in his "Computation and Reduction".⁴⁸ There he convincingly argues that "the classical reduction of psychology to neuropsychology" (or physics) can result in a loss of explanatory power. Put in the

⁴⁶ See Bromberger (1966).

⁴⁷ For example, it might be required that the derivation be from laws concerning objects that are parts of wholes which the derived laws are about. See Oppenheim and Putnam (1958) and Causey (1977) for discussions of such a view.

⁴⁸ See Fodor (1978).

terms of our discussion, what Fodor suggests is that the neurophysiological translations of psychological laws (given definitions of psychological terms by terms drawn from the neurosciences) will fail to provide adequate explanations of the phenomena subsumable under the psychological laws.⁴⁹ The reason for this is that definitions constrained by only nomological coextensionality need not preserve features of the defined predicates that contribute essentially to their roles in (psychological) explanation. Fodor argues that this is plausibly the case for psychological predicates that express the content of a mental state and neurophysiological predicates that may be nomologically coextensive with them.⁵⁰

If Fodor's argument is sound, then it provides a counterexample to the claim that the derivability of a class of laws from another class entails that the phenomena subsumed under the members of the first

⁴⁹ Hence, goal (b) above will not be satisfied.

⁵⁰ The alleged loss is said by Fodor to consist in: (i) a loss of the distinction between arbitrary and coherent relations between states, (ii) an inability to state in neurophysiological terms generalizations statable in psychological terms, (iii) an inability to make the same explanations in both fields, and hence, (iv) a loss of explanatory power. Given (i) - (iv) and assuming that the discussion is concerned with "non-eliminative programs", then "total science" does not lose explanations as a result of reduction as Fodor suggests; but, neither is it unified by reduction in the way thought by classical reductionists. Lower level sciences do not gain in the explanatory power of their principles as a result of such reduction; and, neither are the phenomena and laws studied in the higher level sciences explained by lower level sciences. On the other hand, if one is concerned with an eliminative reductive program, then reduction would result in a loss of explanations and explanatory power of total science.

class are explained by the members of the second and, hence, that the laws in the second class gain in explanatory power. This point, in conjunction with the idea that the derived laws themselves are not a fortiori explained, substantially undermines the classical reductionist program with respect to the explanatory goals of physicalism.

In the same paper, Fodor goes on to discuss potential additional constraints on the (derivational) reduction of psychology to neurophysiology which would effectively rule out the possibility of loss of explanatory power resulting from a "successful" reduction. But, in doing this, Fodor is quick to point out that, although such constraints may be forthcoming in the psychology/neurophysiology case, they are almost, certainly not going to be forthcoming in the psychology/physics case. For the physicalist this is sad news, since it is reduction to physics that is of primary importance.

Fodor appears to have provided good grounds for believing that classical derivational reduction of theories is not the proper expression of the physicalist's concerns with explanation, since such reduction is compatible with a consequent failure to increase explanatory power. As we have emphasized above, an adequate expression of the physicalist view would not be compatible with such a failure.

Therefore, the general dubiousness of the alleged entailment of explanation by derivation, the likelihood that this cannot be generally guaranteed by supplying additional linguistic constraints, and final-

ly, Fodor's specific arguments and conclusions suggest that the classical linguistic construal of physicalist explanatory concerns fails to adequately express the intended physicalist thesis: i.e., that all laws of the special sciences and their instances are explained in physical terms, that the explanatory power of the explaining principles is increased by such explanation and that the total understanding yielded by science is thereby increased.⁵¹

In addition to classical reductionism, physicalists have proposed alternative, and logically weaker, linguistic theses concerning inter-theoretic relations. One such thesis, advanced by Quine⁵² and developed by Friedman⁵³ and by Hellman and Thompson,⁵⁴ is the thesis of physical truth determination: the physical truth determines the whole truth about nature. As discussed above, this thesis does not adequately express the physicalist's ontological claim about the physical determination of all phenomena, but nonetheless, it may accurately express important relations between physical truth and all other truth, especially the truths developed in the special sciences.

⁵¹ It will be seen below that many physicalists view these goals as "utopian" and in need of modification for a realistic physicalist program.

⁵² See Quine (1969b).

⁵³ See Friedman (1975).

⁵⁴ See Hellman and Thompson (1975).

As formulated by Hellman and Thompson, the thesis gives expression to a linguistic, non-reductive thesis which expresses the physicalist's intuition that "given a full characterization of things in physical terms, one and only one full characterization of things in (non-physical) terms is correct."⁵⁵ The inadequacy of this claim as an expression of explanatory concerns is self-evident: if anything, the claim demands that an explanation (of the determination) be provided. At a minimum, an explanatory thesis must be reductive in that it relates individual members of each class (i.e., it relates the explained and the explaining phenomena, laws, terms, etc); hence, Hellman and Thompson truth determination is clearly inadequate for expressing explanatory concerns.⁵⁶

⁵⁵ Their formalization of truth determination gives precise content to the notion of truth determination in model-theoretic terms which expresses the idea that, given any two "standard" models of natural science, if they are elementarily equivalent with respect to the physical sub-vocabulary of the language of science, then they are elementarily equivalent with respect to the non-physical sub-vocabulary of the language of science. In short, if you fix the physical truths, then you fix all the truths. We must bypass discussion of the interesting details and issues raised by their proposal; we shall be focusing only on its non-reductive and linguistic features.

⁵⁶ Hellman (1978) discusses connections between physical and non-physical terms; thus, he seems to view truth determination as "reductive" in our sense. However, since the connections are not nomological in Hellman's view, there is little reason to believe they constitute explanatory connections. Thus, whether one views Hellman and Thompson truth determination as reductive or non-reductive, it is not likely to satisfy explanatory goals of the program.

An alternative expression of truth determination, offered by Michael Friedman, which he calls "weak reducibility", might also be considered as an expression of physicalist explanatory concerns. He writes as follows:

Let 'F_{1x}', 'F_{2x}', ..., 'F_{nx}' be the primitive predicates of the theory to be reduced. Let a physical realization be a mapping B which associates each 'Fix' with a set of open sentences containing only physical predicates, B('Fix') = ['A_{1x}', 'A_{2x}', ...]. For any sentence containing only predicates from among 'F_{1x}', 'F_{2x}', ..., 'F_{nx}', we can define truth under the realization B and satisfaction under B - they are defined just like satisfaction and truth, except that the clause for atomic formulas now reads: A sequence s satisfies 'Fix' under B iff there exists an 'A_{jix}' in B('Fix') such that s satisfies 'A_{jix}'. Let us now define weak reducibility...: a theory is weakly reducible to physics if there is a physical realization B of its primitive predicates such that for each predicate 'Fix' and each spacetime point q, 'Fix' is true of q just in case some 'A_{jix}' in B('Fix') is true of q (i.e., 'Fix' is not coextensive with any single physical predicate, but rather with a 'disjunction' - possibly infinite - of physical predicates) and in every model of physics the theory comes out true under B.⁵⁷

Here we have the non-physical truths as semantic consequences of the physical truths; the potential derivations of the former from the latter are mediated by (possibly infinite) disjunctive physical definitions of non-physical predicates. If we assume that the thesis of weak reducibility of all theories by physics is correct, would it adequately capture the desired physicalist explanatory concerns? The answer is clearly not! First, weak reducibility does not avoid the general difficulties associated with the required entailment of explanation by derivation: Friedman has generalized classical derivational reduction

⁵⁷ See Friedman (1975), p. 358-9.

and has not provided any additional criteria which would sort out explanatory from non-explanatory derivations. Second, weak reducibility is vulnerable to Fodor's objection that in certain cases derivational reductions can lead to no gain in explanatory power. This is for the same reasons cited above with respect to strong, linguistic reduction. Third, Friedman highlights an explanatory concern mentioned earlier: what principles are there which account for the inclusion of specific physical predicates in the class of physical predicates associated with a given non-physical predicate? The position raises questions that it cannot answer, and hence, at the least, it must be supplemented to yield a more adequate physicalist position concerning explanation. Realization of non-physical attributes by physical attributes should be explained in an adequate physical system; this means that some account must be given of membership in the class of physical predicates associated with a given non-physical predicate. Without such an account, mysterious physical/non-physical connections abound. Hence, although Friedman may have offered a good account of truth determination in science, his thesis does not adequately express physicalist explanatory concerns.

Finally, Hartry Field⁵⁰ has offered a formulation of inter-theoretic reduction which may be more promising than any of the alternatives so far considered; unlike Friedman, he employs Putnam's idea of second order physical properties, and associated definitions, to general-

⁵⁰ See Field (1975).

ize classical reductionism so as to handle the difficulties posed by functionalism in psychology (and plausibly elsewhere). Thus, he formulates the physicalist claim of inter-theoretic reductionism just as the classical reductionist with the exception that the bridge laws may include second order physical definitions of non-physical predicates (i.e., definitions which correspond to specifications of second order physical properties).

Field's formulation is clearly vulnerable to the general difficulty posed for all derivational theses based upon the presupposition that derivation entails explanation. However, it is not clear that his formulation is vulnerable to Fodor's objection or to the objection raised against Friedman. Recall that a second order definition involves quantification over physical properties and specification of a condition that a physical property must satisfy to fall within the class of properties that are associated with the second order physical property. Hence, this position avoids the problem encountered by Friedman, but, only if the condition is non-trivial. Regarding Fodor's objection, all depends upon the character of the condition supplied by the definition. If the condition is explanatorily equivalent to the original predicate, without simply including it then the use of second order definitions may avoid loss of explanatory power: i.e., the physical laws using second order physical predicates may be able to explain the instances of the non-physical law which it reduced as well as that law itself. At this point, we leave this as an open issue pending the development of relevant second order, physical definitions.

In any event, Field's formulation, insofar as it adverts directly to derivation (without providing purely linguistic grounds for distinguishing explanatory derivations) is vulnerable to the first objection. At the least, such grounds must be provided to save the purely linguistic expression of physicalist explanatory concerns.

To summarize this section: purely linguistic theses advanced so far are clearly not adequate expressions of physicalist concerns about ontology, objectivity and explanation. In the case of ontology and objectivity, it is clear that no such thesis could be adequate if the restriction to a finitary language is made; in the case of explanation, the issue remains open.

2.2 PURELY NON-LINGUISTIC FORMULATIONS

Many recent proponents of purely non-linguistic theses (e.g., everything is physical, the physical phenomena determine all the phenomena) have typically been quite aware of the differences between these theses and linguistic theses of the kind considered in the last section.⁵⁹ In the wake of the variety of difficulties that classical, linguistic reductionism has encountered, the principle contention of such thinkers has been that one can be a physicalist without being committed to implausibly strong views about the formal structure of science. For example, Davidson's doctrine of "anomalous

⁵⁹ See Hellman and Thompson (1975), Fodor (1975), Davidson (1970), Boyd (unpublished), Quine (1978, 1979).

monism" is the view that (i) every event is a physical event, (ii) the mental supervenes upon the physical, (iii) there are no nomological correlations of mental and physical phenomena, and hence, (iv) there are no psycho-physical laws. Physicalism, on this view, is compatible with the existence of no strong formal or explanatory connections between theories in different branches of science.

In a quite similar vein, Quine writes as follows:

It [his brand of physicalism] is not a reductionist doctrine of the sort sometimes imagined. It is not a utopian dream of our being able to specify all mental events in physiological or microbiological terms. It is not a claim that such correlations even exist, in general to be discovered; the groupings of events in mentalistic terms need not stand in any systematic relation to biological groupings. What it does say about the life of the mind is that there is no mental difference, without a physical difference.⁶⁰

Because both Davidson and Quine discuss their physicalism in the context of discussions of mental phenomena, it is not entirely clear that what they have to say generalizes to all of science; that is, it is not entirely clear whether a version of physicalism that requires only an ontological claim plus a supervenience or indiscernibility claim and that does not require any stronger systematic relations between physical and non-physical phenomena or theories is sufficient, on their view, for all of science. We shall take them to hold that it is sufficient.⁶¹

⁶⁰ See Quine (1979), p. 163.

⁶¹ This is perhaps debatable, because both Quine and Davidson believe that there is something special about the mental that distinguishes

A third version of physicalism that imposes minimal requirements on the formal structure of science is found in Fodor's introductory chapter to The Language Of Thought. The doctrine of "the generality of physics" constitutes his concessions to previous physicalist strivings for a unified science. On this view, the ontology of physics is adequate for serving as the ontology of all science, but it is not required that there be lawlike correlations between physical and higher level phenomena. To paraphrase Fodor, the natural kinds studied in psychology and the other special sciences need not correlate in lawlike fashion with the natural kinds studied in physics. Further, he does appear to build into his position views at least as strong as supervenience and indiscernibility: he requires that individual physical events "realize" individual non-physical events, or alternatively, that physics provides the mechanisms that underlie the phenomena studied in the special sciences.⁶²

it from science: viz., the methodology of ascription of mental attributes is different from a scientific methodology. Hence, it might be claimed that this weak physicalism is only intended to apply to the mental-physical case. However, in Quine (1978), he is quite unequivocal regarding the applicability of his claims to all of science. Hellman and Thompson similarly characterize their view as requiring no nomological correlations and no psycho-physical (or other such) laws; they appear to differ from Quine and Davidson in that they emphasize certain linguistic claims although there is no reason for Quine or Davidson to deny them.

⁶² See Fodor (1975). Other philosophers who have emphasized non-linguistic physicalism include Boyd (unpublished) and Kim (1982a). In all such views, it is important to clarify, when giving up nomological type-type correlations in favor of individual physical/non-physical associations, whether the associations are nomological, are merely contingent, or are of some intermediate degree of counterfactual force.

Although there is some difference between the views of Quine, Davidson, Fodor, and Hellman and Thompson, they appear to be in agreement on the idea that a version of physicalist doctrine that requires (i) that every individual is physical and (ii) that all phenomena depend upon physical phenomena is a sufficient expression of the physicalist position in science. Such a view does not require (iii) that there are nomological correlations between physical and non-physical phenomena or (iv) that terms drawn from the vocabularies of the special sciences enter into lawlike sentences with terms drawn from the vocabulary of physics or (v) that the lawlike sentences which express the laws of the special sciences are derivable from lawlike sentences that express physical laws, or (vi) that there be explanations of non-physical phenomena (individual and regularities) in terms of physical phenomena.⁶³ Of course, if such stronger connections (i.e., (iii)-(vi)) are developed they are not to be eschewed; the point of the position being considered is that physicalism does not require such connections in order to serve its underlying motivations.

⁶³ Fodor appears to hold a "token explanation" doctrine on which the physical mechanisms underlying individual psychological phenomena are studied and used to offer explanations of the phenomena that depend upon them. However, he also appears to reject the idea that, in general, the higher level laws are derivable from or explainable in terms of physical laws and he rejects the idea that, in general, there are psychophysical laws. As we shall see below, it is improbable that one can hold all of these positions concurrently.

Now, it should be evident to the reader that such a version of physicalism is not adequate by our criteria; it fails because, although it expresses the important features of the physicalist view of nature, it does not make any demands upon the formal system of science vis a vis explanation. As a consequence, the program of physicalism based upon such a formulation of the doctrine could be "successful" and yet fail to realize the goals of the program. In our estimation, physicalists who opt for this weak version of the doctrine have retreated much too far back from the original strong formulations of the doctrine.

Not all physicalists who have emphasized the non-linguistic side of the doctrine have retreated so far; that is, some recent physicalists have not restricted themselves to claims concerning ontology, supervenience, and indiscernibility. Beginning with purely non-linguistic claims, such thinkers have suggested that there are some fairly strong consequences for the formal system of science that follow from the non-linguistic claims. Hellman and Thompson, for example, appear to suggest that from the physicalist view that "the physical facts determine all the facts" (i.e., ontological determination) it follows that "the physical truths determines all truths about nature".⁶⁴ As noted above, such a view is, by itself, too weak to satisfy the criteria concerning explanation. In addition, Hellman and Thompson explore the idea that their weakened form of physicalism leads logically to ex-

⁶⁴ See Hellman and Thompson (1975) p. 553.

plicit, physical definability of terms in the formal system of science: i.e., it is suggested that a set of purely non-linguistic theses may, in the presence of non-controversial assumptions, lead to strong linguistic claims concerning the structure of scientific knowledge. Hellman and Thompson claim to have shown that such an argument, although very interesting, ultimately fails.⁶⁵

However, other physicalists have more or less directly suggested that non-linguistic physicalism does have certain strong consequences for the formal system of science. Kim, for example, has argued that, given the supervenience thesis, there must be nomological correlations between physical and non-physical attributes and, hence, that the physical definability of non-physical terms is possible; this, of course, reopens the door to the derivation and explanation of non-physical laws by physical laws.⁶⁶

Less directly, Field and Friedman, beginning with reflections upon the ontological view that mental states are "functional states" that may have multiple physical realizations, have generalized classical linguistic reductionism in different ways.⁶⁷ The strategic suggestion is that, as our conception of the kinds of systematic correlations between physical and non-physical phenomena evolves, our conception of

⁶⁵ See Hellman and Thompson (1975), p. 561-3.

⁶⁶ See Kim (1978).

⁶⁷ See Field (1975), Friedman (1975).

the formal structure that reflects these relations can and must evolve concordantly. Whether Field and Friedman believe that there are logical arguments from non-linguistic to linguistic theses is not clear from their writings; they do appear to believe that there are stronger non-linguistic theses than supervenience which are plausible (i.e., certain kinds of nomological correlations) and that the existence of such correlations opens the door to a revitalized form of linguistic reductionism.

Finally, Putnam has contended that a purely ontological form of physicalism that eschews nomological correlations of physical and non-physical attributes and associated linguistic relations is untenable: as he puts it, "type-type correlations" are required as a part of a minimal physicalist position.⁶⁸ Again, such correlations invite consideration of strong physicalist theses concerning definition and derivation in science.

To this point, we have seen that there are, at least, two brands of non-linguistic physicalism advertized in the literature. The first, advocated by Quine and Davidson, requires supervenience without calling for nomological connections between physical and non-physical phenomena; as a result, the formal connections between various branches of science need only be quite minimal. The second view, advocated by

⁶⁸ See Putnam (1979). It is not entirely clear from his discussion why he believes this and whether his emphasis is on explanatory concerns or more fundamental epistemological concerns.

Kim and suggested by the work of Field, Friedman and Putnam, holds, in addition to ontological and supervenience claims, that such claims entail that there are nomological correlations between physical and non-physical phenomena and that these are expressible in the form of explicit definitions, thus imposing stronger requirements upon the formal structure of science.

What will be of interest to us in this section are the ideas that (i) supervenience theses entail nomological correlations between the supervening properties and the properties supervened upon, and (ii) there are arguments leading from such non-linguistic theses to strong linguistic theses concerning the structure of science. Thus, we are interested in the idea that apparently minimal formulations of physicalism (e.g., those of Quine and Davidson) are seriously underestimated by their proponents.

The key question that we shall address is whether a purely non-linguistic formulation of physicalist doctrine is adequate by our criteria. As we have already observed, the first form of non-linguistic physicalism is clearly not adequate because it fails to impose any structure upon scientific knowledge such that the explanatory goals of the program are attained. There are two lines of reply to this charge. The first is to challenge the criteria of adequacy as too restrictive; as we have already acknowledged, the criteria are proprietary but well motivated. The second reply is to adopt the other view regarding

non-linguistic physicalism; that is, to adopt the view that a purely non-linguistic formulation of physicalism is adequate because (i) it explicitly captures the ontological views of the physicalist and (ii) it has consequences regarding the structure of scientific knowledge that are sufficient for satisfying the remaining criteria.⁶⁹

To summarize: a purely non-linguistic formulation of physicalism is adequate if and only if it has consequences which are jointly sufficient for satisfying the criteria of adequacy; the criteria can be satisfied only if a version of physicalism involves fairly strong claims regarding the formal structure of science. Hence, the Quine-Davidson brand of non-linguistic physicalism is not adequate by the criteria, assuming they are right that it has no strong consequences concerning the formal structure of science. However, this raises the question of how certain non-linguistic theses that are generally agreed upon by all physicalists (e.g., supervenience) are logically related to linguistic theses concerning science. If non-linguistic and linguistic theses are logically independent of each other, then a purely non-linguistic formulation of physicalism must be counted as not adequate; if, on the other hand, they are not independent, then a "purely non-linguistic" formulation could be counted as adequate if it

⁶⁹ Such a reply is somewhat paradoxical since it involves showing that a "purely non-linguistic" formulation is adequate because it has consequences for the formal structure of science (i.e., it is not really a purely non-linguistic view). The point may better be conceived in terms of the dependence of the one type of thesis upon the other.

had the right logical consequences.⁷⁰

In the remainder of this section, we shall briefly describe two lines of argument that have appeared in the literature which bear on the issue as we have just formulated it. Specifically, Kim⁷¹ has presented an argument designed to show that, beginning with an assumption of the supervenience of one class of attributes upon another (e.g., the non-physical attributes supervening upon the physical attributes), it can be inferred that (i) there are nomological correlations between attributes in the supervening class and attributes in the supervenience base, (ii) there are definitions, in a sufficiently rich language, of terms designating the attributes in the supervening class by terms designating the attributes in the supervenience base, and (iii) there are resulting derivations of theories concerning the supervening attributes from theories concerning the attributes supervened upon. The argument presented for these conclusions is premised upon the assumptions that the attributes in the supervening class are instantiated and that the attributes in the supervenience base are finite in number. An interesting line of future research would be, if Kim's argument is sound, whether it can be generalized to cases in which not

⁷⁰ The reader is reminded that the consequences in question are those which follow in the presence of modest assumptions about the language of science and its interpretation. Also, "linguistic" formulations will include theses concerning explanation formulations; thus, derivation and definition are not the only pertinent relations between linguistic objects in science.

⁷¹ See Kim (1978).

all attributes in the supervening class are instantiated and in which the supervenience base is not of finite cardinality.

A second line of argument, presented by Hellman and Thompson,⁷² would appear to close off the prospects for successfully generalizing Kim's argument. Essentially they argue from an assumption of supervenience⁷³ to the claim of the implicit definability of terms expressing non-physical attributes by terms expressing physical attributes.⁷⁴ They then suggest that an application of the Beth definability theorem would appear to lead straightforwardly to the explicit definability of the non-physical terms by the physical terms, and hence, to the derivability of theories couched in the former vocabulary from theories couched in the latter vocabulary. However, they argue against this move on the grounds that the Beth theorem is not applicable in the case of physicalism because the class of models which constitute the "standard" models for scientific theories is a proper subclass of all the models for those theories: i.e., there are "standardness" requirements on what counts as an acceptable model for scientific theories, and such requirements effectively rule out some models of such theories as not acceptable. Since the Beth theorem only applies to theory classes (i.e., the class of all models for a theory), it is not appli-

⁷² See Hellman and Thompson (1975).

⁷³ They do not make this assumption entirely explicit, although it is definitely suggested by their discussion.

⁷⁴ A thesis they call "the physical determination of reference".

cable in the case of physicalist science.

We shall not pursue further an assessment of the merits of either Kim's or Hellman and Thompson's arguments, although such pursuit is an important area of future inquiry. Such inquiry is not required for current purposes because, no matter how it turns out, our arguments above against purely non-linguistic versions of physicalism suffice to show that linguistic theses are required for any adequate formulation of physicalist doctrine. If linguistic theses are consequences of non-linguistic theses (plus modest background assumptions) so much the better; but, such a relation is not required: what is required is that linguistic theses feature in a formulation of physicalism.

2.3 PURELY NON-REDUCTIVE FORMULATIONS

We now turn to a discussion of purely non-reductive formulations of physicalist doctrine. Recall that a non-reductive relation between two classes is one that does not involve any specific mapping between the members of the classes; rather, the relation involves "global" characteristics of the classes such as simplicity, explanatory power or some kind of systematic covariation of the properties of the members of the two classes (e.g., as in truth determination).⁷⁵

⁷⁵ Non-reductive truth determination does not involve any particular mapping between sentences of the two classes (mappings do exist of course). Rather, it involves systematic covariation of the truth values of the sentences in the two classes: once the truth values of the sentences in the determining class are fixed, the truth val-

The problems with this form of thesis with regard to the adequacy of a formulation of physicalist doctrine are apparent. Because such a view does not presuppose specific mappings between elements of the two classes involved in the relation, this type of position cannot be an adequate expression of any physicalist concern that presupposes such a mapping: for example, (i) ontological views which identify or otherwise associate individuals or attributes in the two classes and (ii) views concerning explanation of regularities or instances of regularities in one class by specific explanatory principles in the other class. We take this shortcoming to be sufficiently obvious as to not warrant further development.

2.4 PURELY REDUCTIVE FORMULATIONS

To complete our discussion of the "pure forms" of physicalist thesis, we shall make a few comments regarding purely reductive formulations. We remind the reader of the distinction between reductive relations as we are defining them and "classical reductionism." The former is any relation between two sets of objects which involves a specific mapping from one set to the other; clearly, this is an exceedingly weak notion, and the formulation of interesting physicalist reductive theses will depend upon the introduction of constraints upon the mappings. "Classical reduction", on the other hand,⁷⁶ is an instance of two reductive relations introduced for spe-

ues of the sentences in the determined class are fixed.

cific purposes (e.g., unification of ontology and unification of an explanatory system). The constraints on the mappings⁷⁷ are designed to isolate mappings which insure (i) the identity of individuals and attributes and (ii) the explanation of laws and their instances. As we saw above, these constraints do not isolate relations that realize the goals of the program; hence, the formulation was judged inadequate. Retaining the goals of the program, we saw the door opened for searching out alternative theses which would realize the goals. Thus, we have rejected classical reductionism without rejecting the physicalist program. Further, for present purposes, rejecting classical reductionism is not equivalent to rejecting reductive relations in general. Unlike the other pure cases, a purely reductive version of physicalism cannot be shown in general to lead to inadequate formulations. Whether or not a purely reductive formulation is required for all purposes we shall leave open, although it probably is not since the goals of objectivity may plausibly be realized by non-reductive forms of truth determination and supervenience.

To summarize the main results of this chapter, essentially the above described form of argument that we applied to classical reductionism⁷⁸ was applied to a variety of purely linguistic, purely non-

⁷⁶ I.e., explicit definability of terms and derivability of theories.

⁷⁷ I.e., nomological coextensiveness of terms and logical entailment of one theory by another.

⁷⁸ I.e., to show that it is not sufficient for realizing the goals of the program.

linguistic and purely non-reductive formulations of physicalist doctrine. The results were that each of these pure forms cannot lead to an adequate formulation; hence, it is to be concluded that any adequate formulation of physicalist doctrine must involve some linguistic, some non-linguistic and some reductive theses. In the next three chapters, we shall develop an adequate formulation of the doctrine which consists of these types of thesis.

3. THE PRESUPPOSITIONS OF PHYSICALISM

The objective of this and the next two chapters is to formulate an adequate version of physicalist doctrine; in so doing, we shall encounter a variety of objections posed by critics of physicalism over the years. Our task comes down to that of weaving a path through those objections and arriving at a formulation that satisfies our criteria of adequacy. Given our discussion in the last chapter, this will require that we formulate a doctrine that consists of a combination of linguistic, non-linguistic and reductive theses. Further, because a goal of the physicalist program is to describe a structure for all of natural science, our formulations must be pitched at a level of sufficient generality, to both retain significant content and yet accommodate variability in ontology and patterns of explanation among different branches of science.

Our plan is, first, in this chapter, to discuss the vexing problem of the identification of the physicalist bases; a set of three presuppositions of the theses will emerge as a result of this discussion. Such presuppositions will be seen as required by any significant formulation of physicalist theses, since the content of physicalism depends upon there being a principled identification of the physical bases and such an identification of the bases depends upon the truth of the presuppositions. Objections to the presuppositions will be formulated and defended against. Second, in Chapter 4, we shall formu-

late a set of theses which address the three areas of physicalist concern (i.e., ontology, objectivity and explanation) and which are responsive to the results of Chapter 2. Objections to the theses will be considered. Third, in Chapter 5, three metatheses concerning (i) the scope of the theses, (ii) their empirical status, and (iii) their methodological roles in science will be developed. Various objections will be formulated and defended against. Alternative metatheories to those commonly held for physicalism will be considered; and, as we shall see, the existing metatheses will require some revision. Fourth, the resulting physicalist doctrine, consisting of presuppositions, theses and metatheses will then be shown to be adequate by our criteria as well as responsive to the major objections that have been advanced against it. Finally, we shall make some observations concerning the acceptability of the doctrine and the problems and prospects for working out the physicalist program in science.

3.1 IDENTIFYING THE PHYSICAL BASES

Given that the general form of physicalist theses involves a relation between two classes of objects, one of which is designated as "the base class" and contains physical objects,¹ any adequate formulation of physicalist theses requires an antecedent specification of what "the physical" consists in. Further, because of the variability

¹ In this discussion, 'object' is being taken as a term picking out such diverse things as events, states, terms, theories, and objects (in a narrower sense).

in kinds of physicalist theses (e.g., linguistic, non-linguistic) and the variability of the categories within those kinds (e.g., events, states, properties; law statements, theory formulations, explanation formulations, terms), there is need for a specification of what the physical consists in for each kind of object involved in the formulation of physicalist theses. The purpose of this section is to provide an account of the physical bases required for the theses to be developed in the next chapter. Further, we shall make explicit a set of three presuppositions of such an account and defend them against objections.

As a preliminary, we shall present and elaborate upon a distinction, first introduced by Quine² and clarified by Hellman and Thompson,³ between ideology and ontology. Quine writes as follows:

Given a theory, one philosophically interesting aspect of it into which we can inquire is its ontology: what entities are the variables of quantification to range over if the theory is to hold true? Another no less important aspect into which we can inquire is its ideology...: what ideas can be expressed in it?⁴

And, again:

The ideology of a theory is a question of what the symbols mean; the ontology of a theory is a question of what the assertions say or imply that there is. The ontology of a theory may indeed be considered to be implicit in its ideology; for the question of the range of the variables of quantification may be viewed as a question of the full meaning of

² See Quine (1951).

³ See Hellman and Thompson (1977).

⁴ Quine (1951), p. 14.

the quantifiers.

In the foregoing paragraphs I have contrasted the ontology of a theory with the ideology of a theory. But the contrast carries over also into absolute terms; in absolute ontology we ask what there really is, and in absolute ideology we ask what ideas can legitimately be had, or what primitive ideas are given to us as a basis for thinking...I have described the ideology of a theory vaguely as asking what ideas are expressible in the language of the theory. Urgent questions of detail arise over how to construe 'idea'...Both ideology and ontology in their relativized aspects...belong to what is commonly called semantics.⁵

As can be seen, Quine's distinction is a 2x2 distinction: the first dimension being ontology-ideology and the second dimension being absolute-relative. In its absolute aspect ontology concerns what there really is. More specifically, it concerns what ontological kinds of things there really are; such ontological kinds include: physical, mental, attribute, event, individual, abstract, etc. In matters of ontology, the important semantical relation is that of satisfaction of ontological kind predicates; everything has some ontological status or other and some things may enjoy more than one such status (e.g., a physical attribute). For the purposes of the physicalist program, we are interested in (i) what the physical ontology really consists in, (ii) what the ontology of natural science (i.e., nature) really consists in, and (iii) what the relations between the ontology of all of natural science and the physical ontology really are; hence, we shall be concerned with ontology in its absolute aspect.

⁵ Quine (1951), p. 15.

Ontology in its relativised aspect need not be concerned explicitly with what there really is but only with structures which make a theory true under some interpretation. Quine's frequent excursions into Pythagoreanism are instances of exploration of the variety of minimal ontology required for making a theory true: that nature consists of pure sets is something that not even Quine should take seriously. What Quine probably does take seriously is the idea that that kind of ontological speculation is the only one likely to deliver any useful results: absolute ontology may be, on Quine's view, a fruitless enterprise. The assumption of our work is that, although there may be no saying "absolutely" what there is, there are meaningful intermediate alternatives to that enterprise and the kind of inquiry that Quine engages in (i.e., ontological reductions constrained only by preservation of truth under an interpretation). Physicalism is a doctrine concerned with characterizing what there really is in nature; how to construe the "really" is a matter we shall return to below.

Regarding ideology, there is a similar distinction between relative and absolute. In its relative aspect, ideology concerns what ideas can be expressed in the language of a theory: what do the symbols mean?, which symbols are fundamental and which are derivative? In its absolute aspect, ideology concerns more general epistemic and cognitive matters: what ideas can legitimately be had? which ideas are given as a basis for thought and which are constructed out of them? The important semantic relation involved in matters of ideology is that of ex-

pression by a predicate; hence, it is only attributes that enjoy ideological status.⁶ In the current project, we shall be concerned only with ideology in its relativized aspect. We shall be concerned with the specific languages employed in science: what are the constituents of those languages and what are their structures?, what attributes are expressed by the predicates in those languages?, and how do the different languages relate to each other? In the discussion below, our specific focus will be on identifying and relating the predicates and other general terms employed in the development of the various theories found in science.

For expository purposes, we shall extend Quine's distinction to include what we shall call "doctrine". In its relativized aspect, doctrine concerns what true sentences are expressed in a given language. Further, we shall be concerned with what explanations and laws are expressed in a language, and these will both be included under the heading of "doctrine".⁷ In its absolute aspect, doctrine may be understood to concern the ultimate and total truth about reality; since our chief concern vis a vis doctrine will be with the structure of science, we shall be exclusively concerned with doctrine in its relativized aspect.

⁶ See Hellman and Thompson (1977).

⁷ We distinguish sharply between truths, theories, explanations and laws on the one hand, and true sentences, theory formulations, explanation formulations and law sentences on the other. In matters of doctrine, it is only the latter that shall be considered.

Given this tripartite distinction between ontology, ideology and doctrine, we shall now turn to the problem of characterizing the physical bases for ontology, ideology and doctrine that will play critical roles in the formulation of physicalist theses concerning ontology, objectivity and explanation. As we develop the characterization of the bases, we shall distinguish the various categories within each.⁸ The primary problem we shall encounter will be that of identifying the bases in a "principled" way which does not trivialize the theses or make them obviously false: the most telling criticism of physicalist doctrine regarding its formulation that we shall encounter is that there is no principled identification of the bases, and hence, the theses are trivial because they can always be saved by ad hoc modification of the bases. Our task, therefore, is to fend off this objection by showing how the bases can be specified in a principled way.

What then is a good strategy for achieving this goal? Most authors, not always for the same reasons, have attempted to circumscribe the physical bases by first isolating the physical ideology and doctrine, and then giving a characterization of the physical ontology that is derivative. Although there may be alternative strategies,⁹ we shall employ the more standard strategy because (i) it looks like the three

⁸ For example, within ontology, we distinguish classes, attributes and individuals.

⁹ For example, to identify the physical ontology and then characterize the physical ideology and doctrine in some derivative way; or to identify the bases independently of each other. See Cornman (1971) for an example of one such alternative strategy.

bases must be interdependently characterized and (ii) it does not appear likely that one can get very far in the attempt to characterize the physical ontology without relying heavily on what one takes to be physics and its referential vocabulary. Thus, our approach will be to attempt to provide a principled identification of the physical ideology and doctrine, and then to develop the physical ontology in terms of them.

We shall begin by looking at a distinction, drawn first by Meehl and Sellars,¹⁰ between "physical1" and "physical2" terms as follows:

physical1: terms employed in a coherent and adequate descriptive explanatory account of the spatio-temporal order.

physical2: terms used in the formulation of principles which suffice in principle for the explanation and prediction of inorganic processes.

This distinction forms the basis for distinguishing two different types of physicalist proposal. Physical1 was intended to capture the full vocabulary of natural science; any term of natural science that is applicable to some region of "the spacetime causal order" (i.e., nature) falls within the category of physical1. Hence, terms of physics, chemistry, biology, psychology and any other natural science are all physical1.¹¹ The physical doctrine, then, would consist (roughly)

¹⁰ See Meehl and Sellars (1956).

¹¹ On even broader construals, any terms that are applicable to regions of spacetime, whether they are scientific terms or not, are physical1. See Cornman (1971), Davidson (1970), and Malinas (1973)

in the theories that are formulated in these terms and accepted as true. Physicalism based upon this conception of the physical is a pretty mild doctrine, although its defenders do not consider it trivial.¹² On our view, however, this brand of physicalism is much too weak; because everything appears to fall within the bases, it need not introduce any requirements within the formal system of natural science for the kind of structure that is needed to realize the goals of the program.¹³ The challenge that this weak physicalism presents is to develop a narrower conception of the physical ideology, ontology and doctrine which is cogent and which supports strong physicalist theses.¹⁴

for examples of this construal. Thus, there are at least two senses of physical: (a) terms applying to things located in spacetime; (b) terms of natural science applicable to things in spacetime.

¹² Given our earlier discussion of the two conceptions of natural science (i.e., (i) it's all physics, (ii) there are divisions between physics and the rest), this form of physicalism is one that could be expected from someone who believed that the branches of natural science cannot be individuated in any principled way. The physicalism we are concerned with is one which accords "physics" a privileged place in science; and, it is this privileged status that weak physicalists appear to claim cannot be made out in a cogent way because there are no principled divisions between physics and the rest of science.

¹³ For example, explanatory relations between the phenomena and laws studied in the special sciences and the phenomena and laws studied in some basic science.

¹⁴ I.e., theses concerning the relations between the ideology, ontology and doctrine of any part of science to the ideology, ontology and doctrine of the "physical" part.

Therefore, because our interest is in this stronger form of physicalism which is based upon a conception of science in which there are distinct branches, one of which (i.e., physics) is to be accorded a privileged place with respect to ideology, doctrine and ontology,¹⁵ we shall proceed in our efforts to identify the physical bases by attempting to find a principle for distinguishing physics from other branches of science. Once this has been accomplished, the bases for ideology, doctrine and ontology will be fully developed.¹⁶

As a first attempt at solving this problem, one might say that physics is whatever it is that physicists do and the physical ideology consists in whatever terms are typically used by physicists and are found in physics texts when physical doctrine is being presented. The physical ontology on this approach is whatever is the intended interpretation of physical terms and doctrine: that is, intended by the physicists. This proposal amounts to "leaving it to the experts" to

¹⁵ Physical2, as we shall see below, is a proposal that supports the second conception of science in which there are principled divisions.

¹⁶ We are here struggling with two deep and difficult issues in the formulation of the physicalist doctrine: (1) how to give a principled identification of the physical bases which are required for the formulation of significant physicalist theses and (2) how to do this without landing in a vicious circularity in which physics is characterized by the terms employed in the formulation of physical theory and those terms are characterized by being the terms that occur in the theories of "physics." The physical1 criterion breaks the circularity at the expense of the significance and strength of the doctrine, something we do not want to do. Hence, we have rejected physical1 and are now moving on to find a criterion for physics which breaks the circle and preserves the power of the doctrine.

determine what physics and the physical ideology, ontology and doctrine are.¹⁷ Unfortunately, such an approach is totally unacceptable for reasons that we shall roughly characterize now and discuss more fully below. For openers, this view totally dodges the critical problem of offering a principle for identifying what physics is¹⁸ that is at all relevant to the metaphysical and epistemological concerns of the physicalist. The principle offered is completely vulnerable to the objection that what counts as physics is based upon arbitrary administrative decisions or other forms of "socio-historical accident". For example, who is identified as a "physicist" may be due to arbitrary decisions having to do with how to organize and run a university.

Second, the principle leaves open whether it is current physics, future physics, or some ideal physics that is to play a role in the characterization of the bases. If it is current physics, then there would appear to be no room for the growth of physics from the point of view of any particular formulation of physicalism. Hence, current physics must be true and hence it would appear that physicalism is obviously false.¹⁹ If it is some future physics that is intended, no principle for identifying physics has been provided that constrains the evolution of "physics" so that ad hoc modifications of physics de-

¹⁷ See Hellman and Thompson (1975), Friedman (1975), and Boyd (unpublished) for instances of this approach.

¹⁸ And, for who are the physicists.

¹⁹ See Smart (1978) for endorsement of the view that it is current physics that is pertinent.

signed to save the physicalist doctrine can be ruled out; without some such principle, the doctrine has seemed to some to be a trivial doctrine.²⁰ In a nutshell, the problem is that unless there is some antecedently specifiable principle for identifying the physical bases, physicalism cannot be formulated in a significant way. Given our current strategy, this comes down to requiring a principle for the principled identification of physics. This, of course, was the conclusion we reached above in discussing physicalism. Now we have reached it from the point of view of a certain class of objections that any formulation of physicalism must deal with. The problem to be confronted is not a small matter: a principled identification of the physical must be developed which (i) can function to isolate the physical bases for ideology, ontology and doctrine in a metaphysically and epistemologically relevant way and (ii) can accommodate the changes in our conception of what the physical consists in as our knowledge grows.²¹ It is this problem that we are trying to solve now as we search out a principle for identifying physics. In this search, we conclude that iden-

²⁰ See Chomsky (1968).

²¹ It is important to distinguish between (1) a change in our conception of what physics or the physical is from (2) a change in physics; it is the latter that should be accommodated while the former remains fixed. It is also possible, of course, that our conception of what physics is may change; but, in formulating physicalist doctrine, it is only required that a fixed conception of the physical that leaves room for change in what we take to be the extension of 'physical' be developed. Given this distinction, our problem at this point is to try to develop a conception of what physics, and thus the physical, is which remains fixed while physical theory changes. Dealing with potential changes in such a conception is a separate problem.

tifying physics with "what the physicists do" is a non-starter because it is much too vaguely stated and because all suggested refinements are vulnerable to objection.

Physical2 was an early attempt to characterize the theoretical vocabulary of physics and chemistry in a way which leaves open the possibility of principled extensions of that vocabulary; physics and chemistry are, according to this conception, the branches of science concerned with the explanation of all inorganic processes. First, however, as a characterization of these branches of science, physical2 suffers from the defect of not giving an independent characterization of what 'inorganic' means; hence, the major problems for the physicalist may have simply been pushed back a step. That is, if the question for the physicalist is, "What is a correct conception of "the physical" and what correctly falls under that concept?", then, the question seems to have been converted to, without any gain in clarity or solution, "What is a correct conception of "the inorganic" and what correctly falls under that concept?". Although we don't believe that this question is unanswerable, we shall leave it to the proponents of physical2 to do the answering.

Second, physical2 is dissatisfying because it lumps together physics and chemistry; the physicalist who is according to physics a special place in science should be seeking a characterization only of physics in his attempt to isolate the bases for ontology, ideology and

doctrine.²²

A third objection²³ is that there are certain developments in science which could prove highly embarrassing for proponents of physicalism. Thus, if our empirical psychology evolves to the point that it is clear and well established that certain highly complex machines are capable of "mental" activity (e.g., robots that can think), then it appears that purely inorganic processes would exhibit properties that, under nobody's construal of physics and chemistry, can be accounted for by those disciplines. Thus, certain features of a class of inorganic objects would not be explicable within the confines of physics and chemistry as they are usually conceived. Responding to this problem with the claim that physics and chemistry include psychology as a result of the alleged possibility is exactly the kind of move the physicalist must try to avoid, since it is preservation of the boundaries between branches of science that gives the physicalist doctrine its bite. A better response, therefore, is the rejection of physicalism as a characterization of basic science with the new understanding that such categories as "organic" and "inorganic" are probably not going to prove to be the metaphysically and epistemologically interesting ones from the point of view of physicalist science.²⁴

²² Whether one sees this as a flaw will depend upon how fine grained ones individuation of the sciences is. On our view, classical reducibility, for example, is not sufficient for inclusion of one branch of science (e.g., chemistry) within another (e.g., physics).

²³ This objection was suggested by Ned Block.

A final objection to physical2 has been suggested by some comments of Chomsky's²⁵ and developed by Block as follows:

Briefly, it is conceivable that there are physical laws that "come into play" in brains of a certain size and complexity, but that nonetheless these laws are "translatable" into physical language, and that, so translated, they are clearly physical laws (though irreducible to other physical laws). Arguably, in this situation, physicalism could be true - though not according to the account just mentioned i.e., physical2 of physical property.²⁶

Bypassing the issues raised by appeal to translation into a physical language, the suggestion here is that the notion of physical law need not be restricted to phenomena of a certain degree of complexity of organization, and especially, it need not be restricted to "inorganic" phenomena. Rather, there is reason to speculate that which physical laws are actually operating at a given stage of the universe depends upon the degree of organization and complexity to which it has evolved; and, in brains of a certain size and complexity, physical laws operate that do not operate at lower levels (e.g., inorganic) levels of complexity. If this speculation is correct, then physical2 as a characterization of the physical is clearly defective. Of course, the burden of the objector is twofold: to develop an alternative con-

²⁴ As we shall see again below, it is important that the physicalist program not be saddled with too many harsh violations of our intuitions about the differences between the mental and the physical; if such traditional mental concepts and phenomena as qualia and mental content become located in the physical bases, something has gone radically wrong with the "physicalist program."

²⁵ See Chomsky (1968), p. 83.

²⁶ See Block (1980), fn. 4.

ception of the physical that holds up under scrutiny and to establish the truth of the speculation just described given that alternative conception.²⁷ This objection, in addition to undermining physical2, reveals a general constraint on any characterization of the physical: viz., that it be compatible with the idea that there are emergent physical laws.

Given the objections just reviewed, physical2 appears to be an entirely unacceptable account of the physical. However, let us note the good points about the proposal: (1) it suggests a principled identification of physics (and chemistry); (2) it is an attempt to isolate a vocabulary that is narrower than the physical1 vocabulary and hence could serve as a basis for significant physicalist proposals; (3) it attempts to provide a principle for controlling the admissible extensions of physics and of the physical2 vocabulary; (4) hence, the growth of physics is compatible with a meaningful formulation of physicalist doctrine; and (5) the principle provided, although defective, does appear to be capable of being metaphysically and epistemologically relevant to the physicalist program. The characterization of physical2 is capable of such relevance since (a) it makes appeal to the notion of an inorganic process and (b) it makes appeal to the idea of explaining all such processes. First, metaphysical relevance comes from isolating a class of processes which will play a central role in

²⁷ The objector must do both because the objection depends upon an antecedently understood conception of the physical.

the ontological theses (e.g., all entities, processes, etc are built up or depend upon the inorganic entities, processes etc); as we saw, however, the current proposal was in certain respects defective in this respect, although on the right track. Second, epistemological relevance comes from isolating a class of explanatory principles that advert to the basic class of entities; such principles play a crucial role in the theses concerning objectivity and explanation. That they are principles concerning the basic processes is what links the ontological and epistemological concerns of physicalism together. Physical2, despite its fatal flaws, teaches us alot about how to proceed.

Another characterization of physics has been suggested by Quine in a number of recent papers.²⁸ On his view, physics is the branch of science whose goal it is to discover the minimum set of scates such that there is no difference in nature without some difference in those basic states. Although this characterization is of interest, especially to the physicalist who holds to ontological determination as we do,²⁹ and, although it may be a correct, albeit highly abstract, partial characterization of what physics is, it is not detailed enough to identify the subject matter of physics or the questions and kinds of answers with which it is concerned. We shall be asruming that an adequate characterization of any discipline must at least stipulate the

²⁸ See Quine (1975a, 1978).

²⁹ See below, Chapter 4.

characteristic questions and answers of that discipline.³⁰ Thus, it is not clear that Quine's characterization successfully distinguishes physics from a different science (e.g., psychology) which may have the same feature of indiscernibility of any difference without a difference in one of its states.³¹ Further, it should become clear as we proceed that if the characterization of physics that we consider below cannot be made cogent then neither can Quine's and for similar reasons. So, let us turn to a consideration of a final proposal for what physics is.

A third characterization of physics is as follows: physics is the branch of science that applies to everything. For reasons that are by now familiar, it won't do to cash this in as: physics is the branch of science that provides a description of everything. An alternative rendering is provided by the work of Hilary Putnam,³² in which he sug-

³⁰ We are assuming a strong criterion of adequacy here in conformity with our general policy of beginning with strong criteria and weakening them only if necessary; as a consequence, the most interesting results will be obtained.

³¹ See Goodman (1979) for expression of this claim. It is important to keep in mind that it does not follow from the fact that, say, physics and psychology both provide a basis for the principle of indiscernibility, that physics and psychology constitute equivalent bases for every purpose; in particular, they are not equivalent for all ontological or epistemological purposes. Further, Ned Block has suggested that the alternative indiscernibility claims are not serious competitors. Whereas the physicalist claim is "internal" to our theory of nature, the psychological indiscernibility thesis is either "external" to our theory of nature or, if internal, obviously false. We shall discuss below (in chapters 4 and 5) some of the issues raised by this suggestion.

³² See Putnam (1970).

gests that physics is the branch of science concerned with identifying a basic class of physical magnitudes which are such that there are a set of principles couched in terms of those magnitudes which are sufficient for explaining the composition, structure, dynamics and interactions of all things with respect to those magnitudes. The distinguishing feature of this class of magnitudes and the associated principles is that everything has the properties associated with those magnitudes and everything satisfies the principles with respect to those magnitudes. Physics just is the branch of science concerned with identifying those magnitudes and principles; it is their complete generality in this sense which distinguishes them from other properties and principles.

Further, the principles are supposed to account for such features of all things as (i) the composition of all things in terms of the basic constituents, (ii) the dynamics of all systems in terms of the basic magnitudes (i.e., how do things evolve over time?), and (iii) the interactions between things (including all causal interactions) with respect to the basic magnitudes. Hence, physics has a set of questions the answers to which are constrained by the requirement of full generality of the explaining principles and the properties adverted to by those principles. Hence, any body of theory that provides answers to these questions and that satisfies this condition would count as physics.

The ontological and epistemological relevance of this proposal comes from its specific concern with such questions as (1) what are the fundamental constituents of matter?, (2) what are the processes that underlie all causation and all interaction?, (3) what parameters are relevant to describing the dynamic unfolding of all systems in nature and, hence, all change?

The conception of the physical here is that something is physical just in case it is among the basic constituents of all matter or among the fundamental processes on which all causation and interaction depends or among the basic magnitudes in terms of which the dynamics of all systems can be characterized. Given this, there is as yet no commitment to what particular terms or theories nor to what particular entities, processes or magnitudes fall within the category of the physical; hence, this characterization is quite compatible with the growth of physics and with the possibility of ontologically emergent physical phenomena. Further, since the principle is relevant to the physicalist program, if it is a cogent principle it provides a principled way of distinguishing physics from the rest of natural science. Hence, if it is cogent, then it provides a basis for formulating significant physicalist theses which are neither trivially true nor obviously false. So, if the principle is cogent, it is what we are looking for.

But, is it cogent? To answer this, a number of more specific questions arise: (1) Is physics anything which satisfies this principle?, (2) Does the principle guarantee that there is one physics? Should it?, (3) Does it lead to the conclusion that what is our physics might not be physics in some other possible world? (i.e., is physics in our world necessarily physics?), (4) Does it really provide a way of ruling out ad hoc modifications of physics to save physicalism?, (5) Does it not make physicalism trivially true?, (6) Is the principle subject to change in ways that could be appealed to to save physicalism? In what follows, we shall address these questions by considering a number of objections to the proposed principle for identifying physics.

Physics is that branch of science that is concerned with studying the fundamental constituents of all matter, the fundamental processes and magnitudes in terms of which all interaction and dynamics can be characterized; the constraint on the inquiry is that the principles postulated and the attributes and entities posited must be completely general. However, it would seem that this principle allows for a number of different physics to be possible. That is, there is no guarantee that this principle picks out a unique body of knowledge about the natural order. A variant objection is that this principle allows that in our world physics may be one thing and in a different possible world physics may be something completely different. Although these are separate objections, they can be dealt with in exactly the same way. Thus, physics just is anything which satisfies this principle,

so that (1) there may be many physics in this world and (2) there may be radically different physics in different possible worlds. Non-uniqueness is not a problem for the physicalist as long as, relative to a given physics, the total theory of nature (ours or some other) is structured in accordance with physicalist principles.³³

A second objection is that this principle does not rule out certain unacceptable conjunctions, say, between physical theory and Hegelian metaphysics, in which the physics does the work and the metaphysics is just additional fluff. But, this objection fails for two reasons: (i) the principle may be seen to rule out the unwanted addition on the grounds that it is not relevant to the goals of physics as characterized and (ii) general scientific methodology would certainly come into play in ruling out the additional stuff on simplicity grounds and the like. In the background of the principle is the idea that it is only a minimal such theory which suffices to achieve the goals of physics that we count as physics.

A related and more important objection is that physics is a branch of science which is a conglomeration of many different theories that are of radically different degrees of generality and abstraction and that address radically different questions (e.g., quantum mechanics, astronomy, optics, thermodynamics); thus, it is concluded that phys-

³³ The physicalism we are concerned with is to be seen as an abstract structural feature of scientific views and theories of nature; there is no good reason to believe that there is exactly one such view or one such theory.

ics, as it currently exists, does not satisfy the characterization we have offered. Our brief reply to this objection is that (i) the objection is correct given the rough statement of our view and (ii) when we speak of physics we shall mean a body of theory that constitutes an idealization of a certain sort. Some theories that are currently included under the heading of "physics" may be seen as not part of the basic level of the ideal theory. Thus, within physics as it is currently conceived, there are different levels of organization, generality, abstractness of phenomena and associated theory. When we speak of "physics", we shall be referring to the, ideally, basic theory.

Further, it could be objected that our conception of physics is subject to ad hoc revision to save physicalism. We shall look at this more closely below. For now, we note that again general scientific practice should come into play to rule out ad hoc revisions of any scientific concept, principle or theory. An ad hoc revision is barred on the grounds of "ad hocness"; a non-ad hoc revision should be considered on the merits of the case. It is not clear to me that any principle ever has built into it a means for protecting itself against abuse; abuse is ruled out on general grounds. If we can clear a principle for identifying physics of the charges of unclarity and trivialization, and it does the work we want it to do, then we have done all that we have to. That we might decide to revise it simply to protect our doctrine is not something we need specifically promise not to do in order to use the principle. Below we shall consider Chomsky's cit-

ing of the case of electromagnetic theory which is supposed to be an example of how maleable our conception of the physical is. In fact, it was not a case of a change in our conception of the physical or of what physics is, but rather it was an instance of a change in what falls under those concepts.³⁴

Another objection is that, granted that the concept of what physics is is okay, it is still possible to modify what we take to be physics, consonantly with our concept of physics, in an ad hoc way to save the doctrine of physicalism. This is just what the case of electromagnetic theory is supposed to show: physics may be just as the principle describes, but what falls under the heading "physics" is sufficiently maleable so as to save the doctrine. In short, the objection is that we can count anything as a fundamental constituent, magnitude or process just by deciding to include it among the constructs of what we call "physics". The correct reply here is essentially the same as above: ad hoc modifications are ruled out by general principles of scientific procedure; non-ad hoc revisions ought to be considered very seriously. Surely, the physicalist must allow that what counts as physics is an evolving part of our knowledge; what are the basic magnitudes (etc) is a matter of continuing discovery. Allowing this does not in any way commit us to holding that any modification we like is acceptable if it is needed to save physicalism; it simply is not true

³⁴ The above objection should not be confused with the next one; viz., that we can revise physics in an ad hoc way to protect the doctrine.

that anything goes in science.³⁵ The problem is one of being able to identify the ad hoc revisions in knowledge from the non-ad hoc ones; this I am happy to announce is not my problem here.

Finally, let us consider the objection that runs as follows: the conception of physics that is being offered is one which guarantees the truth of physicalism. This objection must depend to some extent upon what the theses of physicalism are, theses that have not been presented yet. However, it shouldn't be too surprising to anyone that the characterization of what physics is is closely related to some of the theses of physicalism since the point of physicalism is "to accord to physics its rightful (important) place in science". Having given this much to the objector, however, the burden still is on him to show that the theses offered below are trivialized by the characterization of physics given here. We shall argue below that the objector cannot carry this burden.

At this point, we shall move on the assumption that, at least a prima facie case has been made for the defensibility of our characterization of what physics is. So, physics is the branch of science the task of which is to identify and characterize the fundamental constituents of all things and the fundamental processes and magnitudes underlying all change and interaction. Which theory is it that answers to this description? We do not know; but our best current guess is

³⁵ Or philosophy for that matter.

that it is current physics (or some suitable elaboration of current physics, given that portions of current physics are quite underdeveloped or are quite tentative). We take it as obvious that we can accept current physics as the best approximation to the physics of nature without giving up the possibility of revising it tomorrow. Hence, our best approximation of the actual physical bases for ideology, ontology and doctrine is to be developed in terms of current physics: it is this belief that makes justified the current practice of most physicists to look to current physics in characterizing the bases.

Given a formulation of physical theory, how do we proceed toward giving a full characterization of the physical bases for ideology, ontology and doctrine?³⁶

We shall begin with ideology; assuming that we can isolate a basic stock of non-logical terms that are employed in formulating the theory and which express the fundamental magnitudes, the problem becomes one of specifying the definitional apparatus that can be legitimately employed in building complex predicates from members of the basic stock.

³⁶ 'Given a formulation' is supposed to leave room for the existence of alternative formulations of the same theory in different vocabularies and, as we allowed above, alternative physical theories. The issues of theoretical and empirical equivalence of theory formulations are not directly pertinent to the problem of formulating a physical basis, given such a formulation. In addition, we observe that given a formulation of current physics, it may be unclear how much is empirically well established, how much constitutes tentative working hypotheses, how much is heuristic, and how much is an artifact of the representational system used (as opposed to being strictly empirical and realistically interpretable).

It is quite beyond the scope of this project to actually extricate from a formulation of physical theory some stock of basic predicates and to rigorously characterize the full range of allowable linguistic constructions built up from the basic stock; but, we shall indicate the directions that this might go in.³⁷ In addition to the usual first order logical apparatus, there are two additions, as our discussion in Chapter 2 suggested, that should be considered: (1) higher order quantification and (2) the introduction of infinitary languages. We know of no principled reason for not allowing such enrichment of the logical apparatus, and the capacity for reference to higher order and infinitary properties was seen to offer the physicalist some advantages with respect to the expressive power of the formal system of science.³⁸

Allowing that the logical apparatus may be so enriched, the physical ideology looks something like this: (i) there is a basic stock of physical predicates and (ii) there is a set of predicates built up from the members of this basic set using the logical machinery decided upon. The members of this set constitute the physical ideology and all of the properties, states, etc expressed by members of the set are (ideologically) physical properties, states, etc: that is, they are the properties that the physical theory can advert to when describing

³⁷ See Hellman and Thompson (1975) for discussion of some restrictions upon admissible predicates.

³⁸ As we observed above (Chapter 2), there may be practical reasons for not employing infinitary languages.

and explaining aspects of nature. Whether the properties so expressed exhaust all of the (ontologically) physical properties is an issue that depends upon which logical apparatus is selected and what the physical properties are. As suggested in Chapter 2, this issue turns in part on considerations concerning the cardinality and definitional power of the language employed.

Given this view of the physical ideology, how is the physical doctrine to be specified? Again, our specification can only be very rough: all general and singular sentences formulable in the terms of the physical ideology and that are true in nature make up the physical doctrine. The class of sentences specified is certainly not assumed to be finitely axiomatizable. In effect, it constitutes the full theory of nature; and, it includes all lawlike as well as all contingent truths concerning past, present and future states of nature. When we eventually say that the physical truth determines the whole truth about nature, it is this class of true sentences that we will be referring to as the physical truth. Physical explanation formulations are drawn from this body of truths as well.³⁹

³⁹ Below, we shall develop an apparatus for representing nature in terms of a spacetime coordinate system; a characterization of the class of physical truths can be developed more rigorously than we have done here given that apparatus. See Quine (1975) and Friedman (1975) for how to do this.

Given the physical ideology and doctrine, there are at least two strategies for identifying the physical ontology: the first, which we have already discussed, is to identify the physical ontology with the class of things which satisfy or are expressed by one or another of the terms in the physical ideology. The difficulties with this approach were seen to be plausibly insurmountable for finitary languages and not definitively resolved for more enriched languages. As a consequence, we shall not pause over it further.⁴⁰

The alternative approach is to employ the physical ideology, to identify a basic class of physical entities, along with a variety of constructive apparatus to develop a specification of the full physical ontology; such a development must include a treatment of individuals, classes and attributes and, on our view, should be viewed as a specification of the "intended interpretation" of the language in which the physical doctrine is formulated.

The task of actually giving a full development of the physical ontology is an extremely involved undertaking which we can here only sketch in coarse detail; if nothing else is accomplished in this section, the reader should begin to appreciate the complexity and philo-

⁴⁰ See Hellman and Thompson (1975) and Boyd (unpublished) for discussions of the problems with this way of identifying the physical ontology. See our discussion above (Chapter 2) for comments on lines of reply invoking infinitary and higher order languages. Because of the boundaries set for the current project, it is not possible to definitively resolve here the question of how the employment of an enriched language impacts on the issues raised by the Replacement Thesis.

sophical delicacy involved in the performance of the task. No author has, to my knowledge, given a full and adequate treatment of the problem; and, it is fairly clear that the literature does not contain solutions to a variety of the more general problems that must be solved before this task can be adequately dealt with.⁴¹ With this said, we shall now proceed to outlining the kinds of problems involved and to giving a specification of the physical ontology which will serve our purposes below.⁴²

We shall view the task as that of specifying a structure that is to be identified with "the intended interpretation of physics" (i.e., nature); however, like any structure it may have numerous features that go beyond those required for interpretation of a particular language.⁴³ The task before us is to represent the physical features of this structure. The issues raised are quite general ontological issues concerning the nature of objects, events, states, properties, relations, and the variety of other ontological category that are pertinent to a full treatment of the problem of specifying "the furniture of the world". Again, we are here interested in specifying only the

⁴¹ E.g., the problem of how to individuate properties, objects and events.

⁴² See Causey (1977), Hellman and Thompson (1975, 1977), Fodor (1975), Oppenheim and Putnam (1958), and Quine (1975a) for treatments of this problem.

⁴³ For example, attributes that are realized by physical attributes may be aspects of the structure here described without being assigned to any term in the language of physical theory.

"physical furniture" so to speak. This is crucial because it is a constraint on specifying the physical bases that the theses of physicalism not be trivialized. If the theses are true, then there will be a significant sense in which the structure here specified is to be identified with all of nature (the so-called "natural order"); but, this ought not be obtained by simply building everything into the bases.

Our problem is to represent a structure that constitutes the physical ontology. In our exposition, we shall adopt a coordinate ontology; that is, an ontology of spacetime points which exhausts all of nature.⁴⁴ Each point in this space is uniquely specified by a quadruple of real numbers. By a "region of nature" we shall mean any set of spacetime points within the coordinate ontology. The adoption of a specific coordinate framework is not assumed to be unique; alternative frameworks are assumed to be isomorphic and, thus, mappable into each other in ways that preserve the important features of the representation. Hence, our ontological theorizing takes place within a four dimensional space which we take to be exhaustive of all temporal and spatial locations in nature; regions are taken to be four dimensional portions of this space specifiable in terms of sets of points.

⁴⁴ See Carnap (1967), Quine (1975b), Friedman (1975).

Given such a coordinate ontology, the strategy for specifying the physical ontology includes the following steps: (1) to determine the ontological categories that must be dealt with (e.g., events, states, properties, relations, objects, kinds); (2) to determine, for each such category, how to deal with it (e.g., to answer such questions as "what is an event?" and "how are events to be individuated?"); (3) to determine, for each category, a "core class"⁴⁵ of physical instances; and (4) to determine and use, for each such category, a constructive apparatus for specifying the full class of physical instances of the category, given the core class. The actual execution of this strategy would lead one along a path fraught with philosophical complexity and the tedious demands of technical rigor.⁴⁶ For our immediate purposes, we need not travel this path; however, the actual working out of the physicalist program would require this.

In what follows, we shall develop informally a characterization of the physical ontology in accordance with the strategy just mentioned. To begin with, it appears that an adequate specification of the physical ontology must include a treatment of both individuals and attributes. Among the individuals, the following ontological categories must⁴⁷ be considered: objects, processes, events and states. Among the

⁴⁵ E.g., using the physical ideology, the core class of physical properties might be all of the properties expressed by the attribute predicates in the physical vocabulary.

⁴⁶ See Carnap (1967) and Goodman (1977) for the best examples of attempts to implement such a strategy.

attributes, properties and relations are included. Further, it has been emphasized by both Quine and Hellman and Thompson that building a set-theoretic hierarchy⁴⁸ into the ontology of physics has its rewards. Whether or not the full set-theoretic hierarchy is desirable, it does appear that a characterization of classes of objects in nature (especially, natural kinds) is required. Thus, the following ontological categories should be treated in a complete specification of the physical ontology: objects, events, states, processes, properties, relations and classes. The need for all of these categories is based upon attempting to accommodate all of the standard ways of talking about ontology within science. Given this prima facie list of categories, it is, of course, open to reduce the list as Quine frequently shows us how to do.⁴⁹

The strategy we shall employ is to follow, in essential respects, Quine's recent treatment of ontological physicalism up to a point⁵⁰ and then depart radically. We shall deal with only a few of the ontological categories of interest.

⁴⁷ The grounds for including a given category concern the purposes that adverting to that category would serve. In specifying categories, we have tended to be overinclusive, perhaps.

⁴⁸ A hierarchy that is based upon either pure sets (Quine) or individuals other than pure sets (Hellman and Thompson).

⁴⁹ See Quine (1975a) for an elegant example of this.

⁵⁰ Roughly, the point at which he says that our physics need only be committed to the existence of pure sets.

The physical ontology on this view is developed as follows: first, as initial ingredients we have (i) the ontology of spacetime points and regions described above and (ii) the physical vocabulary distilled from a scouting of contemporary physics; second, the vocabulary items are taken to express magnitudes (with possibly infinite ranges of values) which include all of the "fundamental magnitudes"; third, the members of this class of basic magnitudes are evaluated⁵¹ at every region of the spacetime ontology; fourth, given the basic attributes and given a set of attribute forming operations⁵² that take attributes and generate complex attributes, a class of all attributes⁵³ constructible from the basic class is generated; fifth, these attributes are likewise evaluated at every spacetime region. The result is a "complete" distribution of all physical attributes over the entire spacetime ontology. On this view, the individuals are the regions themselves. Un-

⁵¹ Each value is taken to be a discrete physical attribute (property or relation). And, it is the actual value (not a measured value) that is pertinent here; there is no limitation based upon the precision of measuring instruments. Any real value within the range of permissible values is an attribute. Finally, the cardinality considerations discussed above are not pertinent here; the predicates are employed only to identify the fundamental magnitudes, not every one of their values. This approach clearly separates the issue of the defining power of the language of physical theory from the specification of the physical ontology. There need not be a term for each value of a magnitude although there must be an attribute for each value.

⁵² It is left open what operations are to be included in this set.

⁵³ It is a feature of our construal of the physical ontology that a specification of the basic attributes "determines" all the complex attributes in the basis; once all the basic physical attributes are fixed, then so are all the complex physical attributes. See Kim (1978) for discussion of this feature of the basis.

like Quine, we unashamedly admit attributes into our ontology: the class of such attributes consists of (i) all the fundamental physical properties and relations and (ii) every attribute constructible from the fundamental ones.⁵⁴

So, we have spacetime regions and attributes distributed over them. To many, this will seem too modest an ontology: where, after all, are the objects and events? It is at this point that we must conserve energy; our claim, which we shall not here defend, is that, given this characterization of the physical ontology, a more robust ontology can be built up in its terms depending upon what view one takes concerning what an object or an event is. Our motive for this way of proceeding is that the more, detailed and intricate issues concerning specific ontological categories, although interesting and important for some purposes, are not pertinent to ours, especially if the above claim (concerning construction of objects and events) is correct.⁵⁵

⁵⁴ In this, of course, we depart significantly from Quine who, when reaching this point in the discussion, opts for reconstrual of predicates to avoid quantification over states in favor of quantification over numbers; the "ontological debacle" he speaks of begins with this move (cf., Quine (1975a)). We avoid it because it appears that the purpose it serves (i.e., that of identifying the minimal ontological commitment a theory of physics need make) is not our purpose, which is that of identifying the physical ontology in some more substantial sense (i.e., "the real ontology of physics" that, for example, we take ourselves to be part of and embedded in.)

⁵⁵ See Cartwright (1975) for a discussion of material objects; and, see Quine (1975a) for suggestive discussion. As Quine points out, issues concerning the notion of a "body" are not directly pertinent to the problem of specifying the physical ontology. See Thomson (1977), Kim (1976), and Davidson (1970) for discussion of events.

For those who prefer to have set-theoretic structures close at hand, our specification of the physical ontology is extended to include a set theoretic hierarchy built upon the individuals of the ontological system (i.e., regions of spacetime). Hence, as Quine puts it, "we suffer no shortages" when it comes to our need for classes of objects.⁵⁶ Further, note that the fusions of sets of regions are themselves regions; hence, any ontological purposes served by studying fused objects can be quite adequately accommodated by our physical ontology. Given our construction, all attributes, classes and individuals that are required in physical science are included within the ontology here described; and, surely, this ontology exceeds all that is required for physics.

This completes our presentation of the physical bases. Before turning to a discussion of the physicalist theses which advert to them, we shall discuss a number of presuppositions of our account which have been the target of various objections in the literature.

Our development of the physical bases depended upon, at least, three critical assumptions which are correctly construed as presuppositions of that development, and hence, of the theses also:

(P1) There are principled divisions among branches of science.

(P2) There are determinate physical bases to be developed.

See Causey (1977) for discussion of kinds and of "structured wholes".

⁵⁶ See Quine (1975a) and Hellman and Thompson (1975).

(P3) Physics occupies a special place in science which justifies developing the physical bases in terms of its ideology, ontology and doctrine.

For each of (P1)-(P3), we shall discuss (i) its content, (ii) why it is a presupposition of our account of the bases, and (iii) the major objections that have been advanced against it.

Before proceeding to a discussion of these presuppositions, let us note the importance of what we are now doing for the defense of the physicalist program as we are developing it. The following is a characteristic form of argument which is implicit in the critical discussions of a number of writers on physicalism:

(A) The theses of physicalism depend for their content upon the determinate and principled identification of the physical bases.

(B) The determinate and principled identification of the physical bases depends upon the truth of presupposition x.

(C) Presupposition x is false.

(D) Thus, there is no determinate and principled identification of the physical bases.

(E) Thus, the theses lack content.

Since our approach in developing the theses assumes that (A) and (B) are true, it is rather important to look closely at the most frequently attacked presuppositions and to defend against the arguments cited against them. In this way, we shall close off a number of lines of ob-

jection to the physicalist program. Our discussion of the three presuppositions we have identified will also make clear why we hold that (A) and (B) are true.

3.2 PRESUPPOSITION 1

What, then, is the content of (P1)? It is, roughly, that the institution of science has a certain structure: it is partitioned into more than one part and the basis for the partitioning is some principle (or set of principles) of metaphysical and epistemological significance to the physicalist program. As we shall see, for physicalist purposes of the kind that we are concerned with, it is only required that there be such a division between physics and the rest of science. Recall that our strategy for identifying the physical bases only assumed that we could isolate physics. Although this is all that is needed, it is possible that the principle(s) that we use to distinguish physics from the rest may suggest ways of making further divisions.

Which principle is operative in effecting this partitioning must eventually be clarified in a full discussion and defense of the physicalist program; above, we introduced one such principle in our development of the bases. Below, we shall explore a number of further such principles to, at least, open the path for further work on this question. At a minimum, an appropriate principle must have some bearing on (i) the subject matter of science and (ii) issues concerning the na-

ture of explanation, in order for them to be pertinent to the physicalist program. A central problem here is the more general one of how to individuate research programs and patterns of explanation in science.

So, (P1) states that science is partitioned according to metaphysically and epistemologically significant principles. What kind of claim is this? First, it is not a purely descriptive claim about the current divisions made among branches of science; thus, it is compatible with (P1) that current science is not carved up along the boundaries indicated by what are the optimal principles. However, although current divisions don't necessarily reflect the physicalistically relevant divisions, they can plausibly be viewed as good first approximations. It is an important research question to discern what are the best ways of cutting up science; it is a question that the physicalist must take seriously as part of his program. The issue is, ultimately, whether science does carve up in physicalistically interesting ways or not; and, the prospects of the program hang crucially on how this issue is resolved. As we shall see, the arguments of the skeptics that no relevant principles exist are inconclusive and there are numerous plausible candidate principles that deserve exploration: whether science carves up in the way that physicalism requires is a currently open issue. The inquiry should begin with a study of existing divisions and head toward a clarification of the ways that science divides or ought to divide up. It is, also, quite consonant with our position

that there are many correct ways to partition science; what we require is that one of those ways accords well with the goals and needs of the physicalist program. There may, of course, be very different purposes that lead to different divisions. A physicalistic conception of science motivated by the goals suggested in Chapter 1 depends for its development upon the identification of a principle that partitions science in ways pertinent to the physicalistic program; alternative conceptions of science might call for different kinds of partitionings and, hence, different principles.

Why is (P1) a presupposition? Recall what our strategy for identification of the physical bases was: first, we identified physics, where by 'physics' we meant a branch of science that was included within natural science, that satisfied certain conditions and that was narrower than all of natural science; second, we extracted from physics so construed a core ideology; third, we suggested how the full physical ideology might be constructed out of the core ideology; fourth, we suggested how the full physical doctrine might be specified; and fifth, we suggested how the core ontology might be specified in terms of the core ideology and how the full ontology might be constructed out of the core ontology. This strategy led to a characterization of the physical bases in terms of which significant physicalist theses are to be formulated. Further, the strategy was based upon a characterization of physics which was metaphysically and epistemologically relevant to the program. It is rather explicitly assumed in all

this that physics is an, in principle, distinct branch of science; a branch that is distinct for physicalistically relevant reasons.⁵⁷

Besides explicit assumption, however, there are other reasons for holding that (P1) is a presupposition of physicalist theses. In particular, let us reflect upon the consequences of rejecting (P1). There are two related ways in which things go radically wrong for physicalism if (P1) is denied, ways which correspond to two different ways of denying (P1). First, it might be claimed that there simply are no divisions of science into distinct branches and that every part of science is just a part of one undifferentiated total science (i.e., "physics"). Second, it might be claimed that there are divisions into branches of science, but the principles that form the basis for the divisions are metaphysically and epistemologically irrelevant to the physicalist program.⁵⁸

If one rejects (P1) for the first reason, then the untoward consequence for physicalism is that it looks to be unavoidable that the theses of physicalism are trivially true because every term of science falls within the basis for ideology, every truth of science falls

⁵⁷ By 'physicalistically relevant' we mean relevant to the ontological and epistemological motivations behind the physicalist program. We emphasize the notion of physicalistic relevance because, as will be seen, objections have emphasized the arbitrariness vis a vis physicalism of boundaries between branches of science.

⁵⁸ For example, the divisions among branches of science are the exclusive products of socio-historical factors that have no further significance.

within the basis for doctrine, and every entity posited or constructible within science falls within the basis for ontology. The physicalist program, therefore, would be quite pointless and of absolutely no interest.⁵⁹ Strictly speaking, we have just shown that (P1) is not a logical presupposition of the theses: to show that, it must be shown that the denial of (P1) entails the denial of the theses. But, we take it that the force of calling (P1) a presupposition is still pretty strong if its denial leads to trivialization of the theses. Hence, we shall continue to refer to (P1) as a presupposition.

If one rejects (P1) for the second reason (i.e., that the principle of division is irrelevant to physicalist concerns), then a few different bad possibilities present themselves: (i) the boundaries among disciplines, being sensitive to irrelevant factors, do not reflect the physicalist bias: as a consequence, the theses simply will not be expressing the ontological and epistemological claims they purport to express; (ii) the boundaries, being irrelevant to physicalist concerns, may be easily reshaped for either irrelevant purposes or for ad hoc defense of the program. The first point is that the content of the theses would appear to depend, not upon facts about nature and science, but upon irrelevant factors (e.g., socio-historical factors) and, hence, it would not be what the physicalist intends. The second point is that the boundaries between physics and other branches of

⁵⁹ This is the first prong of 'Chomsky's fork': i.e., either physicalism is true but trivial or significant but false. See below in our discussion of (P2) for the second prong.

science may be too fluid for the theses to have significant empirical content. Thus, we conclude that these considerations, although not sufficient to establish that (P1) is a logical presupposition, are sufficient to show that if the physicalist wants to be expressing a significant doctrine concerning ontology, objectivity and explanation, a doctrine whose fate is not sensitive to irrelevant factors, then he had better hold on to (P1).

Remarkably, not all physicalists see it this way. Quine, in particular, appears to hold two views which, if we are right about (P1) being a presupposition of the theses, are incompatible: viz., (1) that physicalism is a significant and true doctrine and (2) that (P1) is false (i.e., that natural science is the same as physics). We shall now explore Quine's views here to show that he can't have it both ways.⁶⁰

To do this, we shall make an excursion into the Quine-Chomsky debate regarding the so-called "indeterminacy of translation".⁶¹ In his

⁶⁰ The argument just cited, that the denial of (P1) in favor of no divisions in natural science entails the trivialization of physicalist theses, is sufficient to show this. It is of interest, however, to see how this issue arises in the context of Quine's defense of another of his favored doctrines, "the indeterminacy of translation".

⁶¹ There have been many non-equivalent formulations of the indeterminacy thesis; and, the discussion has been quite muddled in both Quine and his critics. Only recently has it been appreciated that Quine has had several things in mind (cf., Friedman (1975) for a lucid attempt to sort out Quine's views), and only recently has Quine sorted things out in print (cf., Quine (1979) and his review of Goodman in Quine (1978)). It is now quite clear that it is his

early reply to Chomsky,⁶² Quine attempts to argue that his thesis of indeterminacy is, contra Chomsky, distinct from the thesis of the underdetermination of theory by evidence; he claims that "it is parallel but additional". The argument is, roughly, that relative to "the totality of truths about nature" translation is not determined (i.e., there are alternative incompatible possibilities of translation each compatible with the totality of such truths).⁶³ He concludes from this that translation is indeterminate and, hence, that there is no fact of the matter concerning translation as distinct from legitimate scientific theories which, although underdetermined by evidence, are determined by the totality of truths about nature. As stated, Quine's argument is quite open to Chomsky's sharp reply⁶⁴ that nothing has been said which shows that "translation theory" is not included among the totality of truths about nature; and, if it were so included, then it would be trivially determined by the totality of such truths. Thus, the only remaining indeterminacy is that which holds for all theory relative to all possible evidence: an obviously true claim on Chom-

physicalism that is his primary reason for believing in the indeterminacy thesis; it is his physicalism that gives that thesis significant content that is clearly different from the underdetermination thesis.

⁶² See Quine (1969b).

⁶³ This contrasts with the underdetermination thesis since the bases for determination in the two cases are different. Thus, the basis for the underdetermination thesis is the totality of possible evidence, a subset of the totality of truths.

⁶⁴ See Chomsky (1975), p. 183.

sky's view. This is quite right! Quine misstated the physicalist's case.

The problem with Quine's argument lies in his appeal to the totality of truths about nature as the relevant basis of determination involved in his thesis of the indeterminacy of translation. With such a basis, being determined by that basis is trivially true of all of its members and true of nothing else; the only interesting issues concern what to include in the basis. Further determination by it has no significance, as Chomsky rightly claims. To formulate a significant doctrine of physicalist determination in terms of which interesting charges of indeterminacy can be made (charges which differ from the underdetermination of theory by evidence), the basis for determination must be narrower than the totality of all truths about nature. Thus, if Quine's argument is reformulated in terms of "the totality of physical truths about nature", where this totality is a class narrower than all of the truths about nature, then it is not vulnerable to Chomsky's reply. However, in order to do this, something very much like (P1), which allows for the separation of physical from other truths, must be accepted. Thus, Quine must either give up making any significant physicalist determination claims⁶⁵ or he must give up his denial of (P1).

⁶⁵ And, thus, he must give up his contention that there is an indeterminacy thesis distinct from the thesis of the underdetermination of theory by evidence.

In summary, Quine's attempt to argue for indeterminacy from a position that is both physicalist and a denial of (P1) fails. What is required in an argument for indeterminacy is the assumption that the physical truths can be distinguished from the rest of the truths of nature, including the truths of translation.⁶⁶ We conclude that, as physicalists, if we wish to formulate non-trivial theses that are distinct from standard epistemological theses (e.g., the underdetermination of theory by evidence), then we must suppose that the physical basis for truth can be isolated from other truths of science; and this, given our strategy, presupposes that there are principled divisions among branches of science.

As was mentioned in Chapter 1, there are a number of views regarding the structure of science: (i) physics is the only branch of science, all else is a branch of physics or is eliminable in favor of a branch of physics, (i') "physics" is the only branch of science, but "physics" includes chemistry, biology, psychology, linguistics, etc (i.e., 'physics' means all of natural science), (ii) there are divisions among physics and other branches but each is autonomous relative

⁶⁶ Thus, the indeterminacy claim is that translation is indeterminate relative to the totality of physical truths, where that totality is construed more narrowly than the totality of truths about nature. And, this way of putting things does distinguish that thesis from the thesis of the underdetermination of theory by evidence. As we shall see, it is open to Chomsky to reply that nothing has been said to show either that such a class of truths can be identified in a principled way (i.e., that (P1) is true) or that translation is in fact indeterminate relative to such a class if specified. But these are different matters from those considered now.

to the others (i.e., no structural relations need exist between branches), (iii) there are divisions among the branches and physics is basic with regard to ontology and epistemic matters and the others relate to each other in sundry ways, (iv) there are divisions and physics is the basic science in a strict hierarchy ordered by some set of ontological and epistemological principles. For the purpose of establishing (P1) as a presupposition, we need not take a stand on which of (ii)-(iv) we favor.⁶⁷ What is needed is to reject (i) and (i'), as we have just done; and, for the purposes of the argument presented, the differences between them are immaterial. Hence, we conclude that, for physicalism to be a significant doctrine, principled divisions between physics and other branches of science must exist.

We now turn to a discussion of the objections that have been advanced against (P1). None of these objections have been adequately discussed in the literature either by physicalists or their opponents; since the prospects of the program depend crucially on fending off these objections, it is curious that physicalists have chosen to remain silent for the most part regarding them.⁶⁸

⁶⁷ Naturally, (ii) is not in the spirit of the program.

⁶⁸ Such ardent physicalists as Quine, Field, Friedman, Hellman and Thompson, Boyd and Fodor have not discussed these objections and have proceeded as if they did not exist; perhaps they see, what we think is true, that there are ways of defeating the objections.

Critics of physicalism have, in effect, advanced a two part assault on (P1) that is based upon a positive and a negative thesis. The positive thesis is that the apparent divisions among branches of science are the product of socio-historical factors that are irrelevant to the ontological and epistemological concerns of the physicalist program. From this thesis, it has been thought to follow that even if there are divisions among branches of science, they are based upon irrelevant principles and they are vulnerable to irrelevant and arbitrary shifts. Hence, insofar as the theses of physicalism depend upon (P1), they lack the ontological and epistemological content that the physicalist supposes they have. Not just any principled divisions will do; (P1) requires that the operative principles have the right ontological and epistemological significance. To buttress the attack, the negative thesis is that there are no ontologically and epistemologically interesting principles for distinguishing branches of science: all alleged principles of this type are defective. Hence, there couldn't be principled divisions of the kind required and (P1), even if true, could not be pertinent to the isolation of physicalist bases that play a role in significant physicalist theses.

In response to this attack, first, we shall show that the positive thesis, even if true, does not entail the alleged consequences; and second, we shall claim that the negative thesis is essentially unargued for, that there are reasons to believe that the divisions among branches may be relevant to physicalist concerns and that there are plausible candidate principles for characterizing such divisions.

The positive thesis, that the structure of science (i.e., the divisions among and relations between branches) is the product of arbitrary socio-historical forces which concern human interest, the dynamics of group interaction, and institutional structures that happen to exist at a given time, has been advanced by a diversity of figures.

The following are typical expressions of this view:

[...]for many purposes, a simple division of Science into branches is very useful, but we have found no sufficient reason for assigning deep significance to this classification. We conclude that Science is an enormous area of human research which is united by a common method. Its divisions are for convenience in describing results and do not represent a fundamental feature of Science.⁶⁹

The division of science into areas rests exclusively on differences in research procedures and direction of interest; one must not regard it as a matter of principle. On the contrary, all the branches of science are in principle of one and the same nature, they are branches of the unitary science, physics.⁷⁰

The province that is actually regarded today as belonging to each science is very largely the result of historical accident;...Such considerations clearly justify the view that science is a single whole and that the divisions between its branches are largely conventional and devoid of ulterior significance.⁷¹

The issues raised by the positive thesis are: (i) is it true? and (ii) if it is true, does it entail that the divisions among branches of science are arbitrary with respect to the physicalist program? To begin with, it is hard to deny that the above mentioned factors play

⁶⁹ See Kemeney (1959), p. 182.

⁷⁰ See Hempel (1949), p. 382.

⁷¹ See Cambell (1953), p. 13-14.

an influential role in determining what scientists do, how they are identified, and what lines of development are opened or closed in science. So we shall allow that the positive thesis is true.

It is, in fact, a truism that the divisions in science, like every other product of human thought and action, are the result of psychological, social, political, historical and genetic factors: i.e., every such human product is a result of an interaction between genes, individual constitution and the social and physical environments. It should be obvious that this claim entails nothing about how well the content of intellectual products accords with reality. At issue, in part, is what role the "world" plays and what role reason plays in the shaping of our thought. Thus, two different readings of the positive thesis are: (1) that social (etc) forces shape science to some extent and (2) that only such factors shape science (i.e., reality and reason play no role). Our contention is that on neither reading does the positive thesis secure a victory over (P1). We shall accept (1) as a truism, but argue that it has no interesting consequences for the import of divisions in science. Further, we shall suggest that the existence of contributions from reality and reason lead to the rejection of (2) and to an understanding of how divisions in science might be non-arbitrary vis a vis the physicalist program.

It simply does not follow from the positive thesis that the structure of science need be, or in fact is, a "socio-historical accident"

with no ontological or epistemological significance. It is a non-sequitur to conclude, from the claim that socio-historical factors (partially or totally) influence the course and structure of science, that the structure of science is arbitrary from the point of physicalist concerns. Of course, such a claim doesn't exactly ensure that the evolving structure of science is relevant to physicalist concerns either. Additional argumentation is clearly required for both sides of the dispute.⁷²

For our part, we shall suggest two lines of argument designed to make plausible the idea that, despite the impact of socio-historical factors, the existing divisions among branches of science could have some of the ontological and epistemological significance required by the physicalist program. Then, in our discussion of the negative thesis, we shall review some principles that could be operative in making divisions with such significance. In doing this, we shall be taking some steps in the direction of showing how science could be ideally organized in line with the physicalist program and showing that the existing organization approximates this ideal.

The first argument for the claim that, despite the impact of socio-historical factors, the existing divisions in science could have physicalistic significance runs as follows: although it is true that

⁷² For the proponent of the positive thesis simply to add the premise that, if such factors play a role in science, then the resulting structure must be arbitrary would make the argument valid at the expense of begging all the questions at issue.

quite arbitrary (from the point of view of the physicalist program) factors influence the conduct and structure of science, that conduct and structure do not result exclusively from such factors: scientists sometimes seek the truth and sometimes they do so in intellectually responsible ways. There are pressures from within science to proceed (at times) in ways that are orthogonal to social or individual interests and political or religious institutions.⁷³ There are also pressures to constrain thought by principles of rationality and method that do not obviously line up with socio-historical factors; and there are within science research questions concerning what are the ways to individuate and relate programs of research and branches of science.⁷⁴

It appears that the proponents of the positive thesis view science as a "first-order" enterprise with little or no self-reflective capacities; the point of our reply is that this is a false conception of science and that the self-reflective capacities found in the institutions of science are a serious factor in the determination of how science is structured. It appears to us that one of the important goals

⁷³ Consider, for example, the actions of Freud and Galileo: two thinkers who, though influenced by existing institutions, did not succumb to them (at least, in the privacy of their own studies).

⁷⁴ The programs of physicalism and the unity of science are examples of attempts to identify such meaningful divisions in science as well as to formulate relations between the parts so divided. Thus, the current project is an attempt to contribute to the course of science as well as to capture existing trends in current practice; as such, it is a force of a non-arbitrary kind vis a vis the structure of science. Such pressures are not obviously in the service of arbitrary social and historical forces.

of science is to determine where important boundaries in both nature and knowledge about nature reside. Despite the impact of arbitrary forces on institutional divisions, it is difficult to take seriously the idea that none of the divisions in science are significant; and, we are suggesting that this is the result of scientific activity itself. At the minimum, the objector must allow that divisions among branches of science are imperfect reflections of significant principles and not just the reflection of arbitrary factors.⁷⁵

A second reply, in response to the positive thesis and associated argument, runs as follows: if, as science evolves, it becomes increasingly more successful in providing explanations and predictions of natural phenomena, then there is some reason to believe that the divisions within branches of science (e.g., the identified natural kinds) and the divisions among branches of science reflect important metaphysical and epistemological facts.⁷⁶ That is, success within a given do-

⁷⁵ It appears that only the most trenchant relativist of one form or another could hold onto the positive thesis in its stated form; perhaps, such thinkers would hold on because either they over-emphasize the power of arbitrary factors (it seems that to chalk up scientific change and activity to arbitrary factors entirely here is to overplay one's hand considerably), or they hold a vaguely formulated view about relative reality and the relativity of knowledge which allows no constraints by rationality or the "world". Either form of relativism is untenable on our view. We are, however, sympathetic to the idea that the structure of our representation of nature is determined by our own cognitive activity: this is pretty much our point that within the activity of science there are cognitive forces counter to the socio-historical ones.

⁷⁶ The debt to Boyd is obvious here although it is not clear that he has or would employ the success argument with regard to the divisions between branches. See Boyd (forthcoming).

main suggests that what is included within that domain hangs together in certain ways that phenomena from that domain and phenomena from some other would not. The success of the various branches of science suggests that they are focusing upon features of reality that do not represent an arbitrary grouping; the failures within a branch would quite naturally suggest that the right domain and groupings of phenomena within that domain had not been identified. Further, success in a domain suggests that good questions are being asked and effective strategies for answering them have been found; again, failure would suggest the opposite. What we are suggesting is that success within a branch of science is an indicator that the branch has found a coherent subject matter, that it has a certain amount of theoretical integrity, that it has hit upon significant truths about its subject matter, and it has been asking the right questions about its subject matter; failure suggests the opposite. By 'subject matter' is meant the individuals within a domain, their attributes, and the regularities exhibited by them. Thus, success indicates that the right questions are being asked about the right things and the right attributes of things. This suggests three possible principles of division for demarcating the boundaries between branches: (i) what the individuals in different domains are and how they are related in terms of, say, complexity of organization, (ii) what the attributes in different domains are and how they are related (e.g., degree of generality or abstractness), and (iii) what the questions asked and patterns of response are in the different domains. We shall look at such principles below.

What the above argument is supposed to show is that, again, even if socio-historical factors influence the structure of science, it doesn't follow that that structure is merely an arbitrary one or one that is exclusively determined by such factors. Science also follows paths of success. Hence, to the extent that science is successful, an account of that success might be offered in terms of the ontological and epistemological facts concerning its structure; if the structure were purely an arbitrary one such an account would not likely be possible. We are not suggesting that the (full or partial) success of science entails that the structure of science is significant; we are suggesting that a certain kind of structure could account for such success, and that, in the spirit of an inference to the best explanation, the actual success of science leads to the view that its current structure is to some extent non-arbitrary. Notice that we have not said what principle of the possible ones mentioned is relevant here. This is only an argument for the possible non-arbitrariness of the structure; an account of the details of such structure is still owed.⁷⁷

There are two objections to this line of argument that we shall consider. First, it will surely be pointed out by the defender of the positive thesis that this type of argument cannot be applied just anywhere. In this case, it could be claimed that, although the form of

⁷⁷ So far we have suggested two sources of non-arbitrary factors in science: (i) scientists and (ii) the world, insofar as reference to it plays a part in an account of the success of science.

argument does apply to natural kinds in science, it is not appropriate to apply it to larger divisions (e.g., branches of science): there is a significant lack of similarity in the two cases. If the natural kind terms are the ones that occur in the law-statements, then such terms can be taken to pick out a natural kind if the law-statements that they occur in are substantially verified. The predictive and explanatory success of the law-statements suggest their truth, and their truth suggests the existence of natural kinds associated with terms that occur within them. However, the success of a branch of science is a much more hazy idea; why, it might be argued, couldn't the success of a branch only be an artifact based upon the individual successes of laws grouped together, however arbitrarily? For example, put the laws of mechanics and the laws of biology together; such a grouping will, undoubtedly, have a number of explanatory and predictive successes. But is that grounds for inferring to any interesting integrity of the grouping? The answer is "no" of course.⁷⁸ But, we didn't claim that the argument was demonstrative or that any grouping of successful principles constituted a branch of science. However, the objection does raise the question of whether we have correctly applied this form of argument. What are the marks of a case in which it applies? What does the success of a branch of science consist in?

⁷⁸ As we observed above, this kind of conglomeration may be what the current branch of science called "physics" is; our discussion and claims are to be construed in terms of an idealization of physics taken as a particular body of theory characterized by the principle we discussed earlier when physics was identified.

Before attempting to respond to this challenge, let us look at the second reply to the success argument, since it raises similar issues. The defender of the positive thesis will point out that existing institutional divisions among branches of science are obviously arbitrary from the physicalist's point of view: electrical engineering departments, for example, include computer science, brain physiology and the study of electrical phenomena in general; earth science departments include geology, paleontology, meteorology, and geophysics; and, a brief scouting of the humanities, physics and philosophy suggests that disciplines are grouped together quite arbitrarily. How, in the face of these facts, could we contend that the divisions among branches reflect non-arbitrary factors? It might be further pointed out that changes in the groupings are obviously sensitive to quite arbitrary factors; boundaries in academic institutions generally, and in science in particular, are not clearly delineated so much in terms of subject matter or questions asked or methods employed as they are delineated in terms of political and economic factors. Such considerations strongly suggest that, whatever account of success is offered, it had better not lean heavily on the idea that divisions among branches are metaphysically or epistemologically significant.

So, to summarize the point of the objections, the success argument has unclear conditions of applicability and the facts of the existing situation in science suggest that, whatever the conditions are, they don't obtain in the case of the existing structure of science. Is the success argument defensible in the face of these replies?

With regard to applying the success argument, things are at somewhat of an impasse awaiting the more adequate understanding of what "success" consists in, what the structure of the argument is and what the conditions of its use are. In considering the contrived example of mechanics-biology, we were immediately able to recognize the non-applicability of the success argument; but, our recognition depended upon knowing how to distinguish mechanics from biology. The existence of such divisions is precisely what is at issue, and thus, it would appear that we were begging the question by assuming knowledge of how to make them. Our capacity to make such divisions may only reflect our knowledge of current groupings; but, it is the alleged arbitrariness of these groupings that the success argument is introduced to dispell. Thus, the success argument can be of no help in establishing the non-arbitrariness of existing groupings if its application has unclear conditions or if its application depends upon having knowledge of the very principled divisions that it is supposed to establish.

The second objection to the success argument was that, whatever the conditions of correct application, they don't obtain in the case of existing science because the divisions there are patently arbitrary: thus, on this view, existing science is both successful and arbitrarily organized into branches. This, of course, begs the question at issue. However, it does point out that, at best, existing divisions only approximate a principled organization of the sciences; and, it, along with the previous objection, effectively rubs in the futility of ap-

pealing to the success argument as a means of showing that existing divisions even approximate physicalistically significant ones. This is because, in the absence of knowledge of the principled divisions, the sorting out of applicable from non-applicable cases for the success argument cannot proceed.

In reply, we must abandon the success argument and fall back on our earlier position that it is part of the ongoing research program of science, and of physicalism in particular, to isolate branches of science that are based upon ontologically and epistemologically significant (and yet to be clarified) principles.⁷⁹ It is within the framework of specific principles that divisions among sciences and revisions of such divisions are non-arbitrary; our claim is that it is a part of the physicalist program in science to develop such principles. With such principles in hand, it will be clear in what ways existing divisions do or do not approximate the physicalistically ideal ones that are required for development of the physical bases. Such principles could play a methodological role in science in deliberations concerning revision of boundaries in science; and, they could show how the success argument, if it is clarified, should be applied (i.e, how an account of the success of science might be given in terms of its structural divisions). This, of course, is of no use to our current problem which is that of identifying the relevant principles

⁷⁹ For example, principles concerning levels of organization, abstraction or generality.

and establishing the existence of significant divisions. A burden clearly exists for the physicalist to carry, although the plausibility of denying (P1) has equally clearly not been established by the proponents of the positive thesis.

Thus, our view is that it is a part of the physicalist program to delineate the structure of science at a certain level of generality and, hence, to delineate principles that distinguish branches of science in a physicalistically relevant way. The physicalist project, therefore, is to some extent a bootstrapping one. That is, the physicalist program must involve development of the principles that divide branches in relevant ways; the opponent, as we shall now see, suggests that such principles are not there to be found. Whether or not he succeeds in this, we conclude that his positive thesis does not lead to the skeptical conclusions that it was thought to.⁸⁰

The negative thesis advanced by the opponent of (P1) was that there are no metaphysically and epistemologically relevant principles for distinguishing branches of science; hence, there could not be any principled divisions of the right kind for physicalism. If such a claim is true, then, regardless of the merit of the positive thesis just discussed, (P1) would be defeated and with it the entire physi-

⁸⁰ Another way of seeing this is that, even if divisions serve heuristic purposes as Kemeny and Hempel suggest, that does not mean that there is no ontological or epistemic significance of the divisions. Such utility raises questions that, as we suggested above, could be answered in terms of such significance.

calist program.

We shall consider one line of argument in support of the negative thesis and then consider some variants. The quotation that best instantiates the basic idea of the argument is to be found in Chomsky as follows:

It is an interesting question whether the functioning and evolution of human mentality can be accommodated within the framework of physical explanation, as presently conceived, or whether there are new principles now unknown, that must be invoked, perhaps principles that emerge only at higher levels of organization than can now be submitted to physical investigation. We can, however, be fairly sure that there will be a physical explanation for the phenomena in question, if they can be explained at all, for an uninteresting terminological reason, namely that the concept of 'physical explanation' will no doubt be extended to incorporate whatever is discovered in this domain, exactly as it was extended to accommodate gravitational and electromagnetic force, massless particles, and numerous other entities and processes that would have offended the common sense of earlier generations. But it seems clear that this issue need not delay the study of the topics that are now open to investigation, and it seems futile to speculate about matters so remote from present understanding.⁸¹

Now, in order to see the relevance of this passage to our problem, it must be assumed that its point is to show that there are no principles for constraining the evolution of the divisions between physics and other branches of science and, hence, of our concept of the physical. That is, we shall take the point of this passage to be that of purporting to show that what counts as physics and what counts as "the physical" is open to unconstrained revision.⁸² The argument for this

⁸¹ See Chomsky (1968), p. 83-4.

⁸² Chomsky seems to be suggesting that what counts as physical is only

consists in suggesting examples of how physics has been revised in the past; for example, the suggestion is that there was no operative principle for incorporating electromagnetic theory (EM) into physics other than a principle which states that the failure of the attempts to reduce it to mechanics is sufficient for inclusion. The conclusion seems to be that, since physics is an evolving branch of science, there is no firm conception of what physics is; and hence, anything could be included within physics by arbitrary fiat. If this were true, then (P1) would, at best, concern current divisions which are completely maleable (as evidenced by the EM case): what was once thought to be a separate and reducible branch of science was "shifted down" into physics as a result of repeated failure of reduction attempts and the arbitrary decisions of scientists. The morals are supposed to be: (1) the divisions among branches are not and could not be constrained⁸³ by significant principles and (2) the theses of physicalism are trivial

a terminological matter of no substantive interest. If, as Chomsky sometimes suggests, 'physics' and 'the physical' are really terms designating all of natural science, then the point he makes in the cited passage may well be correct; but, as we have emphasized, that construal of 'physics' and 'the physical' is not the one intended by the physicalist. We shall understand all issues raised by the negative thesis as bearing on the attempt to individuate physics and other branches as proper parts of natural science. See Chomsky (1980) for explicit discussion of the broader construal of 'physics'.

⁸³ The claim here is stronger than the claim that existing divisions are vulnerable to socio-historical factors (i.e., the positive thesis). Here there is no mention of such factors, but rather, the claim is that we are free to shift boundaries in science as we will (i.e., there are no principles for constraining such shifts.)

as a result.⁸⁴

We have two replies to this: first, a look at the example cited reveals that the attempts at reduction were quite strenuous.⁸⁵ The decision to shift the boundaries so as to include EM theory within basic physics was not whimsical or arbitrary, but rather the outcome of numerous efforts and considerable methodological debate. The claim that this shift is indicative of the arbitrary revisability of the boundaries within science is pretty much of an overstatement: a patently arbitrary shift, which this is not, would be needed to make the intended case. An adequate understanding of the methodological situation raised by the kind of conflict that the EM case exemplifies would be valuable. To my knowledge no one has worked this out in very much detail to gain such understanding; and, in its absence, it is difficult to understand the appeal of the Chomskian interpretation.⁸⁶ We do not see that the example goes very far in showing either that the specific case of EM theory counts as an arbitrary shift or that the divisions in science are completely and arbitrarily maleable as Chomsky suggests (i.e., that there are no viable principles for individuating the branches of science).

⁸⁴ For example, the thesis that there is a physical explanation for each phenomenon is trivially guaranteed by the arbitrariness of the boundaries (assuming that there is an explanation at all).

⁸⁵ For a general overview of the various attempts, see Whittaker (1951) chapters 8 and 9.

⁸⁶ See below, Chapter 5, for discussion of such methodological situations.

Second, we urge the distinction employed above between the concept of physics and specific physical theories that fall under that concept. The opponent of (P1) must, in effect, be denying the existence of such a distinction, since once it is made it is possible to see how physics could be an evolving science while the concept of the physical remains fixed and useful in drawing principled boundaries between branches of science and in constraining the development of physical theories. Presumably, if such a distinction can be made out and the concept of physics delineated, then we would see why the EM case is not an instance of arbitrary revision of the boundaries of science, but rather, an instance of revision of physical theory.

It might be objected here that the Chomskian objection does not lose its force with the introduction of this distinction since, if physical theory is completely maleable and subject to any change, then a revision of that theory to incorporate any other theory could occur; hence, the principled boundaries between branches would be meaningless and the physicalist theses would still lack content. But, as we observed above, the EM case is not a clear instance of arbitrary revision of physical theory (as opposed to a revision that corrects a false version of physical theory); and second, the conditions of that revision of theory are not completely understood in terms of what factors were operative in and relevant to the decision. Hence, no demonstration of the general arbitrariness of revision of physical theory has been given. Our contention is that, in the presence of a clear

conception of the nature of physics, the boundaries between sciences will be clear and revisions of "physical" theory will be constrained in significant ways. As a result, the content of physicalist theses will be guaranteed and a clearer conception of the empirical content of the theses will be attained. Thus, we conclude that the Chomskian appeal to the EM case is quite inconclusive and does not motivate belief in the non-existence of any principles for distinguishing physics from other branches of science; rather, it begs that question.

The strategic situation is currently this: in our development of the bases we appealed to the distinction between the concept of physics and physical theories; such a distinction allows that physics is an evolving branch of knowledge without denying that physics is distinct from other branches of science in a principled way. And, our above characterization of physics is a specific case of the more general appeal to principles for individuating branches of science which would underwrite (P1). Given such a principle for identifying physics, the evolution of physics can be viewed neither as arbitrary from the point of view of the physicalist program nor as simply ensuring the truth of physicalism in a trivial way. Our burden is to produce and defend such principles; our opponent's burden is to produce arguments either to the effect that no such principles are possible (i.e., the negative thesis) or, at least, to the effect that all specific proposed principles are defective in some way (e.g. by being question begging). If our opponent succeeds, then (P1) is defeated and no

principled identification of the physical bases exists; hence, the theses are vacuous or trivial. If we succeed, then (P1) is vindicated, principled identification of the physical bases is possible and thus the theses will have significant content. So far, we have concluded that the Chomskian appeal to the EM case does not substantiate the opponent's claim that no such principles are possible.

A second argument for the negative thesis runs as follows: any attempt to formulate a principle for identifying physics (or, the physical) is defective because it inevitably contains a term (e.g., 'matter', 'inorganic') which raises exactly the same problems that the principle is supposed to solve with regard to the term 'physical'; hence, any such principle is question-begging and of no value. This objection clearly locates the burden of the physicalist: to develop a non-question-begging characterization of physics. Our strategy for carrying this burden will be to look at some principles (including the one employed above) to assess their chances of surviving the current objection and of eventually being developed into an acceptable form for distinguishing branches of science. The only way we can see of defending against the negative thesis and any argument designed to support it is to actually develop a viable principle; this, in our view, is a key task in the ongoing development of the physicalist program within science. In allowing this much, we have conceded nothing to the position of the opponents who have typically begged all the questions.

On the basis of our discussion so far, we conclude that the features that any adequate principle for distinguishing branches of science must have are: (i) it divides up science in physicalistically relevant ways, (ii) it constrains the growth of science in principled ways, (iii) it leads to non-trivial physicalist theses, (iv) it does not have any question-begging terms, and (v) it has reasonable prospects for being developed. There are, at least, three kinds of principle that we shall consider: ontological, explanatory and determinatist. That is, science may be seen as dividing into separate parts on the basis of (i) differences and relations between the subject matters of different branches, (ii) differences and relations between explanations offered in the different branches, and (iii) semantical relations holding between theories in the different branches.

Among the ontological principles, the following three hold out promise: first, Oppenheim and Putnam⁸⁷ suggested in their landmark paper a hierarchy of individuals based upon mereological relations which rigidly demarcated the subject matters of the various branches of science. The idea was that each branch contains individuals which were "parts" out of which the individuals at the next higher level were composed; no individual, on this view, is a member of some domain if it contains as parts individuals that are members of the same or a "higher" domain. Putnam and Oppenheim took this picture to be a reflection of the "Democritean Tendency" in science; and, clearly on

⁸⁷ See Oppenheim and Putnam (1958).

this view, physics occupies a special place relative to all other sciences: viz., it contains in its domain individuals that do not have parts drawn from any other domain but that are parts of the individuals in all other domains. Despite the various criticisms that this view has been subject to,⁸⁸ it is an instance of a principle that could have succeeded (i.e., although its prospects are in fact quite poor, it does not have any principled defects such that it is a vacuous principle or such that it must fail). Our purpose in this section will not be to develop specific proposals as much as it will be to establish that there are principles that could serve the required purpose and that there are some principles with a future. The proposal just considered is an instance of the first but not the second motive.

A related principle has been suggested by Boyd and Shoemaker⁸⁹ with regard to attributes: roughly, the view is that attributes are to be individuated in terms of their causal powers and that the physical attributes are the ones out of whose causal powers all other attributes are realized. As an approach to the individuation of attributes, this view fails. However, if the notion of a causal power can be made precise, and if the composition of causal powers can be developed, then the principle may have some promise as a principle for ordering the attributes which comprise the subject matters of the various branches

⁸⁸ See Causey (1977) for discussion of the flaws with this proposal and for an alternative development of the core ideas.

⁸⁹ See Boyd (unpublished) and Shoemaker (1979).

of science.

With regard to both of the principles just mentioned, it might be objected that the notions of "part" and "causal power" are open-ended in a trivializing way: that is, it will always be possible to include within the "physical parts" or the "physical causal powers" an individual or power which does not comfortably fit into any existing framework; there is nothing in either of the principles to prevent ad hoc modification of the physical basis. This is the claim that we previously defended against by claiming that this "problem" is nothing peculiar to the issues at hand, but is rather an instance of the general problem of how to distinguish ad hoc changes of a theory from more legitimate changes. We will assume that, if this is the only problem that a proposed principle faces, there is nothing defective about the principle itself, since the problem is a quite general one. What the critic must show is that there is some special way in which formulations of physicalist principles for demarcating branches of science are question-begging; this is what we claim he cannot do in the general case.⁹⁰

⁹⁰ As we shall see below, there is a general problem that does appear to be insurmountable but which is not that ad hoc modification of the bases is possible; rather, it is that within the parameters of allowable revisions, intuitively non-physical entities may be included in the physical bases.

⁹¹ See Fodor (1975).

A third principle that has been proposed by Fodor⁹¹ is the view that branches of science can be individuated in terms of the degree of generality of their subject matter. This view locates physics as the most general branch of science and other branches as less inclusive: all natural phenomena fall within the domain of physics, while the domains of all other special sciences constitute a proper subset of the domain of physics. Unfortunately, such a principle is unsatisfying for two reasons: first, even if it is correct, it does not identify physically relevant features of the domains of different branches (e.g., features of the phenomena or features of the patterns of explanation beyond generality). And second, it is not sufficient for identifying physics uniquely as the basic science: if it is just generality that is at issue then it is not obvious that psychology is not completely general in science in addition to physics.⁹² Our view is that by appeal to the details of the subject matter and patterns of explanation⁹³ in distinguishing branches of science, a more detailed understanding of the basis of the 'generality of physics' will be gained; and, as a consequence, physics will be clearly distinguished from psychology in the structure of science. Further, the physicalistic relevance of the divisions will be clearly exhibited. We do not view Fodor's suggestion wrong so much as not as useful in the development of the physicalist program as more detailed alternatives.

⁹² See Goodman (1979) for this suggestion.

⁹³ Perhaps construed in terms of the questions asked and the constraints placed upon answers to those questions.

The final ontological principle we shall consider suggests that the sciences are to be individuated in terms of some measure of the degree of abstractness of the phenomena studied; although Chomsky sometimes talks in these terms, it is not clear how the idea is to be developed. One construal might be in terms of abstractness relative to the physical basis, a construal that would comport well with the physicalist program. The burden of such a view is to make clear what determines degrees of abstractness. The possibility discussed earlier (i.e., levels of complexity of individuals or attributes) could serve such a purpose; but, perhaps, a general notion of "realization" of one attribute by another could better serve to characterize the dimension of abstractness without being committed to exactly one way in which attributes are realized (e.g., part-whole composition or the composition of causal powers). On such a view, the domain of physics would provide a general basis for the realization of all other attributes studied in science; other branches might also be ordered in terms of the realization of the attributes studied at successively "higher" levels (e.g., psychology might be considered more abstract than the neurosciences on such grounds).⁹⁴

⁹⁴ It should become increasingly clear as we proceed that there may be considerable distance between the actual divisions in science and an idealized set of divisions rigorously based upon some clearly articulated principle; to the extent that there is such distance, the discussion here and the physicalist program generally should be construed normatively (relative to a commitment to the basic motivations of the physicalist program). Our point earlier concerning the positive thesis was that there is considerable "descriptive" accuracy in certain proposed principles for individuating the sciences, despite the lack of independence from socio-historical fac-

To summarize: one plan for individuating the branches of science is in terms of their subject matters (either the individuals or the attributes studied). There are no obvious grounds for ruling out this enterprise as defective in some principled way; it is not even implausible that, as science evolves, it will resolve itself into branches that do differ in terms of their subject matters in some principled way (e.g., generality, abstractness, degree of complexity). That this is possible is sufficient to undercut the skeptics negative claim (no such principle is possible). Whether science actually resolves itself in some such way is open to further research and is an integral part of the physicalist program. That such a resolution is subject to arbitrary, physicalist whim seems implausible as long as science is conducted in a way that prohibits ad hoc modification of its structure.

A second class of principles for individuating branches of science concerns explanation; as we saw above, physical2 was an attempt along these lines which we rejected. Alternatively, it has been suggested, especially in the classical reduction literature,⁹⁵ that physics was to be distinguished from other branches of science because it was com-

tors. We think this is especially true of the principle just discussed: an idealized ordering of branches of science based upon the degrees of abstractness of the attributes studied is clearly, but imperfectly, approximated in existing divisions in science. Such an ordering serves well the purpose of developing the physicalist bases and theses; and, we shall propose below (Chapter 4) that such an ordering be refined as the program of physicalism is worked out in science.

⁹⁵ See Feigl (1963), Hempel (1949), Nagel (1961), and Carnap (1969).

pletely general vis a vis explanation (i.e., physical explanation subsumed all explanation.) As we have seen,⁹⁶ this vision was premised upon a defective notion of what explanation is and, therefore, is not acceptable as it was formulated. Quine and Davidson,⁹⁷ apparently having an equally unacceptable view of what explanation is, have suggested that physics is completely general vis a vis explanation in the sense that physical explanation is the only real explanation there is: all other apparent forms of explanation are developed as placeholders in the areas of our ignorance of what is "really" going on or they are developed for purely heuristic reasons. The idea is that they will all be eventually replaced in our ultimate physical theory.⁹⁸

Despite the deficiencies of these approaches, it is still plausible that branches of science might be individuated in terms of the patterns or kinds of explanation offered; what is required is a good theory of explanation. The so-called "question approach" to explanation offers a potentially profitable yet not fully explored avenue of

⁹⁶ See Chapter 2.

⁹⁷ See Quine (unpublished) and Davidson (1970).

⁹⁸ Again, we do not find this approach acceptable because of its inadequate assumptions about the nature of explanation. Very roughly, our dissatisfaction with this approach is based upon the following: (i) it assumes that derivation is sufficient for explanation and offer no understanding of how explanation works, (ii) it involves poorly defined or non-existent conceptions of how to individuate kinds of explanation, and (iii) this view, being eliminative, apparently proposes to discard perfectly good patterns of explanation.

research.⁹⁹ With such a theory in hand, physics and other branches might then be distinguished in terms of the questions asked and the constraints put on answers to those questions. The state of development of this approach is quite immature, but there are no known reasons to believe that it is defective in the ways that the D-N model of explanation is and it is worthy of serious attention. Despite the promisory note here, the idea that no such principle is possible is clearly premature, if not quite mistaken.

The principle that we suggested above for identifying physics is an instance of a combination of the various ontological principles and the question approach to explanation; thus, it is an attempt to distinguish physics from the rest of science in terms of both its subject matter and its patterns of explanation. As noted, there is no reason to view this principle as involving any question-begging vocabulary or as not having prospects for future development and use. It is, thus, a potentially sound and plausible principle for individuating physics from other branches of science; and, it suggests a general strategy for identifying all branches (i.e., in terms of both ontology and explanation). Our view is that the strong claim of the skeptic appears to be false, despite the fact that our principle is under-developed.

⁹⁹ See Bromberger (1966).

We note, finally, that Quine and Hellman and Thompson have proposed versions of a determination principle as the distinguishing mark of physics: viz, it is the job of physics to identify the minimum catalogue of states such that nothing happens in nature without some change in those states.¹⁰⁰ Although we have considerable sympathy for this view, it does not appear that it, like the principle of the generality of physics, can serve adequately for uniquely distinguishing physics from other branches of science (e.g., psychology), since there is no guarantee that other branches might not fit this characterization while not counting as physics.¹⁰¹ Although such a principle is a necessary component of any full characterization of the role of physics in science, it does not appear to be sufficient: it leaves out the ontological and explanatory primacy of physics which are its distinguishing marks.

It might be objected that the claim that ontology and explanation are sufficient for individuating physics and other branches is also subject to the same problem of not being sufficient. What rules out a phenomenalist ontological basis or a phenomenalist explanatory basis?

¹⁰⁰ This formulation is Quine's as expressed in Quine (1978). See also Hellman and Thompson (1975) for discussion of an alternative formulation.

¹⁰¹ See Goodman (1979) for concise statement of the objection here. In short, the principle above might be equally well satisfied by a mentalistic basis where the underlying claim is that every difference we find in nature depends upon differences in our conceptualization of nature. We shall not, in the current project, explore how serious such a proposal is and whether it is truly a competitor to the physicalist principle proposed by Quine.

We defer discussion of this question until we consider the third presupposition of the theses. We note, however, that it is not our view that phenomenalist programs concerned with ontological or explanatory reductions are impossible or of no conceivable value; what determines the "direction of reduction" will ultimately depend upon one's interests and other commitments. On the other hand, we do not hold that physicalist and phenomenalist programs in science serve the same goals or express the same metaphysical views; these differences should be manifested in the principles employed in dividing up science within the framework of a given program. It is not clear that the ontological and explanatory goals served by the two programs could be the same. Thus, the objection that there is no principle that is adequate for distinguishing physics from psychology is not clearly correct; if the ontological and epistemic differences between the two kinds of program are expressed by a principle of division, it should suffice to isolate the basic science from all others. What we have been arguing is that the principles of generality of subject matter and of determination are not detailed enough to accomplish this end.

This completes our brief discussion of the candidate principles for discerning the boundaries between physics and the other branches of science; the view we have taken is that, although physics is an evolving branch of knowledge, it is not a branch with arbitrary boundaries. The development of an explicit formulation of principles which demarcate the boundaries between branches of science is an ongoing enter-

prise within science and philosophy; and, there are no currently known reasons for believing that such principles won't be developed or that science won't resolve itself into a set of branches which conform to epistemically and metaphysically relevant principles. To this point, we conclude that neither the positive thesis nor the negative thesis is sufficient to underwrite a rejection of (P1); thus, the physicalist need not feel threatened by the contention that there are no physicalistically relevant principled divisions in science.

What we have argued so far is that there are no good reasons to doubt that the ontology and theories of science could be (and probably are to some extent) partitioned and ordered by principles which distinguish more and less ontologically and epistemically basic parts. Thus, there is no reason to doubt that sense can be made of a most basic science which can play the role required by the physicalist program. Further, we have contended that what gets included within the basic science is not a simple matter of arbitrary revision of boundaries: to make it into the basis for either ontology, ideology or doctrine, something must be compatible with the (yet to be fully developed) principles that order the different parts of science and which distinguish physics from the rest. This we have contended is not a simple matter of terminology or of convention or of heuristic convenience, but rather, it is a matter of theory construction on a par with all other theory construction in science.

However, an alternative reading of Chomsky's discussion of these matters cited above suggests a difficulty with (P1) that our response to the positive and negative theses is not effective against. The difficulty may be formulated as follows:

The sense in which our conception of the physical is evolving is based upon the idea that, at higher levels of organization and complexity, new phenomena and principles concerning them "emerge"; and, if the antecedent physical principles concerned with less complex phenomena prove not to be sufficient for accounting for the emergent phenomena, then, in order to preserve the basicness of physics such phenomena and principles must be conceived of as physical. But, if this happens, although the revision need not be seen as arbitrary or a matter of terminology or convention, it leaves open the possibility that phenomena and principles originally conceived of as "mental" (i.e., paradigmatically non-physical phenomena and principles) must be reconceived as part of the physical bases. This, by all accounts, undermines a central motivation of the physicalist program.

Thus, according to this objection, even if it is allowed that revisions of the basic science are not arbitrary, it is not ruled out that non-arbitrary revisions of the basis might lead to inclusion of obviously mental phenomena and principles, thus defeating a central ontological motivation of the program.

This objection is a serious one to which the physicalist program, as we have conceived of it so far, is vulnerable. We shall defer further discussion until we have completed our presentation of the entire physicalist doctrine. In Chapter 5, we shall consider possible avenues of reply that have promise for saving the program from the kind of self destruction envisioned by the Chomskian objection.

3.3 PRESUPPOSITION 2

We turn now to a discussion of the second presupposition: i.e., that there are determinate bases to be developed. By 'determinate' we mean that there is a fact of the matter regarding each thing as to whether or not it is included in the physical bases. So, (P2) means that for each of the three bases that were developed, there is a fact of the matter for each thing as to whether or not it is included in that basis (i.e., ontology, ideology, or doctrine).

(P2) is a presupposition because the bases must be determinate if the theses of physicalism are to have content; that is, if there is no fact of the matter as to whether or not objects are included in the bases then there is no fact of the matter as to what the theses are expressing and there is no fact of the matter as to whether or not they are true or false. At this point, we are assuming that we have a general characterization of what physics is without a specific version of physical theory yet in hand; this strategic move allowed us above to avoid the objection that physicalist theses were significant but obviously false.¹⁰² The issue now is: given a characterization of what physics is, is there reason to believe that determinate bases for ontology, ideology and doctrine are possible? If not, then the significance of the theses is seriously challenged (i.e., the theses appear

¹⁰² Assuming the inadequacy of current physics.

to lack determinate content and truth value).

So, given a characterization of physics which puts principled limits on how physics might evolve,¹⁰³ we must now explore the prospects for finding determinate bases within these constraints. First, it is a general problem we face to establish the determinateness of the bases without sacrificing the plausibility of the theses (i.e., spare them from the objection that they are significant but obviously false): we can make no rash moves in attempting to defend (P2). Second, we shall see that there exist a number of objections that must be contended with:

1. That the existence of possible worlds other than our own, each with a radically different physics from the physics of the actual world, creates insurmountable problems for the isolation of the "physical" bases.
2. That given our general characterization of physics there is no guarantee of a unique physical theory that satisfies it.
3. That given a formulation of a physical theory there is no determinate and unique identification of the empirical (as opposed to the purely formal or heuristic) vocabulary of the language it is formulated in.

¹⁰³ Such a characterization stems the initial tide of objections that physicalism is insignificant because the boundaries of physics are completely maleable.

4. That given a formulation of a physical theory and a specification of the empirical vocabulary of the language of physics, there is no unique or determinate ontology that satisfies the theory.
5. That certain forms of physical theory (e.g., quantum mechanics) undermine the claim of determinateness of the bases.

In what follows we shall survey these objections to see if (P2) can be saved.

Assuming our characterization of what physics is, it might be objected to (P2) that physics in our world could be radically different from the "physics" in some other possible world; hence, to develop the physical bases in terms of our physics is either to be guilty of arbitrary parochialism or to have failed to have given an adequate account of the physical bases. The issue raised here is whether or not to include in the bases the attributes, individuals, terms, etc adverted to or employed in the other possible physics. The defender of (P2) can respond adequately to this line of objection by claiming that physicalism is, to some extent, a doctrine that gets developed in a parochial manner: it is concerned with one world at a time (in the present case, our own). As a general doctrine, physicalism might claim that, in each world, the metaphysics and scientific knowledge in that world are structured according to the physicalist theses; but, what counts as physics in the different worlds might vary. In short, we can take physicalism as a view about the structure of ontology and knowledge

without saying what the entities, terms, etc in the bases are. Relative to a given physics (in this world or some other), the development of the content of the bases takes place, no matter that there are other possible physics. We have paused over this objection because, although it is innocuous, it is similar to more serious objections to follow.¹⁰⁴

The second objection to (P2) is based upon the idea that there may be more than one actual physics¹⁰⁵ that satisfies the characterization of what physics is and which meets all empirical and methodological tests.¹⁰⁶ There are two possibilities to consider here: first, although there are different formulations of physical theory, they are formulations of the same theory (i.e., there is only one physics, but

¹⁰⁴ The modalities here are important to keep track of. In talking about other worlds we should distinguish them from possible but not actual states of our world which are sometimes represented in a possible worlds semantics. Physicalism is quite concerned with possible states of nature; hence, the bases developed in a given world should include all the possible states of that world. This is not the same as requiring that all possible "physical" states of all possible worlds be included in the bases. The point here is that, unlike Quine and his followers, we see nothing wrong with talk about possible states of this world (states that might never in fact be actual); and, this talk must be distinguished from talk about other possible worlds, which may be quite correctly objected to. If it is, then much of the confusion just mentioned would be eliminated as well as would the objection based upon "the physics" of other possible worlds. On the other hand, if talk of possible worlds other than our own is acceptable, then, probably, a model theoretic representation of all possible worlds and of all the possible states of each such world could succeed in making all the requisite distinctions via a judicious use of an accessibility relation in the model structure. In this case, the reply in the text to the objection based upon multiple possible worlds should suffice and the situation should be clearly representable in the

there are alternative formulations of it). Second, the different formulations are really formulations of different theories that are empirically equivalent; within this second possibility, there are two further possibilities: either the two theories are compatible with each other or they are not.

If the situation is one in which there are two formulations of one theory, then there is no problem with relativising one's development of physicalist bases and theses to a given formulation: there may be different characterizations of the bases, but the differences will only be notational (i.e., there will be one set of bases described in different notations). The point for our purposes is that "merely" notational differences do not make any difference to the physicalist program. There is no way to undermine (P2) in this case since the existence of alternative characterizations of the bases does not undermine the determinateness of the bases characterized. If alleged notational differences did make a significant difference to the program, that would be grounds for believing that the differences were not just

structure.

¹⁰⁵ I.e., physics of the actual world.

¹⁰⁶ An alternative framing of this objection is given in Putnam (1979). There he considers the existence of "non-equivalent reductions of physics to itself" (e.g., particle and field formulations). His use of the term 'reduction' obscures whether he is concerned with formulations of physical theory, with the ontology of physical theory or both; thus, it is not entirely clear whether his objection is a version of the one we are currently considering or the fourth one. In any event, he is concerned with non-uniqueness of either physical theory or physical ontology.

notational. For example, since notational differences appear to constitute a difference in ideology, the basis for ideology appears to be affected directly. However, if such apparent ideological differences do not have any ontological or epistemological consequences,¹⁰⁷ then the notational differences plausibly do not count as significant ideological differences.¹⁰⁸

Suppose, on the other hand, that the case is not one of alternative formulations of the same theory, but one of formulations of alternative theories; and suppose that the two theories are empirically equivalent (i.e., compatible with all possible evidence and methodological considerations). That is, we have formulations of two theories, each of which satisfies the general characterization of what physics is and which are such that there are no evidential or methodological grounds for choosing between them: does this present a serious challenge to (P2)?¹⁰⁹ The idea behind an objection to (P2) based upon this

¹⁰⁷ For example, the same attributes are expressed by different terms and the same explanations can be formulated in different terms.

¹⁰⁸ The issues here are more complex than we have space to explore: when a case of theoretical equivalence of the type described holds is not clearly defined or easy to establish. We are content to relativize a development of physicalism to any member of a class of formulations of the same theory, since, by assumption, the differences between members of such a class are merely notational and not of any metaphysical or epistemic importance. See Quine (1975b), Glymour(1971), and Horwich (1982) for discussions of theoretical equivalence.

¹⁰⁹ The situation described here is one which Quine (in Quine (1975)) seems to have doubts about; as we understand him, he hypothesizes that any such situation is an instance of the first case considered (i.e., notational variants). If Quine is right, that does not

possibility might run as follows: even though we have given a principled characterization of what physics is, we have pinned down neither a unique physical theory nor the physical bases, and, since there are no considerations that could, the bases are indeterminate.¹¹⁰

The two possibilities that such a case can exhibit are (i) the two theories are logically compatible and (ii) the two theories are logically incompatible. In either case, the argument to the denial of (P2) is a non-sequitur; the fact that a characterization of physics does not uniquely determine a physical theory with which to develop the bases does not entail that there are no determinate bases in terms of which the theses can be formulated. Our explanation of this varies slightly in the two cases, and we shall consider them separately.

If the two theories are compatible, then we might view them as two different ways of "cutting up the pie", each of which counting as an empirically correct physics; and we can take them both as true. If this is possible, then as physicalists, we would require that each physics provides grounds for developing physical bases and physicalist

damage the physicalist position if our response to the first case stands. We shall take no stand on the correctness of Quine's claim, but will consider a possible line of reply for the physicalist if Quine is wrong.

¹¹⁰ To buttress the argument, it might be claimed that if there are two such theories then there are indefinitely many. We shall not consider this in our discussion below. The notion of theoretical equivalence is still badly in need of further development; as a result, the individuation of theories, given a class of theory formulations, is an unclear business.

theses. Thus, we would have two true but very different physical theories, each of which provides the bases for a physicalist picture of nature and scientific knowledge; we would have as a result two different "total theories of nature"¹¹¹ each based upon one of the alternative physics here hypothesized and each structured by physicalist theses. In this case, there is no reason to believe that (P2) is threatened, since relative to a given physics, pending the outcome of other objections to be considered shortly, the bases will be unique and determinate.

What if the theories are logically incompatible?¹¹² Either there is a fact of the matter as to which (if either) is true or there is not. If there is, then, even if it is forever beyond our ken to tell which, there will be no problem with determinateness of the physicalist bases (pending the outcome of other objections) since, relative to whichever is the true one, the bases will be unique and determinate.¹¹³ What if there is no fact of the matter as to which is the true physics?

¹¹¹ Smart (1978) suggests that this is not true; his thought is that much of the "higher level" theory in science might be invariant relative to different formulations of fundamental physics. However, even if Smart is right, our claim is not affected as long as relative to each true formulation of basic physics physicalist theses are satisfied.

¹¹² This is the case that Quine has his doubts about.

¹¹³ Putnam has argued vigorously in recent years against such a form of "metaphysical realism". We are sympathetic to Putnam's position although, here, we take no stand on it; we are considering the consequences of assuming that certain possibilities obtain rather than whether or not they do or do not obtain.

First, pending the outcome of other objections and relative to one or the other of the two theories, the physicalist bases will be unique and determinate. Second, in this case, the indeterminateness of the bases apparently depends on the question of whether it is possible to have alternative theories which (i) both count as physics, (ii) are empirically and methodologically correct relative to all possible evidence and all considerations bearing on truth (or, rational acceptance), (iii) are logically incompatible and (iv) are such that there is no fact of the matter as to which is true. If such a situation is possible, although it appears that we have a case of indeterminateness of physical theory, it is not clear that the physicalist need feel threatened.

Our reply is that, in such a situation, we can develop our total theory of nature and our physicalism on the basis of one or the other but not both of these theories. Such a choice may be one of the points within the development of science at which a (real) conventional choice is called for. The issues raised by this possibility concerning realism, conventionalism and truth are significant; but, it is not clear that any of this poses any difficulty for (P2) or physicalism generally; physicalism is a doctrine about matters "internal" to scientific knowledge and its ontology. The issues raised here, though important, fall outside of the structure of science. Within the indicated conventional choice, the physicalist bases are to be developed and the theses are to apply; whether it is a real choice and how we go

about making it, if it is, are antecedent to the development of a science structured by physicalist principles. The problem posed by the alleged possibility is one which exists in science whether we are physicalists or not; once it is resolved by conventional choice, the physicalist program proceeds without difficulty. It is essential to our view of the physicalist program that it not be maintained that all questions of determination be resolved by appeal to a physical basis. Physicalism is, rather, to be construed as a doctrine concerning certain (but not all) features of the structure of scientific knowledge and concerning the structure of the natural order that science is about. The development of such structures falls "within" a number of choices that must be made on non-physicalist grounds.¹¹⁴

To summarize to this point: the first class of objections against (P2) (i.e., those objections premised on the possibility of alternative formulations of physics given a general characterization of what physics is) do not seriously threaten (P2); in each case, we saw that it was possible to relativize the development of physicalism to a given formulation, although there were different interpretations of what

¹¹⁴ The reader is reminded that the motivations of the physicalist program as we conceive of it largely concern the gains that would result from achieving a body of knowledge structured in a certain way. And, although pursuit of the program requires that the theses play a normative role in the development of science (roughly, they guide scientific progress toward the development of knowledge with a physicalist structure), it is a substantial error to view physicalist doctrine as the key to resolving all philosophical and methodological questions; this places a demand on the doctrine that neither it, nor any other, can bear.

was going on in the various cases considered. We now turn to a second class of objections each member of which assumes that (i) there is a general characterization of what physics is and (ii) there is a given formulation of physics that will be used for the development of the physical bases: these objections are to the effect that, even given a single formulation of physical theory, there are sources of indeterminateness that undermine (P2).

The first such objection is that, given a theory formulation, there is no determinate way of sorting the terms in the language of the theory into those which have referential import and those which do not. The idea is that theory formulations embody mathematical or other apparatus which serve functions in the theory other than that of denoting objects, classes or attributes. For example, the theory may contain machinery whose job it is to facilitate deductions of one kind or another; or, the mathematics that are employed in the theory may bring with it trappings that are not relevant to the subject matter of the theory (e.g., solutions to equations that can have no physical interpretation). These claims by themselves are not sufficient for undermining (P2), however; the objector must claim in addition that it is not in general possible to sort out the two classes of terms. A full discussion of the issues raised here is beyond the scope of the current project; hence, we shall pause briefly over two points before moving on.

First, one way of reading this issue is as an instance of one source of referential indeterminacy that Quine has argued for: i.e., indeterminacy of reference resulting from the indeterminacy of what the referential apparatus of a language is. Whatever apparatus is identified is relative to a manual of translation of which there are many that are equally acceptable. We do not propose to talk now about the indeterminacy theses.¹¹⁵ Our strategy, in the current context, is to "acquiesce" in the semantics of the mathematical language adopted to the extent that the terms are identified; and then, the problem is one of finding physical interpretations for those terms (or, for as many of those which have such interpretations). That there are many ways of parsing a language, if true, is (as in the last class of objections) not anything which the physicalist need claim is resolved on physicalist grounds; the isolation of the bases and the development of the program in science only presupposes some resolution or other.¹¹⁶

Second, the above mentioned strategy points to the fact that it is a part of the physicalist program to use whatever means are possible for sorting terms of a formulation into those with empirical import and those without: this is a major component of the theoretical and

¹¹⁵ See Chapter 5.

¹¹⁶ As we shall see below, our strategy here of sorting out the sources of indeterminacy, which are "prior" to the formulation of the doctrine of physicalism and which are not resolvable on physicalist grounds, does not mean that there are no significant determination claims that the physicalist can make; there are several claims of determination relative to physicalist bases, once they are identified, that are important in science.

experimental enterprise in physics. It is an activity internal to the conduct of science to determine which elements of the theoretical vocabulary have empirical import and which ones do not; the possibility that there are always alternative ways of construing the referential aspects of a language and no fact of the matter concerning which is correct is, first, an undemonstrated claim and second, even if true, not a serious threat to the physicalist program. If there are alternatives, then choices must be made; and, it is relative to those choices that the physicalist bases and theses are to be developed. At worst, therefore, we are now in a position of not having identified which of many possible construals of the physicalist ideology will play a role in the actual development of the bases: more work is called for.

The next objection to (P2) runs as follows: assuming that we have a formulation of physical theory and that we have identified the terms of the theory with referential import (i.e., terms that pick out individuals, classes or attributes), there is no fact of the matter regarding what it is that they pick out; hence, even if we can make a case for the determinateness of the bases for ideology and doctrine, the basis for ontology is not determinate. Again, the issues here lead us far afield; and, it would be inappropriate to review now the various discussions that bear on this argument. Hence, we shall limit ourselves to a few comments which suggest a way of reducing its force.¹¹⁷

¹¹⁷ See Putnam (1979), Quine (1975a), Field (1974), Hellman (1978)

Our view will be familiar: for the physicalist program in science it is okay to relativize a formulation of physicalism to some selection of an interpretation of the language of our physical theory. Relative to that selection the bases will be determinate: a determinateness by fiat! From "within" the physicalist program and science conducted relative to that selection, there is no problem of determinateness even if there are deeper issues about how our science "really attaches to the world", if it does so at all. Within science, we will be able to tell a good story about reference to and knowledge of nature, despite the gloomy possibility raised here that there is no fact of the matter about what we are talking about. This case, like those considered earlier, is a case in which we have numerous alternatives and no clear grounds for choosing among them: the suggestion is that there is an element of relativity that is unavoidable but not destructive of the program. We relativize to some conception of what the objects of our inquiry are. Of course, if there are ways around the indeterminacy cited here so much the better.

The idea that there is a point beyond which our choices are not objectively determined is one which must be taken seriously. From the point of view of the current project, it is important to distinguish two types of such indeterminacy: (i) within the program, indeterminacy relative to the physical basis which is prohibited by the theses of the program for claims within science and (ii) outside of the program,

and, especially, Goodman (1977, 1978).

indeterminacy relative to all rational considerations which is thus unavoidable and which must be resolved so that the program (as well as all other rational pursuit) may get off the ground: it is within the context of such resolutions that the physicalist program is formulated and pursued. This claim is only a problem for the monopolistic physicalist who wishes all knowledge to be related to and determined by the physical bases; ours, clearly, is a more modest physicalism concerned with the structure of knowledge and of reality as conceived from within science.¹¹⁸

One last remark: the issues raised here about (P2) which we have responded to with a certain amount of relativism and conventionalism should not be confused with the issues raised by physicalists about the indeterminacy of reference. Field and Friedman have contended that there are reasons for believing that Quine's various theses of indeterminacy¹¹⁹ have not been adequately established; similarly, they are

¹¹⁸ Goodman's important exercises in alternative ontological specifications, given a theory and the referential apparatus of the language in which the theory is formulated, are a reminder of the existence of the issues underlying the present discussion; they signal the difficulties involved for someone who wishes to isolate one interpretation as "special" above all others. In his recent reflections upon the significance of the Skolem-Lowenheim theorem, Putnam has also challenged the cogency of an intended interpretation for our theories of the "world". In our strategy for meeting the objection above, we have banked on being able to distinguish the issues as considered from within a given theory and associated interpretation and the issues as considered from outside. This distinction is not the same as Carnap's distinction between internal and external questions about a theory and ontology; see Carnap (1967b). However, earlier Carnapian writings distinguish between "metaphysical" and "empirical" realism; see Carnap (1967).

in dispute with Putnam on the same point. What is interesting is that Putnam and Quine are not clearly in agreement on the moral to be drawn from the existence of indeterminacy: Quine rejects semantical phenomena as not real while retaining physicalism, Putnam seems to acknowledge the reality of semantical phenomena but rejects physicalism as a general doctrine. We take all of these issues to be concerned with the prospects of the physicalist program within science: there the physicalist must be assuming that the bases are determinate (to within the relativity we suggest¹¹⁹) and the issues concern whether or not physicalist science can tell a story in which semantical phenomena are "determined by the physical facts". The objections against (P2) we have been considering concern an indeterminateness of the ontology of physics. Since, for the physicalist, the ontology of physics constitutes a basis (within science) in terms of which determinacy and indeterminacy claims are made, the indeterminateness here is not of the physicalist variety. Resolution of this indeterminateness is required by the physicalist doctrine, although there is no reason to require that the resolution be made on physicalist grounds. As long as it is made some way or other the physicalist program can proceed. We shall not now discuss the grounds that may be relevant to making such resolutions.¹²⁰

¹¹⁹ I.e., indeterminacy of reference, translation, and mentalistic psychology. See Field (1975), Friedman (1975) and Chapter 5 below.

¹²⁰ Putnam's concerns about retrospective reference assignments and the need for a full theory of human rationality are all in the context of arguing, not that the physical bases are indeterminate,

Another objection to (P2) that we shall mention may be quite serious; but, we are in no position to assess its merits. The argument is, roughly, that even if we can settle upon a formulation of physics and an identification of the referential terms and their intended interpretation, there is a source of indeterminacy of ontology and doctrine which originates from the very nature of certain kinds of theory (e.g., quantum mechanics). The argument is that there is no fact of the matter about what the physical facts are (or, as some would have it, any facts) when there is no actual observation taking place; and, even if there is an observation taking place, the facts are statistical facts. Given this indeterminateness of the physical facts and truths, what is left of (P2)? Such a line of argument could be quite fatal to the strategy that we employ in assuming an evaluation of physical terms and sentences at every region of spacetime. If there is no fact of the matter concerning the past, present or future (not to mention possible) states of a region of nature unless an observation is taking place, then there is no fact of the matter about the truth

but that it is utopian to suppose that the physical bases (or physicalists) are up to the task of determining semantical facts (within science) as required by the program. This issue is one which concerns the truth of the theses and not their lack of content because the bases are indeterminate. See Putnam (1975). Similarly, Boyd's argument for realism must be viewed as an account from within science of how we are to account for the success of science: it presupposes a resolution of any indeterminacy of the facts science can appeal to in telling the story of its own success. See Boyd (forthcoming). This is surely the resolution that philosophers concerned with certain deeper philosophical problems do not assume; but, to make such resolutions is the only way to proceed with science (physicalist or not).

value of sentences describing unobserved regions. But, (P2) requires that there be a fact of the matter for every region regarding its physical state and regarding whether it satisfies sentences in the language of physical theory regardless of whether an observation is taking place at that region. The problem is surely doubly serious for counterfactual assertions and possible but not actual states of nature. We shall leave it as an open issue whether or not there is anything in this objection for the physicalist to be worried about.

3.4 PRESUPPOSITION 3

We turn now to (P3), the claim that there is justification for appealing to a characterization of physics in developing the bases for physicalism. The point of identifying (P3) as a presupposition is to underscore the fact that our strategy for developing the bases is a motivated one not subject to serious objection. That is, if (P3) were false, then our development of the bases would not be motivated from the point of view of developing the physicalist program. The issues here are of, at least, two types: the first concerns the propriety of appealing to physics in developing the bases for physicalist theses rather than adopting an alternative strategy (e.g., is there something wrong with appealing to physics?, does this approach trivialize the theses or does it commit us to untenable forms of conventionalism or essentialism about the nature of physics?). This kind of issue concerns the propriety of the strategy rather than its prospects

for yielding a successful program; it raises the question of whether there is something fundamentally wrong with our approach. The second kind of issue concerns the motivations behind the program as a whole (e.g., what's the point of developing a set of theses concerning bases that have been developed from physics?, why should theses concerning a set of bases developed from physics be of any interest at all?).

Again, (P3) is not strictly a logical presupposition of the theses; but, if they are to be contentful and motivated, then our strategy had better be justifiable in those terms (i.e., it leads to the development of bases that are components of contentful and motivated theses). We shall now explore a number of objections that have been, in effect, directed against (P3).

The first objection to (P3) concerns the propriety of our strategy: the claim is that appealing to physics in characterizing the bases trivializes the theses. Earlier, we considered and rejected this claim; we add only that if the point of the program is in part to "accord to physics its rightful place in science" and the way to do this is to formulate theses that relate the rest of science to physics and its ontology, then there was little choice in deciding upon a strategy. The issue of trivialization arises in connection with how we characterized physics; that our characterization does not trivialize the theses is true because (i) the generality of physics does not guarantee the ontological theses, (ii) the claim of supervenience was

not built into our characterization (contra Quine), (iii) theses about vertical explanation in science do not follow from the characterization of physics in terms of physical explanation of all matter, interaction and dynamics; and (iv) our characterization of physics isolates it as a proper part of all natural science, thereby making possible significant relations between it and other branches of natural science (i.e., we rejected the identification of physics with all of natural science).

The second objection runs as follows: the strategy adopted depends critically upon accepting a definition of what physics is; given this definition, many of the standard objections to physicalism have been swept aside (e.g., objections concerning principled divisions in science, scientific growth, indeterminacy of the bases). But, the strategy is defective since it depends upon what is either an arbitrary conventional stipulation of what physics is or, alternatively, an essentialist thesis concerning the nature of physics; neither of these alternatives is acceptable. Without entering into the many issues raised here, we shall respond to the objection by denying the premise that our strategy depends upon a conventional or essentialist claim concerning what physics is; whether or not these positions are really unacceptable is something we shall bypass. Rather, our view is that in our strategy we adopted a theory of physics and science as we currently conceive of them; this theory is subject to revision if good grounds are provided for such revision. But, our picture of physics is

not an arbitrary convention nor is it based upon any pernicious form of essentialism.

We conclude that the first two objections do not pose any serious threat to (P3); thus, our strategy is neither a trivializing one nor one which is committed to unacceptable theses about the nature of physics. Our approach is based upon the idea that physicalism, being a doctrine that concerns the structure of science, can and should exploit any theory of science and physics that, to the best of current knowledge, reflects the way they are. The nature of such theories raises issues that fall outside of the current project, but there is no reason to believe that such theories must lead to trivialization of physicalist theses or that they must be committed to conventionalism or essentialism.

The next objection that we shall consider does not bear upon the internal acceptability of our strategy, but rather concerns the grounds for adopting the strategy at all. There are two ways of looking at this: (i) why adopt this strategy as opposed to some other? and (ii) what is the point of adopting any such strategy at all? Whereas the first construal appears to give some credence to the development of the program, the second appears to doubt the value of embarking upon such a project at all. Our reply to the first is rather brief: viz., there are no currently known alternatives which have a chance of working (i.e., of providing a characterization of the physical bases). We shall now move on to the second construal.

We choose to pause over this line of objection because a number of quite notable thinkers (e.g., Goodman, Chomsky) have made claims that are easily construed as challenging the point of any enterprise that would attempt to relate the phenomena, terms and theories of the special sciences to bases that are developed in terms of physics. Since this type of issue goes directly to the heart of the physicalist program, we shall take some time to respond directly.

To paraphrase some remarks made by Chomsky:¹²¹ so what if psychology doesn't reduce to physics, physics doesn't reduce to psychology either. If what is meant is that the failure of "reduction" does not constitute any serious loss (perhaps because the various branches of science are supposed to be autonomous in some sense), then the remark is seriously mistaken. Our discussion in Chapter 1 showed that there are a number of important gains to be had as a consequence of establishing certain relations between branches of science; it is simply not true that there are no important gains to be had if the physicalist program were to be successful. Further, the autonomy of the sciences is hardly a thesis that can be assumed without argument, and it is pretty clear that such autonomy yields fewer cognitive and non-cognitive gains than the alternative physicalist program we are discuss-

¹²¹ See Chomsky (1980), p. 20. Because he was treating physicalist issues in terms of an identification of physics with all of natural science, we shall be taking some liberties in our construal of his comments there. Perhaps, Chomsky would not make the same kind of remarks relative to the kind of physicalism we are attempting to develop.

ing here.

Alternatively, if what is meant is that the program for reducing psychology to physics is no more and no less of a failure than the program for reducing physics to psychology,¹²² and hence, there are no interesting morals to be drawn regarding either science as a consequence of the failure of reduction, then the point is much too quick.¹²³

First, if the remark is to be construed this way, then it rests on the doubtful assumption that the physicalist program is failing as abysmally as the program for reduction of physics to psychology: it is obvious that the two programs are not failing to the same degree, in the same ways or for the same reasons.¹²⁴ Second, since the point

¹²² See Goodman (1978) and Putnam (1979) for explicit statement of this claim.

¹²³ The use of 'reduction' and its cognates is here intended to be quite general covering a wide range of relations between branches, not just "classical reduction". Similarly, the program of "reducing physics to psychology" is used quite vaguely here to include a wide range of such programs, not just the program for phenomenalist reduction as developed by either Goodman or Carnap.

¹²⁴ The vast literature in recent years concerning "reductionism" in science reveals a number of (at least partial) individual successes (e.g., in physics, chemistry, biology). The failures generally are not total in the sense that, although very strong reductive relations are seen not to be plausible, weaker, but nonetheless significant, relations are seriously entertained instead. This is not clearly true of phenomenalist reductions or other kinds of psychologistic reductions. Goodman has urged the exploration of various types of reductive relations in both types of program: most of the response is into the physicalist, not the psychological, reductive programs. This may be merely a reflection of the temper of the times; but, it may also be a reflection of a differ-

of the two programs is probably quite different, then the failures have quite different significance. Hence, for example, the failure of phenomenalism might signify the demise of a certain epistemological goal (e.g., foundationalism) whereas the failure of physicalism might signify the failure to attain certain ontological goals (e.g., monism) or epistemological goals (e.g., unity of science with respect to explanation and determination). Depending upon one's outlook, one or the other of these failures might be more important; indeed, the "quest for certainty" has not been a very important goal for most philosophers in the past thirty years, whereas the quest for a science with maximal explanatory power has always been a goal of philosophers and scientist's alike. The point is that Chomsky's remark overlooks the fact that the two programs have radically different motivations and that the motivations of physicalism are widespread motivations within philosophy and science while the motivations of phenomenalism and related programs are not. To discover the failure of the reducibility of psychology to physics is to discover sad news that could lead to a revision of our psychology or our physics or our conception of the structure of science; to discover the failure of phenomenalism would have little effect on us now, although it is granted that there was a time when it was sad news.¹²⁵

ent assessment of the merits and prospects of the two programs.

¹²⁵ Our concerns at this point are not with the prospects of the success or failure of the program, but only with the propriety of our strategy. Later, we shall briefly consider the empirical situation with regard to the program: writers differ enormously on this is-

The final objection that we shall consider is that there is "no preferred direction of reduction in science"; hence, to formulate physicalism in terms of relations to bases developed from physics is an arbitrary, monopolistic move. There is little to argue about here since to say that there is no preferred direction of reduction is not to say that there aren't a number of very interesting directions, one of which is reduction to physical bases. Further, however, as suggested above, some directions are more interesting than others; and, it was the burden of Chapter 1 to suggest that physicalist reduction is about as important a direction of reduction as there is in science. Under no construal should we be taken as claiming that physicalist reduction is the only direction of any possible interest; we do hold, however, that if one is interested in ontology and explanation, then such reduction is to be preferred over the known alternatives. So, the charge that physicalism is an arbitrary, monopolistic program is not quite right; the strongest position we need take is that physicalist reduction is potentially of great importance: this we can say without ruling out other forms of reduction. But, other forms of reduction have the burden of establishing their ontological and epistemic relevance if they wish to compete with physicalism (for which such relevance is affirmed by existing practices in science); it is this that the alternatives have not done. The alternative programs (e.g., phenomenalism and other forms of psychological reduction) have different

sue with the views ranging from "it's obviously failing" (Goodman) to "it's obviously succeeding" (Boyd).

goals from physicalism. Hence, there is a sense in which physicalism does have a monopoly; but, this monopoly is due to a lack of real competitors rather than an act of legislation by physicalists.

In summary, then, there are no reasons for believing that (P3) is false; hence, there is no reason to believe that the bases are arbitrary or pointless. This concludes our discussion of the physical bases and the presuppositions of the physicalist theses; in the next chapter we turn to a development of specific theses that will jointly satisfy the criteria of adequacy set forth in Chapter 1.

4. THE THESES OF PHYSICALISM

The goal of this chapter is to develop a set of theses which give adequate expression to the physicalist concerns about ontology, objectivity, and explanation within natural science: theses which accord to the physical bases, as we have developed them above, a special place relative to the ontologies and doctrines of the other branches of science. Although the theses we shall put forward are connected historically with the classical theses of definitional and derivational reduction, we want to emphasize the differences. In particular, since our standard of adequacy requires that the theses must have the property that if the program based upon them is successful, then the goals of physicalism must be satisfied, we shall take to heart the various results of Chapter 2. For example, we reject the claim that ontological theses can be expressed in "the formal mode". On the other hand, purely ontological versions of physicalism cannot meet the explanatory goals of the program. And, although we reject classical reductionism, we hold that "reductive" theses are required for adequate formulation of physicalist ontological and explanatory theses. Hence, an adequate version of the doctrine must consist of linguistic, non-linguistic and reductive theses. Further, in light of the various failures of past formulations, the level of abstraction of the theses must be such that they express physicalist concerns while allowing for variability among branches of science.¹ Finally, we note that although the theses are

¹ See Fodor (1975, 1978) for discussion of how psychology, though de-

taken to apply to all of natural science, they are not taken to exhaust the important relations that hold between the various branches of science. We turn, now, to a consideration of the physicalist theses; as we proceed, we shall check for the adequacy of the formulations as well as their vulnerability to various objections.

4.1 THESES CONCERNING ONTOLOGY

A formulation of the physicalist ontological position must give expression to the idea that the physical ontology is basic within the natural order. To make this idea more precise, it is necessary to say what is meant by "the natural order" and to say what being "basic within the natural order" means. We shall discuss the former below² in the section concerned with the scope of the theses. Preliminary to clarifying the latter, we observe that, as noted above, it is necessary to treat the various ontological categories separately in a full development of the physicalist position. However, we shall not do this in this project for reasons of space. Rather, we shall focus our attention upon attributes (i.e., properties, relations), about which we make the following assumptions. First, we reject completely an "extensional" approach to the individuation of attributes; a full development of the reasons for this would take us too far afield, but, roughly, our view is that "intensional distinctions" are of critical

parting from other sciences in many respects, might still be incorporated into a physicalist framework for science.

² See Chapter 5.

importance in science: an account of science which does not recognize such distinctions cannot provide an adequate account of scientific explanation and understanding.³ Second, we take it as obvious that there are non-physical as well as physical attributes; and, we reject as obviously false a physicalist position that holds that every attribute is a physical attribute. Third, we reject as false the view that, while denying identity of attributes, asserts "token identity of attributes".⁴ This view founders either because it leads one to the view that there are "property particulars" or that somehow it is possible for properties to be distinct but their instantiations to be identical: none of this is particularly cogent on close inspection. Our view is that, if the individuation of attributes is such that some attribute A is not identical to some attribute B, then it is not possible for any instance of A (whatever an instance might be) to be identical to any instance of B. We assume this without further argument.⁵

³ The view that an extensional treatment of attributes supplemented by the view that we can "conceive" of such attributes in different ways, as reflected by the employment of different ideologies, has a serious flaw. Either alternative conceptualizations are taken to be based upon features of the (extensionally construed) attributes or they are not. If not, then we appear to be committed to viewing our conceptualizations as arbitrarily related to what they are conceptualizations of. If they are based on features of the attributes, then either we are landed in a serious regress or we are ultimately committed to non-extensionally construed attributes.

⁴ The relation between "type" and "token" identity claims is sometimes obscured by various theories of the nature of events and properties; see Kim (1979) and Swinburne (1982) for relatively lucid discussion of the relevant distinctions and relations between identity claims for events and such claims for properties. We are exclusively concerned with properties in this discussion.

The problem set by these assumptions is to develop a view that is compatible with them and which expresses a significant sense in which the physical ontology is basic within the natural order. Having given up the obviously strong, but obviously false, views of type and token identity, we must search for alternatives that are compatible with the assumptions, adequate by our criteria and not obviously defective.

The physical basis for ontology developed above looked somewhat as follows: (i) we assumed a spacetime coordinate system within which spacetime regions could be precisely individuated (i.e., a region R is identical to a region S just in case R and S contained the same spacetime points; (ii) we assumed a basic class of physical attributes which can be evaluated at each spacetime region; (iii) we assumed that a specification of these basic attributes was sufficient for determining all complex or higher order physical attributes that can be built up from the basic attributes; (iv) hence, the actually instantiated physical ontology is specifiable by the distribution of the basic physical attributes over every region of spacetime; and (v) different possibilities for the actually instantiated physical ontology are specifiable in terms of alternatives from the actual distribution of such attributes. We left unsolved the problem of how to extend this development to a full development of the physical ontology which includes a treatment of all ontological categories (e.g., states,

⁵ See Swinburne (1982) for discussion. Also, although we deny identity theses for attributes, we allow for the possibility of type or token identity theses for other ontological categories.

events, objects, etc). For our present purposes, the physical ontology of attributes as roughly outlined will suffice.

In what follows, we shall develop the physicalist ontological position by specifying a set of relations which elements in the ontologies of the special sciences must bear to the elements in the physical ontology. To be adequate, the theses must specify relations which, if they are satisfied, are sufficient for realizing the ontological goals of the program and which are compatible with our assumptions. We shall restrict our discussion to attributes that are members of the ontologies of the special sciences.⁶

In considering the ontology of the other parts of science, we assume that, as in the case of physics, the ontologies of the special sciences can be developed in terms of basic and complex or higher order attributes; further, all such attributes are assumed to be instantiated, if at all, in regions of spacetime. Thus, as with the physical ontology, every basic attribute in the ontology of a given special science can be evaluated at each spacetime region; and, such an evaluation determines which other complex or higher-order attributes are instantiated in which regions. Further, as with the physical ontology, we distinguish the actually instantiated ontology of a given special science from the possible alternatives (i.e., alternative distribu-

⁶ Thus, the physicalism considered here is not completely general regarding all attributes; see below (Chapter 5) for discussion of the scope of the theses.

tions of attributes over regions of spacetime.)⁷

Now, given the attributes in the physical basis and the attributes in the ontology of the special sciences, the physicalist ontological thesis is that, associated with each attribute that is actually (or possibly) instantiated in some region of spacetime, there is a physical attribute that is actually (or possibly) instantiated in that region. As stated, this is a rather trivial, vague and uninteresting claim. Mere co-occurrence in the same region does not provide much bite to the idea that the physical ontology is basic. And, although the generality of the physicalist ontology does provide some bite to the physicalist claim, it is minimal. What, then, is the nature of the association between physical and non-physical attributes such that the former is basic?

There are two features of the association that are important: (a) the physical attribute is a sufficient condition for the non-physical attribute and (b) the physical attribute is not merely contingently associated with the non-physical attribute. Thus, the ontological thesis of physicalism that we propose is:

⁷ The ontology of a science includes all attributes subsumed under the theories of that science, whether or not the attributes are actually instantiated. Thus, consideration of possible, but not actual, states of nature does not involve augmenting the ontology of a science; it only means considering which members are actually instantiated and which are not.

(T1) For each non-physical attribute in the ontology of the special sciences and for each region of spacetime, if an attribute is (actually or possibly) instantiated in a region of spacetime, then there exists a physical attribute which is a nomological sufficient condition for that non-physical attribute and which is instantiated in that region.

This thesis captures both the direction of the determination of the non-physical by the physical as well as the appropriate modal force of that determination. The direction is secured by the requirement that the physical attribute be a sufficient condition for the non-physical attribute; the converse need not in general hold. And, the determination must be at least nomological⁸ to avoid, what is surely not in the spirit of the program, the possibility that a physical attribute could be a sufficient condition for incompatible non-physical attributes.

A consequence of (T1) is that, for each attribute in the ontology of the special sciences there is a class of attributes in the ontology of physics such that each member is a nomological sufficient condition

⁸ Fodor has suggested that this much modal force (i.e., nomologicality) may be too much and that the requirement should be something weaker although it is not clear how to express intermediate amounts of modal force between contingent and nomological. In Davidson (1970) there is a mention of "degrees of lawlikeness" which is not clarified; and Fodor speaks about a regularity having counterfactual force without its being nomological. The issue turns on what nomologicality consists in, an issue that no one has adequately addressed and that we do not propose to take on here. We shall issue the following caveat on the condition appealed to in (T1): if an intermediate degree of modal force between contingency and nomologicality can be identified, it may be a serious candidate for replacing the condition of nomologicality employed.

for the non-physical attribute in question. Further, this thesis entails the "supervenience" of the non-physical on the physical (i.e., two regions alike in all physical respects must be alike in all non-physical respects). The formulation is an especially strong claim in that the physical nomological sufficient conditions for non-physical attributes are to be located in the same region as those non-physical attributes.⁹

We note, further, that (T1) expresses an "ontological" thesis which involves no identifications of the non-physical attributes with physical attributes; only the regions in which they occur are to be identified. Rather, (T1) attempts to capture the idea that the physical attributes determine what non-physical attributes occur; this is, we take it, the first step toward clarifying the idea that physical phenomena "realize" non-physical phenomena in nature. Below, in our discussion of the explanatory theses, we shall add to this development by introducing the notion of a "realization theory": a kind of theory which purports to explain in virtue of what physical attributes realize non-physical attributes. Although, the ontological thesis put forward eschews identification of attributes, it is to be kept in mind that it is compatible with this position that there might be identifications involving other ontological categories (e.g., events, states);

⁹ This could spell trouble if the physical conditions of realization for a given non-physical attribute occur in a different region from the realized attribute (e.g., the case of "broad", as opposed to "narrow", mental states).

such identifications depend crucially on what one takes an event or a state to be. Finally, although a nomological relation is required between given physical and non-physical attributes, this does not guarantee that for each such association there will be a law-statement expressing that relation in the relevant physical and non-physical vocabularies. Of course, identification of such relations is an important goal of science nonetheless.

Thus, the physicalist ontological thesis is a reductive, non-linguistic claim to the effect that for each non-physical attribute actually (or possibly) instantiated in some region, there is a physical attribute instantiated in that region which is a nomological sufficient condition for it. Hence, for each non-physical attribute, there is a class of attributes in the physical basis which consist of all and only the physical nomological sufficient conditions which co-occur in the same region as the given attribute. The requirements of nomologicality of the association and of the attributes being located in the same region when they are instantiated put stringent constraints on the mapping from the class of non-physical attributes into the base class of physical attributes. As we shall see shortly, these constraints will be objected to on the grounds that (1) they are (nonetheless) too weak to determine a unique mapping and (2) they are too strong to be plausible. In any event, these constraints do give content to the idea that the physical ontology is basic within the natural order. The actual development of an ontological system structured

in accordance with this thesis is a quite arduous task which is just barely off the ground; a principal reason for this is that "inter-branch sciences" (e.g., neuropsychology) in which the task of identifying the associations between physical and non-physical attributes are immature in most cases. Further, of course, the treatment of other ontological categories must also be incorporated into a full development of ontology for science. Rather than pursue this task, which is quite dependent upon future advances in both philosophy and science, we shall turn to a discussion of the objections to our brand of ontological physicalism.

If our discussion in the last chapter was right, then the theses of physicalism are not vulnerable to the objection that they are trivially true, obviously false or indeterminate because the bases are defective in some way. But, given that the bases are acceptable, there is a second wave of objections that must be dealt with: (1) another triviality objection, (2) a claim that the theses are "utopian" in character, and (3) another indeterminacy objection.¹⁰

During our presentation of the ontological thesis, we mentioned that, by itself, the claim, that to every non-physical attribute instantiated in a region there was associated a physical attribute in-

¹⁰ An objection that would almost certainly be raised by Quine and others of his inclination is that our whole approach to developing the physicalist program is excessively "intensionalist": we have freely helped ourselves to property, nomologicality and related notions.

stantiated in that region, was trivial: there will always be such associations given the generality of physics with respect to regions of spacetime and given that every instantiation of a non-physical attribute is located. Hence, such a view cannot capture the essence of the physicalist position. And, of course, we do not think that it does; we went on to put constraints upon the nature of the association.

These constraints are quite adequate for fending off the objection that physicalist ontological reduction is trivial:¹¹ any such reduction must map attributes into physical attributes that are instantiated in the same region and it must have counterfactual import. However, we may have fended off this objection at a very high price: the thesis, although not being trivial, appears to be "utopian". And further, despite the imposition of these constraints, the thesis may be, nevertheless, indeterminate in the sense that the constraints do not determine a unique mapping and there is no fact of the matter concerning which of conflicting mappings is right. We shall now consider these objections in turn.

¹¹ There are formulations of the physicalist ontological position (e.g., Davidson (1970), Fodor (1975)) which appear to require something like contingent associations only. These formulations are trivially true given the assumptions of the generality of physics and the locatability of the non-physical in spacetime regions.

The idea that physicalism is a utopian program has been expressed in one way or other by a number of prominent thinkers.¹² The exact meaning of the epithet "utopian" is not very clearly discussed however; the general idea is that at the present time it looks as if the successful completion of the program will be forever beyond our reach.¹³ The reasons for this gloomy forecast are equally undeveloped: (1) the program doesn't appear to be working out, (2) it is unreasonably demanding, (3) it calls for "ideal theories", and (4) it may be out of our reach because of our cognitive limitations or because of other epistemic difficulties. The issue is whether it is at all reasonable to expect that the program will be successful; or, alternatively, whether there are principled or practical reasons for believing that the program cannot be successful. The proponents of this objection hold that there are such reasons and conclude that physicalism, as we conceive it, ought not be taken seriously as a research program in science. Or, as Goodman and Putnam put it, the program ought not be taken any more seriously than the phenomenalist program the preceded it.¹⁴

¹² See Putnam (1978, 1979), Quine (1979), Goodman (1978), and Chomsky (1975, 1980).

¹³ Or, at least, it will be beyond our reach for the immediate future; 300 years appears to be a favorite number, although it is not clear why.

¹⁴ In Chapter 5, we shall address the question of whether a strong analogy between the physicalist and phenomenalist program exists.

With regard to (T1), the utopianism objection claims that the general discovery of physicalistically correct mappings (i.e., mappings which satisfy the constraints) from the ontologies of the special sciences into the physical ontological basis is a utopian dream. To assess this claim it would have to be determined exactly what 'utopian' does mean here, whether or not the thesis is utopian in the intended sense, and, most importantly, whether or not being utopian is a serious defect for the physicalist program. In the absence of clear discussions, by the proponents of this objection, of both what being utopian is and what are the arguments for the charge that physicalist theses such as (T1) are utopian, our strategy will be to focus on whether or not being utopian is really a serious defect of the program. That is, for the purposes of our discussion, we shall take it that being utopian means that we can reasonably expect that the totally successful completion of the working out of the physicalist program in science is unlikely; and we shall grant that (T1) is utopian in this sense. Our contention will be that this is not a serious flaw of the physicalist doctrine. We shall develop this point only after we have discussed all of the physicalist theses, since each can be objected to on the same grounds and our reply in each case will be the same.

The next objection to the ontological thesis that we shall be considering is that it is subject to a certain kind of indeterminacy. Unlike the last objection which despaired of our ever discovering any

mappings between physical and non-physical attributes that were compatible with the constraints, the present objection argues that, even if mappings are discovered, there are too many such mappings and no objective grounds for choosing among them. Hence, for any given mapping there is no fact of the matter as to whether it is the physicalistically correct one. (T1) is thus defective because it calls for mappings, from attributes in the ontologies of the special sciences into attributes in the physical basis, which are not uniquely determined by the constraints and for which there are no objective, physicalist grounds for choosing one as the "correct" mapping. Thus, the mappings that (T1) calls for do not satisfy the demands of the physicalist program itself (i.e., the demand that relations in science be physicalistically determined.)

The principal steps in the argument above are as follows:

1. It is assumed that the physical ontological basis is fixed as is the non-physical ontology to be reduced.
2. It is argued that relative to the constraints there is non-uniqueness of reduction of the one ontology to the other.
3. It is argued that this non-uniqueness is indicative of physicalist indeterminacy of reduction (i.e., there are alternative, incompatible reductions and no objective physical grounds for choosing among them).
4. Such indeterminacy is grounds for rejection of the physicalist mappings and the thesis which calls for them as unacceptable

science.¹⁵

In assessing this argument, we shall claim that even if step 2, that there is general non-uniqueness of physicalist ontological reduction, is correct, it does not follow that such non-uniqueness is grounds for rejecting (T1) as not acceptable by physicalist standards.

The first step in the current objection is the assumption that the physical bases are determinately specified; of course, those who were not persuaded by our discussion in Chapter 3 will not accept this assumption. However, for the purpose of clearly distinguishing different types of objection, the assumption should be made for the space of the argument. On the other hand, if we are successful in fending off the current objection, the critic can retreat to the objection based upon denying the determinacy of the basis.¹⁶ Further, the present argument assumes that the ontology to be reduced is likewise determinate. Thus the problem facing the physicalist is the construction of a reductive mapping between the two ontologies which satisfies the constraints¹⁷

¹⁵ The issues raised by this objection will be familiar to those who have followed the debates concerning Quine's indeterminacy thesis. The situation here for the physicalist thesis and the reductive relation that it postulates is alleged to be quite comparable to the situation that Quine sees translation, reference attributions, and psychological theorizing as being in. That is, physicalist reduction of one ontology to another is seen by proponents of the current objection as being another instance of the general physicalist "indeterminacy" of intentional and semantical phenomena. See Putnam (1979) for discussion of this kind of objection to physicalism.

¹⁶ See Putnam (1979) for precisely this strategic move.

¹⁷ Recall that the constraints are that the mapping take non-physical

and which does not have equally acceptable, but incompatible, alternatives for which there are no grounds for rejection.

The second step in the argument is the claim that there is, in general, non-uniqueness of physicalist ontological reduction: i.e., that given the two ontologies and given the constraints there is more than one mapping from the ontology to be reduced into the physical basis which satisfy all the constraints and which are not jointly acceptable because they are logically incompatible with each other. The arguments which bear on this claim have been discussed in numerous places¹⁸ and will not be reviewed here. We shall assume that, if it is at all possible to construct a mapping which satisfies the constraints, then there will always be alternative mappings which are incompatible with each other but which all satisfy the constraints. Although some may believe that this is too large a concession, our view is that, as we shall now argue, the truth of this claim is by no means fatal to the physicalist program in science.

The reason that non-uniqueness is not fatal to (T1) is that, as described, it is an embarrassment of riches. Theory construction within physicalist science has, in the situation described, a choice to make between one set of sets of nomological sufficient conditions for the

attributes into nomological sufficient conditions that are instantiated in the same region as the reduced attribute when it is instantiated.

¹⁸ See Carnap (1967), Goodman (1977), Quine (1968), Putnam (1975, 1979), and Field (1974).

attributes in the ontologies of the special sciences and other such sets: each of the alternatives serves the physicalist equally well in realizing the ontological goals of the program. Once a choice has been made between the various alternatives, a physicalist representation of nature is structured in the way that the physicalist requires. That there are alternative representations of nature that would serve those requirements equally well is not a difficulty for physicalism as long as it is not required by the physicalist that there be physicalist grounds for breaking the ties between the alternative reductions. The issue becomes that of how it is possible that the physicalist can consistently allow such physicalist underdetermination of the choice between ontological reductions and still be a physicalist about natural science.

To get at this issue, let us consider an alternative way of stating our contention: viz., as long as physicalist demands are satisfied within a representation of nature, it does not matter to the physicalist program in science that, viewed from outside of that representation, there are alternative ontological reductions that could have been adopted for the purposes of constructing such representations. What this means is that, as long as physicalism does not purport to resolve all underdetermined issues that arise in the activity of theory construction in science, it need not feel threatened by the existence of the general non-uniqueness of ontological reduction. However, this leads us back to what is at the bottom of the objector's

claims: both (1) the specific ontological reductions that are formulated in science and (2) the thesis (T1), being a part of natural science, do not satisfy the physicalist's own demands on what counts as acceptable natural science. Thus, even if we turn our focus away from the non-uniqueness of ontological reductions by choosing one of the many equally acceptable ones, we still appear to have elements of the ontology of natural science (i.e., the reductive relations between specific attributes) and an element of the formal system of science (i.e., (T1)) that are not in compliance with physicalist doctrine.

There are two quite distinct issues here, the first bearing on the reductive relations between attributes and the second bearing on the status of the physicalist thesis. In the case of the relations (e.g., the relation that a physical attribute bears to a non-physical attribute that it is a nomological sufficient condition for), it might be asked "What physical facts determine that relation?" or, given (T1), "What physical relation is to be associated with the relation x-is-a-nomological-sufficient-condition-for-y such that this relation is true of specific attributes?" If these questions have no answer, it would be contended that there is a relation in the physicalist system which does not satisfy (T1); and, hence, either it is not an acceptable relation in science or (T1) is false.

The only reply to this line of reasoning for the physicalist is to contend that, consonant with the claim above that in the case of non-

uniqueness we pick one ontological reduction for the purpose of constructing a physicalist representation of nature, the relation between a physical attribute and the non-physical attribute that it is a nomological sufficient condition for is a basic relation in the system: there are no additional physical attributes that constitute a nomological sufficient condition for that relation.

Now, to soften this blow and to deflect the charge that this is just an ad hoc sparing of (T1) of embarrassment, we offer the following considerations. First, it is to be clearly kept in mind that it is an inevitable fact of all theory construction, physicalist or not, that certain choices arise that are a matter of convention and that it is completely unrealistic for the physicalist to claim that, under all circumstances, physicalist grounds exist to determine such choices. Thus, in acknowledgement of this fact, we have allowed that ontological reduction generally constitutes one such point of conventional choice and that, if there are any acceptable physicalist reductions, there are many. Hence, for the purposes of physicalist theory construction, one must choose from among the alternatives. Given all of this, it seems foolish to turn around and require that the given ontological reduction be physicalistically determined by other physical relations within the system. The only alternative to such a requirement is to make the reductive relation not within the scope of the physicalist thesis that requires such determination; and, in the present case, the only way to do that is to make such relations basic within the system.

Second, we shall introduce below¹⁹ the notion of a "realization theory" which is a kind of theory that attempts to de-mystify the relations that hold between an attribute and its nomological sufficient conditions; thus, although they are basic within the system, such relations need not be mysterious.

Third, the relation x-is-a-nomological-sufficient-condition-for-y is like many other relations in science that are not part of the subject matter of science, but rather, are employed when representing and making claims about the subject matter of science. Other examples might include properties and relations of the logical system (e.g., the consequence relation) that is employed in science and properties and relations having to do with explanation (e.g., x-is-an-explanation-of-y). Since the doctrine of physicalism is supposed to be concerned with the subject matter of science, it does not necessarily apply to properties and relations that are introduced for the purposes of studying that subject matter. We say 'not necessarily' because such properties and relations would be part of the subject matter of science if scientific theory construction and its products were the objects of scientific study.

Fourth, therefore, the relation between a physical attribute and the non-physical attribute that it is a nomological sufficient condition for is in compliance with (T1) insofar as it is viewed as basic

¹⁹ In the section concerning the explanatory theses.

within the system. But, the general relation, x-is-a-nomological-sufficient-condition-for-y, might be best viewed as not itself part of the subject matter of science and, hence, not within the scope of (T1).²⁰

To this point, we have considered a strategy for replying to the charge that there is general non-uniqueness of ontological reduction which entails that (T1) requires mappings that are underdetermined by physicalist considerations: we replied that, if there are many acceptable mappings, it is open to the physicalist to pick one for the purposes of scientific theory construction and that, relative to that choice, the physicalist doctrine is satisfied. This led us to the observation that, given such a choice, there appear to be relations in science (i.e., relations between a specific physical attribute and the attribute that it is a nomological sufficient conditions for) that are not determined by other physical relations. We replied that such relations must be viewed as basic within the physicalist system and we offered some reasons why this is not an ad hoc maneuver to save (T1).

²⁰ Unless, of course, that relation can be seen as being determined by physical relations within the system. As I understand it, the project of naturalized epistemology has as one of its burdens the "physicalization" of all the concepts employed in the process and the products of scientific activity; this is so, at least insofar as one remains a physicalist while engaging in the project of naturalized epistemology. The point we are suggesting here and shall develop later is that one need not pursue that project if one is going to be a physicalist. Indeed, if one is going to be a physicalist, one had better be wary of a project that carries the burden just mentioned.

Now, the outstanding issue that has yet to be addressed directly is whether or not the activity of scientific theory construction is within the domain of scientific inquiry. If it is, then it would appear that all that we have said up to this point goes by the boards since, the very choices that we have been discussing are, in general, probably not resolvable on physicalist grounds and would be required to be resolved on such grounds within physicalist science. As we shall see, the physicalist cannot blithely relegate such activities to a point beyond the perimeter of natural science since it is a favored contention of most physicalists that physicalist theses are a part of empirical natural science; and, the theses being a part of natural science is tantamount to science becoming a part of its own subject matter.

The specific objection that we are left with may be formulated as follows: if (T1) is a part of natural science, then the construction of ontological mappings in accordance with (T1) is part of the subject matter of natural science and, hence, must itself be in accordance with (T1). But, if the construction of such mappings is not uniquely determined by physicalist considerations (i.e., if there are no sets of physical nomological sufficient conditions for such mappings that would serve to identify the unique physicalistically correct mapping), then (T1) is not satisfied by such mappings and, as we shall contend below, (T1) is itself in violation of other physicalist demands (i.e., the demands made by thesis (T3)). So, if (T1) is part of natural science, then either it is false of certain relations in natural science

and itself subject to physicalist indeterminacy or within physicalist science there are physicalist grounds for fixing unique ontological reductions and the truth of (T1). We shall leave off with our discussion of this objection for the time being and move on to a consideration of the other theses of physicalism. In Chapter 5, where we discuss the metatheses of physicalism (and specifically, the claim that the theses are empirical theses of natural science), we shall propose a resolution of the current difficulty for the physicalist doctrine.

4.2 THESES CONCERNING OBJECTIVITY

We now turn to a discussion of the physicalist motivations with regard to objectivity and of the kinds of thesis that will most adequately express those motivations. First, we shall review the conception of objectivity that we shall be working with; second, we shall discuss the demands that shall be placed upon our formulation of the relevant physicalist theses; third, we shall suggest how to formulate theses that will serve the physicalist's purposes; and, finally, we shall consider a variety of objections to the theses.

Although the topic of a fair amount of philosophical discussion, the concept of objectivity is not one that can be easily and precisely tied down; our discussion will be aimed at clarifying some of the central ideas without attempting a rigorous characterization. First, objectivity is ordinarily taken to be a property that attaches to "truth", "knowledge", and "facts"; that is, it is taken to be a fea-

ture of the world as well as of our knowledge of the world. As physicalists, we shall be concerned to capture both employments of the concept, since we are holding to the idea that there is no general equivalence to be had between theses about knowledge and theses about the objects of knowledge.

There appear to be two key ideas relating to the notion of objective truth or knowledge: the first is that of intersubjective testability, upon which great emphasis has been placed. The idea is roughly that objective knowledge must be knowledge for which more than one knower (e.g., the scientific community) can have relevant evidence: shared evidence leads to shared knowledge. The second key idea is that of "knowledge that is independent of the knowing subject"; that is, knowledge that is not dependent upon the variable interests or proclivities of cognizers. The issues here run quite deep, since there may be an important sense in which all knowledge depends upon features of knowing subjects (e.g., upon the systems of representation employed in cognition). Our aim here is not to tackle such issues, but rather to get at an idea central to the physicalist's conception of objectivity; if that idea cannot stand up under scrutiny, so much the worse for the physicalist's conception of objectivity. At a minimum, the notion of independence here alluded to is intended to rule out obvious individual differences upon which certain claims may depend (e.g., specific personal goals in making claims to knowledge).

For example, Quine's thesis of the indeterminacy of translation is a claim concerning the non-objective status of claims about translation from one language to another ("there is no fact of the matter"). Even if such claims are objective in the sense of being supported by evidence available intersubjectively, they are not (according to Quine) objective in the sense of being independent of the knowing subject: different translators with different interests or proclivities will translate differently. Such sensitivity to individual differences is a feature of translation but not of physics according to Quine; the reason for this requires a third idea concerning the notion of objectivity: the idea of there being an objective matter of fact underlying a claim to knowledge.

Translations do not count as objective knowledge because they are sensitive to individual differences between translators: they are not independent of the knowing subject. What distinguishes those claims which are independent of the knowing subject from those which are not? Answer: one concerns matters of fact while the other does not. Hence, physics, being concerned with matters of fact is not sensitive to differences among investigators, while translation, being not so concerned, is so sensitive. The following questions, of course, arise: what counts as a matter of fact? and how can we tell when knowledge concerns matters of fact?

To summarize: insofar as objectivity is a feature of claims to knowledge (or truth), there are two features of interest; the first is the intersubjective testability of the claim, and the second is independence of the claim with respect to intersubjectively variable factors (i.e., the truth value of the claim does not vary with subjective differences). Focusing on just this second feature, it is there being a fact of the matter with which a claim is concerned that underlies its independence from subjective factors. As a consequence, two questions arise concerning objectivity: (1) within nature, what counts as an objective matter of fact? and (2) within our system of knowledge, what are the marks of an objective truth? (i.e., within our system of knowledge, how are the claims with a factual basis to be discerned from those that lack such a basis?).

The appropriate physicalist strategy for answering these questions is to relate certain domains (e.g., non-physical linguistic or non-linguistic entities) to the physicalist bases. In particular, the physicalist theses concerning objectivity will characterize relations which must hold (1) between non-physical phenomena and physical phenomena in order for the former to count as an objective matter of fact and (2) between non-physical truths and physical truths in order for the former to count as objectively true or false. If both of these theses are formulable and defensible, they should constitute an expression of the physicalist view that the physical ontology and doctrine provide bases for scientific objectivity.

We turn first to the formulation of a thesis concerning objective matters of fact. The physicalist view is that the objective matters of fact are the physical facts and facts that are determined by the physical facts: the problem of formulation comes down to making precise the physicalist dictum that the physical facts determine all the facts of nature.

The core idea behind this dictum is that there is a world of fact, independent of our cognitions, which has a certain structure (i.e., it is structured by relations to a physical basis); if a purported fact can be shown not to bear such relations, then that is sufficient grounds for the physicalist to reject it as not an objective matter of fact. Thus, the thesis provides a criterion for objectivity, the most notable application of which is to be found in Quine's writings on the indeterminacy of translation and related indeterminacy claims; we shall look closely at this application of the physicalist thesis concerning objectivity below.²¹

The picture of the world here characterized displays a world of fact, independent of cognition and structured by relations to a physical basis, which is available as an object of study for science and which captures the metaphysical intuitions characteristic of a physicalist outlook. Objective facts are such because, being completely determined by the physical basis, they are not sensitive to the differ-

²¹ See Chapter 5.

ences between knowing subjects (i.e., the objective facts are independent of cognition). As we shall see, it is Quine's contention that translation, mentalistic theories of mind and semantic theories are not concerned with objective facts in this sense. Science, being concerned exclusively with getting at the facts about nature, is, on the physicalist view, concerned with getting at the objective facts as characterized above. Thus, science ought not to be concerned with translation, mentalistic theories of mind and semantic theories, if Quine is correct.

It would be most inappropriate not to take any notice of the issues concerning "facts" and the distinction, upon which the above discussion depends, between fact and non-fact (fiction?). The issues here are quite difficult and quite general, and we shall only try to deflect them. With respect to the question of the use of "fact talk" at all, we endorse the view that such talk is completely dispensable in favor of more contentful locutions. Below we shall describe our strategy for this. As is by now clear, our intensional bias will be unsatisfactory to Quine and his ilk; perhaps there is some gain, even in their eyes, in dispensing with facts in favor of properties.

With respect to the distinction between fact and non-fact, we are sympathetic to the idea that all conception of fact is dependent upon features of the conceivers: the nature of representation and differences between representational systems are quite pertinent to the na-

ture of cognitions and the differences between cognitions of "the world". However, our project is not that of developing a monopolistic system; it is the more restricted one of developing a system which we believe is implicit in much of the actual conduct of science and for which there are reasons to think that it ought to be so implicit. Thus, it is from within the physicalist view of nature and science that the conception of objectivity described above takes the form we have been considering: i.e., it is the physicalist view that there is an independent world of fact structured by relations to a physical basis.²² The question of whether this view as opposed to some other is the best for science is one which we shall argue for an affirmative answer. But none of this requires that we deny any of the observations concerning how a completely untainted notion of fact is not viable. From within a given way of conceiving of knowledge and the world, distinctions between objectivity and subjectivity can be clearly delineated, even if that way of conceiving, like all others, involves parochial features of cognition and systems of representation. And, this is true, even if the distinction between objectivity and subjectivity that is so developed is itself limited in its scope and not applicable to all knowledge. As Goodman might put it, there is no one way of conceiving of the world, and hence no one way that the world is. Some

²² It is compatible with this that, as Putnam and Goodman have been at pains to point out, the physicalist's conception itself may not, and probably is not, "objective" in this sense. Thus, the issue of whether the physicalist's views about nature and scientific knowledge apply to themselves arises again.

ways of conceiving might be better for specific purposes than others, which is what we contend is the case for the physicalist way with respect to scientific purposes. Again, within the physicalist outlook, there is a clear distinction between the world of fact and the nature of cognition; it is this picture which, we claim, best serves science as science is currently conceived and practiced.

So, if the objective facts are those that are determined by the physical facts, how is this to be expressed in a precise way? This question raises three more: viz., how are we to characterize the physical facts?, how are we to characterize the non-physical facts?, and how are we to characterize the relation of determination which the former bears to the latter?.

To characterize the physical facts, we shall need the notion of a possible state of nature as follows: given our specification of the physical ontology,²³ a possible state of nature is a complete distribution of physical attributes over the regions of the spacetime continuum which is compatible with the laws of nature; the class of all such distributions is the class of all possible states of nature. We shall represent this class with a class of models, each member of which is a structure in which the individuals are the spacetime regions and in which a complete distribution of attributes over those

²³ I.e., a spacetime continuum in which sets of points are taken as regions of the continuum, and a set of physical attributes consisting of fundamental attributes and complex and higher order attributes built up from the fundamental ones.

regions is specified. This class represents all the possible physical states of nature and, thus, all the possible distributions of physical fact.²⁴

To cash in the notion of the non-physical facts, our strategy is similar: consider the class of all non-physical attributes and the spacetime continuum as before; then, a complete distribution of these attributes over the regions of the continuum which is compatible with the laws of nature is a possible non-physical state of nature; the class of all such distributions is the class of possible non-physical states of nature. As before, we shall represent these possibilities in terms of a class of models in each member of which the individuals are the regions of spacetime and a possible distribution of non-physical attributes is specified. This class of models represents all of the possible distributions of non-physical fact.

Given these two classes of models and given that the domains of the models in each is the same (i.e., the regions of the spacetime continuum), we can specify a class of models each member of which has the regions as individuals and a complete distribution of both physical and non-physical attributes specified: this class represents all of the possible pairings of physical states of nature with non-physical states of nature. And, given this class of models, we can now charac-

²⁴ The general idea behind our strategy is suggested by the work of Hellman and Thompson (1975), although we have departed considerably.

terize the notion of determination pertinent to the physicalist intuition that the physical facts determine all the facts of nature. In so doing, we shall indicate what subclass of the class just described represents, on the physicalist view, the possible states of nature (i.e., the class of possible distributions of physical and non-physical attributes over the regions of spacetime.)

Informally, the relation of determination is expressed by the idea that once the physical facts are "fixed" so are all of the non-physical facts. That is:

(T2) For any complete specification of the physical facts, there is exactly one correct specification of the non-physical facts.

In terms of the class of models just specified, if A and B are models in that class and if A and B agree in the distribution of the physical attributes over regions of spacetime, then A and B must also agree in the distribution of non-physical attributes over those regions. This requirement picks out a proper sub-class of the class of models which consist of all possible pairings of distributions of physical and non-physical attributes. Specifically, it picks out the class of models which constitute all possible physical states of nature and which associates with each distinct physical state of nature exactly one non-physical state of nature. A more formal characterization of the relation of determination which structures this class of models will not be given here, although there do not appear to be any special dif-

faculties involved in providing one.²⁵

To summarize: on the physicalist view, an objective matter of fact is one which is determined by physical facts; we have represented the possible states of nature which exhaust the possibilities regarding objective matters of fact in terms of a class of models each member of which pairs distributions of physical and non-physical attributes in such a way that the class is structured by a relation of determination (i.e., members A and B cannot agree in the physical facts but differ in the non-physical facts). So, any alleged attribute for which it is contended that under identical physical conditions it might or might not be realized is not an objectively real attribute of things. Such an attribute will not be among those included in the class of non-physical attributes employed in the model-theoretic construction described above; hence, it will not be included among the attributes studied in science.²⁶

²⁵ Whether there is only one proper sub-class which satisfies the requirement of determination or more is not important at this point. The current requirement is just that the structure of the natural order as conceived by physicalists is partly captured in terms of the relation of determination. Below we shall discuss explanatory considerations which may lead to isolation of a single class and we shall discuss further the issues involved in isolating a unique class of models for science. Now, we are just concerned with what the requirements are for capturing the notion of objectivity.

²⁶ This, of course, is exactly the Quinean position regarding the attributes studied in semantics, mentalistic psychology, and linguistics.

We note quickly that the above described characterization of determination of the non-physical facts by the physical facts fits well with our discussion of the ontological thesis, (T1). There the claim was that for every (possible or actual) instantiation of a non-physical attribute there exists an instantiation of a physical attribute which is such that (i) the occurrence of that physical attribute is nomologically sufficient for the occurrence of the non-physical attribute and (ii) the two attributes co-occur in the same regions of spacetime. (T2) is a logical consequence of (T1) although the two theses provide different kinds of insight into physicalist thought. Further, (T2) entails the supervenience of the non-physical upon the physical as well as its logical equivalent, the so-called "indiscernibility of physical identicals" (i.e., no difference without a physical difference").²⁷

The second thesis regarding objectivity concerns the formal system of science, rather than the object of study in science. The question to be addressed is: what are the marks, within the formal system of science, of objective knowledge? That is, which are the statements which have a basis in fact for their truth value, statements which are either objectively true or false because of the way the world is, not

²⁷ The recent discussions on supervenience has revealed a number of ambiguities and differences in formulation of a family of related claims; the theses (T1) and (T2) represent the strongest claims from among that family. See Kim (1978, 1981, 1982a), Haugland (1982), Horgan (1981), Hellman and Thompson (1975), Friedman (1975), and Healy (1978).

because of the way that cognizers are?

Above we characterized a class of models which represent the range of possible objective facts of nature; this class is to be viewed as the class of intended models for the formal system of science, and hence, it provides an interpretation for the language of that system. Given this interpretation and given the determination of fact characteristic of this class of models, we are concerned with the question of what structure is "induced" into the formal system as a consequence? Such structure may be sufficient to function as a criterion of objective truth, and hence, provide a measure of which claims to truth value are claims about which there is an objective matter of fact (i.e., claims which are objectively true or false).²⁸

In recent years the doctrine that the physical truth determines all the truth about nature has been favored by many physicalists.²⁹ Further, it has generally been agreed that classical physicalist reductionism is too strong a thesis and that, however the thesis of truth determination is to be cashed in, it must be compatible with the falsehood of classical reductionism. To my knowledge, Quine has never offered a precise rendering of truth determination, although it is clear

²⁸ One of the virtues of classical physicalist reductionism was that it provided such a criterion: derivability from the physical truths was considered sufficient for being an objective truth, derivability of the negation of a sentence sufficient for being an objective falsehood.

²⁹ For example, Quine (1969b), Friedman (1975), Hellman and Thompson (1975), Haugland (1982), and Horgan (1981).

that he is not in any way committed to the strong reductionist thesis. However, as we have seen in Chapter 2, both Hellman and Thompson and Friedman have suggested precise formulations of truth determination that are weaker than classical derivational reduction. For the purposes of the current project, we shall offer only an informal characterization of truth determination in science; the reader is referred to the physicalist literature for more technical developments.

We shall assume that a given class of languages, L , for science is fixed and that the class of models, M , described above provides an interpretation for those languages; the thesis of truth determination in science is as follows:

(T3) Within the class of models, M , if members m_1 and m_2 of that class agree with respect to the physical truths, then they agree with respect to the truths formulable in all other languages in L .

This thesis is very similar in formulation to that offered by Hellman and Thompson; the crucial difference is that they give a technical rendering of this claim in terms of the notion of elementarily equivalent models, whereas we have not limited ourselves in that way. The reason for this is that it is not clear at the present time whether such a restriction is acceptable, given our full blown acceptance of attributes and given the modal force of the claim intended to be expressed.

How does (T3) serve the motivations of the program regarding objectivity? A sentence, s , formulable in some language L_i , has a claim to objective status only if its truth value is fixed once the physical truths are fixed. Thus, for s to be objective, it cannot be possible that, given a specification of the physical truths, either s or its denial could be true (i.e., there could not be models in M which, though agreeing on all the physical truths, do not agree on the truth value assigned to s). As a criterion of objectivity, (T3) purports to capture the idea that it is the mind independent physical facts about the world that determines the truth value of s rather than subjective features of cognizing persons in the world. Thus, a clear distinction between objective truths and judgements sensitive to idiosyncratic subjective features can be made.

The paradigmatic employment of this principle as a criterion of objectivity is found in Quine's attacks on the scientific status of translation, reference and mentalistic psychology where he attempts to marshal arguments designed to show that relative to the physical truths, "truths" formulable in the language of translation theory, mentalistic psychology and referential semantics are not truth determined by the physical basis for doctrine. Rather, they are sensitive to variable and arbitrary features of the knowing subjects. We shall return to a discussion of the issues here in Chapter 5 when we consider the methodological role of physicalist theses in science.

In passing, we note a second thesis concerning objectivity which Hellman and Thompson have proposed and which we see as an important component in a total development of physicalist doctrine. This second thesis is that of the determination of reference:

(T4) For the class of languages L, and for the class of models M, if members m1 and m2 of M agree with respect to the referents of terms in the physical language, then they agree with respect to the referents of terms in all other languages of L.

This thesis captures informally the idea that all the terms in the language of science are implicitly defined by the physical bases.³⁰ As a criterion of objectivity, (T4) requires that terms for which an extension is not determined by the fixing of extensions for all the physical terms will not be accorded objective status in science, since what they are true of is not determined by the mind independent physical facts.

In a comparison of the present version of physicalism with classical reductionism, it emerges that, with respect to adequately capturing the physicalist views about objectivity, both succeed. That is, both implicit and explicit definability capture the idea of objective determination of reference and both truth determination and derivability capture the idea of objective determination of truth. However, for these purposes both explicit definability and derivability are more

³⁰ See Hellman and Thompson (1975), p. 559 for discussion.

than is required; the weaker theses suffice for concerns about objectivity. Given the severe scepticism regarding the stronger theses in recent years, this is good news for physicalists. However, as we have repeatedly emphasized, the physicalist program has more far reaching motivations than just concerns with objectivity; concerns about explanation are quite central to the program. As we pointed out in Chapter 2, such concerns are not adequately addressed by the determination theses and must be handled in some other way. In the final section of this chapter, we shall turn to theses concerning explanation to explore how the explanatory motives of the program might be adequately expressed without flying in the face of known difficulties regarding classical reductionism. Before turning to this task, however, we shall first discuss some objections to the theses regarding objectivity.

There are six objections to the theses just developed that we shall consider:

(1) The notion of objectivity upon which the theses are based is a defective one: there is no realm of pure fact that can be separated out from the contributions of the knowing subject. Hence, there are no facts (or truths) that can stand independent of subjective factors and determined by the physical facts (truths) exclusively.

REPLY: The claim of the objection that all knowledge involves contributions from knowing subjects is correct. However, our theses are designed to capture a particular way of viewing nature and science; within this view, nature and science exhibit the sharp distinction be-

tween objective and subjective factors described above, despite the fact that all cognition involves a contribution from the cognizer. None of this means that the conception of objectivity that we have employed is defective. Rather, the theses (T2)-(T4) are designed to capture a certain view of scientific objectivity, the physicalist's view of objectivity in science. That view is not intended to be a general view about all knowledge; instead, it is a stance taken from within the practice of science as physicalistically conceived. Within the institution of science, there are a number of shared features (e.g., systems of representation, general canons of method) which are features relative to which science is practiced at a given time and which are not themselves objective in the sense of being mind independent. However, given these shared features,³¹ a view of nature and science can involve a sharp distinction between the subjective and the objective: physicalism, as we see it, is a doctrine which codifies such a view of nature and science. It is part of the work of science on this view to sort out the subjective "facts" (and claims) from the objective facts (and claims). Thus, a theory of science and nature from this perspective would substantiate the view of objectivity we are considering from "the inside". None of these considerations implies that this approach is "monopolistic" in the sense of being the only approach to understanding nature or knowledge; none of this implies

³¹ These features may constitute a framework that is objective in the sense of being shared by a community of cognizers or objective in some other sense. See Putnam (1981) and Goodman (1978) for suggestive discussion.

the rejection of the insight that all knowledge involves a contribution from the knowing subject or that distinctions of the kind we are talking about can only be made from within a system of concepts and objects. We add to this highly conciliatory reply, however, that relative to certain cognitive goals the approach we are discussing may constitute the best way of viewing nature and science. We think that it is and that, at least, science as it is currently practiced also embodies the assumption that it is.

(2) The theses of fact, truth and reference determination are not sufficient for all scientific or cognitive purposes; hence, physicalism (with respect to objectivity) is not a complete doctrine.

REPLY: This is quite obvious enough; it has been our view from the beginning that it is a mistake to view ontological or determinationist physicalism as adequate for all of the motives of even the physicalist program itself, let alone for all cognitive purposes. Specifically, such restricted forms of physicalism are clearly not adequate for explanatory concerns as we argued in Chapter 2. However, the critics³² have been rather imprecise with their criticisms. The issue is not whether physicalism is a complete doctrine - that isn't a very controversial issue; the issue is whether it is adequate for a certain set of cognitive purposes, and whether it is superior (for serving those

³² We have in mind here Putnam and Goodman. Neither has ever offered any serious alternative to physicalism with respect to natural science. This may indicate that they do endorse physicalism as a parochial doctrine; hence, there may be substantial areas of agreement between us.

purposes) to other programs. It is the main purpose of this project to develop a version of physicalism that is adequate for a certain set of purposes; relative to such purposes, there is reason to believe that physicalism is superior to other proposed programs. So, if the critics hold that physicalism purports to be a complete doctrine in some highly monopolistic sense, then they are attacking a different program from the one here defended. If they are claiming that physicalism is not adequate to its own goals, much more argumentation is required than has been offered so far, although it is granted that the theses regarding ontology and objectivity are not sufficient for meeting those goals.

(3) The relation of determination is of no value in science since it cannot be studied except in the presence of the stronger relation of derivational reduction; there are no known cases of determination without reduction.³³

REPLY: First, at this point in our inquiry, the issue of utility is of secondary importance to that of formulation; there is no reason to believe that there are not distinct relations of determination and derivation, that one is weaker than the other and that both adequately express the relevant physicalist view regarding objectivity in science. Hence, with respect to the adequacy of the formulation, the objection is not relevant. Second, it seems to be just false that determination cannot be studied independently of derivation; of special interest in

³³ This objection was suggested by Hellman in conversation.

the literature on the indeterminacy of translation have been alleged violations of determination, such violations being identifiable as such without any consideration based upon derivational reduction.³⁴ Third, however, the issue of whether there are actual cases of determination without reduction hinges upon the thorny issues concerning the expressive power of the language employed; in particular, if the language is an infinitary language of sufficiently large cardinality, then the formal system of science may inevitably involve derivational reductions as well as determination.

(4) The theses of determination are trivially true and uninteresting: since there are always physical differences corresponding to any actual non-physical difference and, hence, there will always be covariation of physical with non-physical truth as well.

REPLY: The objection misses the point of the theses since, although the contingent facts may be as described, the theses are concerned with counterfactuals as well: the model theoretic constructions exhibit determination relations which have nomological force. Further, the existence of accidental and "irrelevant" differences with respect to specific attributes misses the real point of physicalist doctrine generally, which is that the world as conceived by the physicalist is the way it is because of the way the physical attributes are distributed. This view ultimately points the way to discrimination of relevant³⁵

³⁴ The violations are established by constructing counterexamples to the determination claim.

from irrelevant physical attributes with respect to the instantiation of given non-physical attributes. Hence, the mere existence of co-occurring physical differences is not of interest unless they have nomological force and they are relevant. The objection, insofar as it depends upon too weak a construal of the theses, is without force.

(5) Appeal to the physical ontology and doctrine as bases for objective fact and truth is of doubtful value since those bases are indeterminate. There is no objective fact of the matter as to what the bases consist in; hence, they cannot serve as bases for objectivity within science.

REPLY: This objection depends upon the success of the objections considered earlier³⁵ regarding the indeterminacy of the bases; insofar as we have responded effectively there, the current objection carries no additional weight. The position we are taking is that determination is a physicalist requirement within science relative to a selection of physical bases. The selection of bases may exhibit a component of conventional choice which, relative to the purposes at hand, may be arbitrary (i.e., all acceptable candidates may serve equally well). However, this does not signal any malignant indeterminacy; rather, it shows that there may be alternative ways of developing physics and science which are equivalent relative to all purposes of scientific

³⁵ The notion of relevance here leads into the explanatory motives of the program to be discussed below.

³⁶ See Chapter 3.

interest. Whichever way is chosen regarding the bases, the physicalist's approach regarding objectivity requires that the theses (T2)-(T4) must be satisfied. These theses impose constraints upon what scientific objectivity consists in, given a choice of bases: the existence of equally acceptable alternative bases does not undermine this requirement on the structure of science and nature.

(6) The relations of determination appealed to in the theses, being semantical in nature, are themselves indeterminate relative to the physical bases: there are no physical facts (truths) which fix the facts (truths) about determination. Hence, by physicalist standards, the theses are not objectively true or false and the relations described by the theses are not objectively real.

REPLY: We defer discussion of this objection until the next chapter where we discuss the use of physicalist principles for methodological purposes. There we shall consider whether the physicalist can be hoisted on his own pitard. The objection here parallels the indeterminacy objection raised earlier with regard to (T1); the issues are whether ontological determination relations in nature present special problems for physicalism and whether physicalist theses satisfy their own demands.

4.3 THESES CONCERNING EXPLANATION

In the first section of this chapter, we saw that the ontological thesis required that there be a mapping between physical and non-physical attributes which was nomological in force; this suggests

that explanatory relations between physical and non-physical attributes may be possible. In Chapter 1, the goals of the physicalist program were seen to include a number of independent explanatory goals which were related to increasing the explanatory power of the system of scientific knowledge and, thus, the understanding that it yields. Such explanatory power is gained through increasingly comprehensive explanation of individual and general phenomena and through increased unification and simplification of the whole scientific explanatory system. In this section, we shall be developing a set of physicalist theses which serve to express the physicalist's motivations concerning explanation and which will clarify the nature of the "vertical" explanation suggested by (T1).

The physicalist program, as we have been construing it, focuses on certain aspects of the system of scientific knowledge: in particular, its "vertical" aspects. As we saw in Chapter 1, there are at least two conceptions of unity of science which have been endorsed in the physicalist literature: (1) the positivist conception of "unitary science" the goal of which is the incorporation of all science within physics, and (2) a view of science as partitioned into distinct branches which are related to one another in various ways and such that all branches bear specific relations to physics. Our development of the physicalist program falls clearly within this second camp; and the theses developed in this chapter are designed to capture some of the relevant relations that all branches bear to physics without denying the distinctness of the branches from one another.

In what follows, we shall focus upon theses regarding explanation. Our approach is informed by the view that, given the existence of distinct branches of science, there are "inter-field"³⁷ questions which naturally arise; and, some, but not all of these questions, arise from a physicalist outlook on the structure of nature and science. Physicalism, on our view, exerts a structuring influence upon science by raising such questions.

To summarize: physicalism, as we conceive of it, expresses the view that science is partitioned into distinct branches which are connected to the basic science and to each other by a variety of relations concerning ontology, objectivity and explanation. The explanatory connections between branches constitute a component of "inter-field" inquiry; and, it is the nature of these connections and, hence, of this type of inquiry that we shall be exploring below.³⁸

Before moving on to a discussion of the theses, we shall briefly note the assumptions and constraints that we shall be working within. First, as we have discussed in Chapter 2, the expression of explanatory connections between branches must be made by reductive theses; and,

³⁷ See Maull and Darden (1977) for discussion of the notion of an inter-field theory; the points we will be making in the text differ substantially from their discussion of such theories.

³⁸ Our primary focus, of course, is on relations that each branch of science bears to the one basic science, physics. However, such relations may be mediated by relations that a given branch bears to another non-basic science (e.g., relations between psychology and physics may be mediated by relations between psychology and the neurosciences and between the neurosciences and physics.)

they must not be purely non-linguistic in character. Second, as discussed in Chapter 3, we presuppose that there are principled divisions between branches of science, such divisions being based upon the attributes studied and the questions typically asked within branches. Third, we assume that, contra a dominant tradition in the philosophy of science, science is concerned with asking questions other than "why questions"; in particular, questions asking in virtue of what physical facts a given attribute or regularity occurs will be seen to be of interest to the physicalist and not readily assimilated as a species of why question. Finally, our discussion in Chapter 2 revealed that classical derivational reduction is both too strong and too weak as an expression of physicalist explanatory concerns. It is too strong in that it requires, what is at best highly problematic, that there be explicit definitions in physical terms of all scientific terms. It is too weak in that it depends upon a flaw in the D-N model of explanation: i.e., it assumes that derivation entails explanation. Thus, our development of the explanatory theses must be strong enough to capture the explanatory connections between branches without requiring implausible physical definitions and without assuming a flawed picture of the nature of explanation. These constraints point us to the rather vexing problems of what the nature of explanation is and how we can identify explanations, problems that we shall finesse as much as possible.

The physicalist's basic conviction is that all phenomena occur in nature in virtue of physical phenomena. The notion of fact determina-

tion expressed by (T2) serves to capture this idea from an ontological point of view; the question remains of how this idea is captured within the formal system of science. The theses about explanation should provide an answer to this question by expressing the idea that all phenomena are "explainable" in terms of physical phenomena. Despite the apparent grandiosity of such theses, the task of formulating and employing them is justified by the potential gains that we have discussed in Chapter 1. In what follows, we shall make precise the following idea: given that all individual phenomena, all regularities, and all instances and exceptions to regularities that occur in nature occur in virtue of physical phenomena, the physical doctrine provides a basis for the explanation of all such phenomena.

By individual phenomena we shall be considering the instantiation of attributes in regions of spacetime; instances of other ontological categories (e.g., events, states) will not be considered. Further, we shall be relying on a distinction between the "horizontal" and the "vertical" study of the occurrence of individual phenomena. By "the horizontal study of some phenomenon" will be meant the study of its causal and, possibly, other relations to pre-existing, co-occurring or subsequent phenomena that fall within the same "ontological level". By "the vertical study of some phenomenon" will be meant the study of its relations to phenomena that fall within different "ontological levels". The specific ontological levels that exist are a matter of scientific discovery, although, as science is conceived by the physi-

calist, the physical level is identified along the lines discussed above in Chapter 3 and is conceived of as the basic ontological level for all of natural science. It is the vertical study of phenomena that is of special interest to the physicalist, because science, on the physicalist view, is structured vertically by a set of relations which relate the ontology and doctrine of all branches of science to the physical bases.

With respect to the explanation of all individual phenomena, the physicalist concern is with the vertical explanation of all phenomena in physical terms. Recall that the ontological thesis called for a physical attribute associated with each instantiated non-physical attribute such that (i) the former is nomologically sufficient for the latter and (ii) the attributes occur in the same regions of spacetime. Such a view suggests the potential for explanatory relations between the attributes involved; and, it is to the nature of such explanations that we now turn.

Vertical explanations may be seen as answers to questions of the following form:

1. In virtue of what physical attributes did the instantiation of such and such non-physical attribute occur?
2. In virtue of what did such and such physical attributes determine the instantiation of such and such non-physical attribute?

A full answer to such questions consists in citing of the pertinent physical and non-physical attributes and in explaining the instantiation of the non-physical attribute in terms of the physical attribute.

To characterize the kind of explanation called for here, we shall introduce the notion of a "realization theory for an attribute".³⁹ Such an idea is implicit in much work in the sciences and in a variety of discussions in philosophy; the idea is that of an account of the occurrence of a given attribute at one ontological level by showing how the occurrence of attributes at a lower level are sufficient for the occurrence of the higher level attribute. Examples of such theories include: the physical account of chemical valence, the microbiological account of genes and the transmission of traits across generations of species, functionalist accounts of mental states, theories of the neurophysiological attributes underlying psychological attributes, the physical account of phenomenological properties such as transparency, temperature and pressure. In all such cases, appeal is made to a theory of what physical attributes must be like if they are to provide realizations of the given higher level attribute. Thus, functionalist accounts of mental states would delineate the pattern of causal relations which some physical system must exhibit if a given mental state is realized by that system. The realization theory in this case consists in specific characterizations of the pattern of causal relations required without making any appeal to specific physical relations that

³⁹ We shall use the abbreviation 'RT' for this kind of theory.

might exhibit such a pattern. Then, for a given instantiation of the mental state in question, the vertical explanation would consist in (i) citing a set of specific physical relations and (ii) by appeal to the functionalist RT, showing how those physical relations suffice for the realization of the mental state.

To summarize: a RT provides an account of what it is in virtue of which physical attributes realize non-physical attributes. The provision of RTs and the explanation of the realization of attributes in terms of them is one of the goals of vertical science. The attainment of such goals would also further the more general goals of the physicalist program in science by reducing the number of mysterious phenomena in nature and by unifying science as a whole via vertical explanatory connections.

Given this brief characterization of what an RT is the physicalist explanatory thesis for individual phenomena is as follows:

(T5) All instantiations of non-physical attributes are explainable in terms of the physical bases and a realization theory for the attribute instantiated.

The objective is, of course, to have an RT for every attribute instantiated in nature. However, given the vastness of the number of attributes and given the probable "wierdness" of most of them, we are prepared to retreat to the thesis that it is the instantiation of scientifically important attributes that must be explained, where a

scientifically important attribute is one which plays a role in scientific explanations and theorizing. This may be viewed as requiring that any attribute introduced in science for explanatory purposes must be accompanied by an RT such that every instantiation of that attribute is explainable as required by the thesis. Whether or not we must retreat to this position is not entirely clear to me, given that the issue rests on such issues as how to individuate attributes and what the range of possible RTs is. At the minimum, however, the thesis does serve the physicalist goals in vertical science in part, by requiring the study of the realization of attributes appealed to in one branch of science relative to the attributes in the physical basis.

It is important to see that RTs are not just explicit definitions in physical terms as earlier versions of the program had it; even if physicalist definitions were forthcoming,⁴⁰ there would still be a need for RTs if the goals of explanation are to be served. This assumes, what we take to be obvious, that attributes are not to be individuated by the criterion of nomological coextensiveness; if this were the individuation criterion, then definability would lead directly to identification of the attributes expressed by the defined and the defining terms. It is a major shortcoming of both classical and recent versions of physicalism that the role of explicit definitions has not been very well understood in terms of the goals of the program.

⁴⁰ And, as we have seen in Chapter 2 this is not entirely out of the question.

Causey, for example,⁴¹ is at great pains to distinguish those nomological correlations which express attribute identities and those which express "causal relations": the criterion is the existence or lack thereof of an explanatory account of a causal relation between the attributes in question. On our view, Causey falls way short of the mark because his dichotomy is not exhaustive: he leaves out the possibility of realization of one attribute by another and he takes much too seriously the idea of attribute identity in science (it is a rarity at best). He has the right idea in seeking explanatory accounts. But, he encourages a false picture of the inter-domain problem of how attributes in one domain relate to those in others, at the expense of appreciating the need for inter-domain explanations in general: the goals of the program are sacrificed by overemphasis on identity.

Further, as we saw in Chapter 2, Friedman's view that each non-physical predicate is associated with a (possibly infinite) class of physical predicates, although sufficient for his purpose of describing the relation of "weak reduction" in science, raises, but does not indicate how to answer, the question "In virtue of what is each member of the class of physical predicates a member of that class?" Simply to say that each expresses an attribute which is a realization of the attribute expressed by the non-physical predicate begs the explanatory question, since, at the heart of that question is the quest for an understanding of why such and such a physical attribute realizes a cer-

⁴¹ See Causey (1977).

tain non-physical attribute while other physical attributes do not. To only respond that the former does and the latter do not begs the question and leaves science with a large host of mysteries concerning realization: the point of (T5) is to rid science of such mysteries.

The general point is that it does not matter for the achievement of the explanatory goals of the program whether each non-physical attribute is nomologically correlated with a single physical attribute or whether, as Friedman and our own (T1) allow, each non-physical attribute is associated with an exhaustive class of nomological sufficient conditions in the physical basis. Either way, the explanation of the realization of non-physical attributes by physical attributes requires something more if a large class of mysteries are to be eliminated from our scientific representation of nature. The role of realization theories in science is the elimination of these mysteries. And thus, to achieve the explanatory motivations underlying the physicalist program, (T5) requires explanations, via RTs, of the realization of all non-physical attributes by associated physical attributes.

We shall now turn to a discussion of theses concerning physicalist explanations of regularities in nature. The original versions of the physicalist program were concerned with what was called "the unity of laws"⁴² in science. The idea was, simply, that all laws of nature were to be derivable from the laws of physics: such derivation was thought

⁴² See Carnap (1969).

to be sufficient for the explanation of those laws and the regularities they expressed. Although we fully support the motivations behind this version of the program, we are skeptical about the specific thesis proposed. That is, we fully endorse the idea that the physicalist program is motivated by a quest for increasing the explanatory power of the system of scientific knowledge by, inter alia, vertical explanation of regularities, but we reject the classical thesis that expressed this motive. Our task now is to formulate an alternative.

The physicalist's conviction is that all regularities, their instances and their exceptions are determined by underlying physical phenomena; this conviction was captured from an ontological point of view earlier by the thesis concerning the determination of fact, (T2). Our question now is: How is this conviction to be expressed from the point of view of the formal system of science?

Consonant with our introduction of the idea of vertical science above, our approach here will center upon the following question: given a set of non-physical regularities concerning some class of attributes, what are the underlying physical attributes and regularities which determine them? It is this and closely related questions that are the object of inquiry within inter-field disciplines. In short, vertical science is concerned with the lower-level mechanisms underlying higher-level regularities. As we saw above in discussing individ-

ual phenomena, the questions of vertical science make no presumption that higher-level attributes are realized uniquely by lower-level attributes. Similarly now, no presumption is made that exactly one lower-level "mechanism" underlies a given higher-level regularity: there may be multiple physical mechanisms which underlie the various instances of a given regularity.

By a regularity, we shall mean a lawlike⁴³ relation between the instantiation of attributes; such relations may be causal or not. By a mechanism for a regularity, we shall mean some set of physical attributes and physical regularities which hold among members of the set; again such regularities may or may not be causal. The current point is that, along with the idea that non-physical attributes may be multiply realized is the idea that relations between such attributes may also be multiply realized.⁴⁴

⁴³ We shall not address the issues raised by the idea of "degrees of lawlikeness" although their development could have an impact on how the physicalist understands RTs and the nature of vertical explanation. See Fodor (1975) and Davidson (1970) for preliminary exploration of these issues.

⁴⁴ We caution the reader about the complexities concerning these points: depending upon how the issues concerning definition (cf., Chapter 2) are resolved, there may be "type-type" correlations between attributes, where the physical type is either disjunctive or of higher order. Thus, with respect to regularities there may be single physical mechanisms realizing higher-level regularities, where such mechanisms are "higher order" or disjunctive physical mechanisms.

Given these preliminaries, let us turn to the problem of formulating the physicalist thesis concerning the explanation of regularities. Roughly, the view is that every possible instance and every possible exception⁴⁵ to the regularities formulated by the higher-level sciences are explainable in terms of underlying physical phenomena. Such explanations consist in identifying the non-physical attributes involved in the regularity to be explained, identifying the physical attributes that realize those non-physical attributes (on the specific occasion to be explained) and providing an account of the relations between the physical attributes in question which determine the relation between the non-physical attributes that the regularity expresses.⁴⁶

For instances of a non-physical regularity, if a RT for each attribute involved is known, and if the physical attributes that realize the relevant non-physical attributes can be identified, and if the physical relations between those attributes are known, then an instance of a regularity is explained if the relation between the non-physical attributes is explained in terms of the physical relations between the physical attributes that realize the non-physical attri-

⁴⁵ See Fodor (1975) for discussion of the idea that the regularities in the higher-level sciences typically have exceptions.

⁴⁶ Since the physical attributes may vary from occasion to occasion, the issue of explaining the regularity itself, as opposed to each possible instance, turns heavily upon the issues concerning definability. Since those issues are quite unclear, our formulation of the physicalist theses focuses upon explanation of instances only. We shall suggest that nothing more need be done to accomplish the principal physicalist motivations regarding explanation.

butes. That is, if non-physical attribute A is a cause of non-physical attribute B, then an instance of this regularity is explained by citing the physical attributes C and D (which realize A and B respectively) and by explaining (in physical terms) that C causes D. The physicalist thesis here is that every possible instance of a non-physical regularity be explainable in this way, although it is not assumed that all relations are causal.

The idea that regularities of the higher-level sciences typically have "exceptions" has been emphasized by Fodor; the significance of this idea for the concept of a law of nature is not entirely clear, but for present purposes the issue can be by-passed. We shall assume that Fodor is right in his claim and show how it can be fit into a physicalist framework. Specifically, the physicalist requires only that such exceptions be explainable along the same lines as the instances. That is, if an exception to a regularity is a case in which the antecedent but not the consequent attribute is instantiated (some other attribute being instantiated instead), then such an exception is explained if a physical explanation of the relation between the attribute realizing the antecedent non-physical attribute and the attribute realizing the non-physical attribute which replaced the expected attribute exists and explains the relation between the two occurring non-physical attributes. The physicalist view here is that every possible exception to a non-physical regularity has a physical explanation.

The physicalist thesis regarding the explanation of regularities is as follows:

(T6) All instances and all exceptions to the regularities formulated in the non-physical sciences are explainable in terms of physical phenomena.

This thesis does not yield as a consequence the derivability of the non-physical laws from the physical laws; hence, it does not appear to capture the physicalist idea that the non-physical laws are explainable in terms of the physical laws as the classical views supposed. However, it does not appear that such explanation of laws is required to capture the idea that everything which happens in nature is explainable in physical terms; (T5) and (T6) do not leave any room for something to take place without physical explanation. What goes unexplained are the regularities themselves (assuming definitions of the right sort are not forthcoming), but this is not a major loss for the physicalist as long as everything that could take place in nature admits of a physical explanation. Perhaps it should have been expected that if not all attributes are physical attributes and if non-physical attributes can be multiply realized by non-physical attributes, then the regularities involving non-physical attributes would not in general be explainable in terms of physical regularities. For now, we reject the classical derivational thesis as well as the thesis that all non-physical regularities are explainable in terms of physical laws. In their stead we offer (T6) which, it should be pointed out, does not

in the least preclude the possibility of finding uniqueness of underlying mechanism for some non-physical regularities or in finding physical explanations of non-physical laws: the point is that these are not requirements of the program. Our contention is that the explanatory goals of the physicalist program are served fully by (T5) and (T6).

We now turn to a consideration of two objections to the theses concerning explanation; first, we shall discuss a claim that it is subject to indeterminacy and, second, we shall consider a claim that it is "utopian" in character. As we shall see, both objections focus upon problems that arise from the heavy employment of the notion of a realization theory by the explanatory thesis.

The indeterminacy objection rests upon the claim that (T5) is defective since it requires explanatory relations which are not objectively real by physicalist standards. The argument for this claim is as follows: the thesis supposes that for each attribute there exists an RT which plays a central role in explaining the specific instantiations of that attribute. However, there are no objective facts of the matter (based upon physicalist considerations) concerning RTs, and thus, the alleged explanatory relations between attributes are not objectively real.

In considering this objection, we shall assume that we have on hand a well established physics and well established higher-level theories; at issue is the existence of RTs that are supposed to provide explan-

tory accounts of the realization of the higher-level attributes. Specifically, is it the case that for each attribute there exists a unique RT which provides "a true account of what it is in virtue of which the attribute gets realized"?

The idea behind the objection is that, if there are alternative and equally acceptable RTs for a given attribute and if there are no objective, physical grounds for choosing among them, then there are no objective facts concerning RTs or the "explanations" they provide.

Fuel for the objector's fire may be found in the Aufbau, where Carnap distinguishes between (i) relation extensions and (ii) the essence of a relation. The former would correspond to the mapping between physical and non-physical attributes as described by (T1); the latter would correspond to our RTs and the explanation of the specific pairings given by the mappings. Carnap held that accounts of the essence of a relation were a species of metaphysical speculation since they were not to be counted as objective or empirical; thus they are not to be counted as scientific. Analogously, it might be argued that RTs are "metaphysical" accounts of mappings between attributes because they are not empirically testable or based upon objective, physical facts.⁴⁷

⁴⁷ For example, functionalist accounts of mental states could be claimed to be non-objective accounts of the essence of the mental, there being no fact of the matter as to whether functionalism or any of the opponent positions (e.g., physicalism, behaviorism, Cartesian dualism) are the true account of the nature of mind. At issue, of course, is how to argue for this claim or for the claim

According to the objection, it is entirely possible to introduce RTs into the structure of science, but which RTs are introduced is a matter that is not objective. The grounds for this claim are (i) there are no empirical, methodological or theoretical considerations which would decide between otherwise equally acceptable alternative RTs and (ii) there are no physical considerations which determine which of competing RTs are true and which are false. Hence, by the physicalist's own standard of objectivity, the choice between competing RTs does not involve an objective matter of fact. In a nutshell, the objector poses the following question: what are the empirical and factual grounds upon which the choice between competing RTs rest? The objector's answer to this question is that there are none. It is to be concluded that the introduction of RTs into science is not an objective, hence not a scientific, matter. Thus, the key notion upon which the theses concerning explanation rests (viz., that of an RT) collapses as do the theses themselves.

In reply to this line of objection, our strategy is to argue that RTs are in no worse shape on the grounds suggested than any other kind of explanatory theory in science and that what the objection shows is that we must be careful in our understanding of how physicalism works in providing structure to science. With regard to the first point, we claim that with regard to testability, there is no reason to believe

that there is an objective fact of the matter about the essence of mind.

that RTs are not subject to empirical test in the same way and to the same extent as other theories. Compatibility with evidence, relations to existing bodies of theory, and being subject to methodological principles all apply to RTs in the same ways and to the same extent as they do to other theories. "To the same extent" means up to the empirical underdetermination of theory by evidence. It is not implausible that, relative to all empirical considerations, there may well be for each attribute in science an equivalence class of theories that could play the role of an RT for that attribute. But this, by itself, is not sufficient to argue for the non-objectivity of RTs in general; among other things, the point doesn't distinguish RTs from any other scientific theory. As we argued above in connection with identifying the physical basis, we are faced in the situation imagined with an embarrassment of riches (i.e., too many equally acceptable theories); and, a choice must be made on "some" grounds for one of the alternatives. Given such a choice, the resulting system of knowledge will be structured in accordance with (T5).

This reply, however, invites the following questions: What is the status of the RTs within the physicalist system?, Do they formulate claims that are subject to physicalist principles? For example, is the truth of the RTs determined by the physical basis as required by (T3)? And, are instances of the claim formulated by a given RT explainable in terms of the physical basis as required by (T5)?

If the claim of the general non-uniqueness of RTs relative to all empirical, theoretical and methodological considerations⁴⁸ is true, then it would appear that the truth of an RT (e.g., that mental state M is realized by functional state F) is not determined by the physical basis nor are instances of it it explainable in terms of that basis.⁴⁹ Therefore, even if a given RT were to play the explanatory role required by (T5), it would appear to fall outside the scope of the physicalist doctrine, and hence, either physicalism is false or RTs are unacceptable parts of science. In such a case, physicalism is unacceptable by its own standards (i.e., it postulates a physicalistically unacceptable relation).

To this argument, the physicalist has only one reply insofar as he intends to maintain that RTs are a part of natural science and he accepts the general non-uniqueness of RTs. That is, the physicalist must allow that RTs are "basic" within the system in that the choice of RTs from among the many acceptable ones is made on non-physicalist grounds and that, once made, it is a feature of the system relative to which determination and explanation of the non-physical levels of the system are to be understood: in this respect, RTs are on a par with the bases.⁵⁰

⁴⁸ Including physical considerations; thus, there are no physical facts which single out one RT from among the many equivalent ones.

⁴⁹ Thus, for example, there is no RT for the realization relation itself that would help locate the physical relation that realizes the realization relation.

We point out that this reply is not simply an ad hoc maneuver designed to save the doctrine of physicalism. The reason that this is so is that there is little reason to believe that RTs differ substantially from other scientific theories with respect to determination by physical fact; and, this is the point at which care must be taken in how to understand the role of physicalist doctrine in science. The situation is, roughly, as follows: relative to a given formulation of physics, there are plausibly alternative formulations of the rest of science: this is clearly true with respect to the choice of theoretical vocabulary for the special sciences, and it is conceivably true for theoretical formulations given a choice of vocabulary. Such alternatives may also be determined by differences in the RTs employed. Now, the choice between such alternatives may be underdetermined by all empirical and methodological considerations including compatibility with physicalism (i.e., each formulation must be structured by the principles of physicalism). The differences between such formulations comes down to choices between explanatory structures employed in science; and, these choices are not determined, nor are they supposed to be determined by the physicalist, by physicalist considerations. The choice of which explanatory structures to employ in science is not objective in the physicalist sense, this is true of all such structures, not just those introduced by RTs. However, once these choices have

⁵⁰ This does not mean that RTs or the bases are not revisable; it means that the grounds for revision must be other than strictly physicalistic ones.

been made, physicalist principles then structure the formulation of science that employs them. Thus, physicalism does not, as it should not, fly in the face of the obvious component of conventional choice that exists in science. Thus, it is important to understand physicalism as not a doctrine which purports to provide principles that determine every choice made within science; physicalist principles characterize the structure of science (and our view of nature) once certain choices which are conventional in character are made.

Although this strategy of reply successfully avoids the problem posed by the general non-uniqueness of RTs, similar problems re-emerge with respect to the physicalist status of (T5) and (T6) themselves. For, if (T5) and (T6) are included within natural science, then they must satisfy physicalist principles as follows: (i) their truth must be determined by the physical truths, (ii) the phenomena of explanation must be "physicalized" (i.e., there must be a set of physical attributes each member of which is nomologically sufficient for something's being an explanation), and (iii) there must be an RT for explanation which explains how explanations are realized in nature. Thus, as with (T1)-(T4), (T5) and (T6) have serious problems in satisfying the demands of physicalist doctrine on the assumption that they are parts of natural science. As we shall see in Chapter 5, this point is especially important with respect to understanding what kind of these: the physicalist theses are. In particular, we shall accept the idea that physicalist doctrine is not objectively determined on

physicalist grounds, and conclude that physicalism must be seen as not just another body of theory within science, but rather as principles that are chosen on grounds that fall outside of scientific theorizing: they are principles that constrain such theorizing without being just another instance of it.

The second objection to the theses concerning explanation has been expressed by a number of authors; frequently the attacks have been directed against classical reductionist views, but there is little doubt that the objectors would launch similar attacks against our version of physicalism. Consider the following:

It [Quine's version of physicalism] is not a reductionist doctrine of the sort sometimes imagined. It is not a utopian dream of our being able to specify all mental events in physiological or microbiological terms. It is not a claim that such correlations even exist, in general, to be discovered; the groupings of events in mentalistic terms need not stand in any systematic relation to biological groupings.⁵¹

But the evidence for such physicalistic reducibility is negligible, and even the claim is nebulous since physics itself is fragmentary and unstable and the kind and consequences of reduction envisaged are vague.⁵²

It may be that the operative principles⁵³ are not only unknown but even humanly unknowable because of limitations on our own intellectual capacities...; our minds are fixed biological systems with their intrinsic scope and limits.

⁵¹ See Quine (1979), p. 163. Although Quine talks of the mental and biology, it is quite likely that all of his remarks apply to relations between the mental and physics and between other branches of science and physics.

⁵² See Goodman (1978), p. 5.

⁵³ Chomsky is here discussing physical or whatever other principles may be required for a "physicalist" understanding of mind.

We can distinguish between "problems," which lie within these limits and can be approached by human science with some hope of success, and what we might call "mysteries," questions that simply lie beyond the reach of our minds, structured and organized as they are, either absolutely beyond those limits or at so far a remove from anything that we can comprehend with requisite facility that they will never be incorporated within explanatory theories intelligible to humans. We may hope that the questions we pursue fall into the domain of "problems" in this sense, but there is no guarantee that this is so.⁵⁴

The thrust of the objection that we shall consider is that there is no good reason to believe that a program based upon the theses concerning explanation will be successful. Such theses lead us to search for far more than it is reasonable to expect that we shall find: in short, a program based upon such theses is utopian.

The arguments, for this claim appear to be of many types; none, however, are or purport to be "in principle" arguments. Rather, it is suggested that (i) the meagre success of the program to date provides us with some grounds for believing that the program is not likely to succeed, (ii) there are examples that strongly suggest that the practical problems of actually carrying out the program is beyond us, (iii) the enormity of the task (viz., to provide RTs for all attributes that play an explanatory role in science and to describe the mechanisms that underly all higher level regularities) makes it difficult to conceive of the program's being carried out, (iv) perhaps most interestingly, there may be "mysteries" in Chomsky's sense which don't

⁵⁴ See Chomsky (1980), p. 6. Although this is not couched as an objection to physicalism, the suggestion is that an understanding of the physical bases of mind may be beyond our reach.

admit of solution by us. Rather than develop these objections in detail, we shall simply offer a brief characterization of some lines of reply. Below, we shall return to a consideration of the so-called "utopian" character of the theses in our discussion of the empirical status of the theses and their methodological roles in science.

First, it should be clearly noted that the current objection is not one that concerns the adequacy of the theses: at issue is not whether the motivations of the program are being served by the theses formulated. Rather, the objection suggests that the theses are too strong to be plausible; this consideration has led some (e.g., Quine) to retreat to a weaker form of physicalism. As our earlier discussion in chapters 1 and 2 has been at pains to point out, such retreats involve a tradeoff between adequacy and plausibility: to retreat to weaker theses involves giving up some aspect of the motivations for the program. In the present case, to give up the explanatory goals seems to be giving up one of the primary goal of the program; it is for this reason that it is more important to attempt to develop an adequate version of physicalism than to retreat too quickly in the face of the difficulties encountered in working out the program in science.

Second, as we noted above, none of the arguments for the utopian character of the theses are in principle arguments: it has not been suggested that success of a program based upon the theses of physicalism is a conceptual or logical impossibility. This raises some ques-

tions about what kind of objection the charge of being utopian is and whether or not physicalist theses are the kind of thesis for which such a charge is pertinent.

With regard to the first question, the arguments based upon past limited success and upon practical difficulty in carrying out the program are not very compelling; such arguments suggest that being utopian means to be implausible in the face of good evidence or to be not within our reach for practical reasons. If this is what it means to be utopian, then the arguments offered do not very strongly support the charge; such arguments are not distinguishable from the typical skeptical arguments offered by "nay sayers" of any stage of scientific progress. In support of this reply, we note that very little systematic study has been given to the explanatory proposals suggested (e.g., the role of RTs in inter-field research); what study there has been is not anywhere near as gloomy as Goodman suggests. It is important to escape from the classical picture of the physicalist program to see that inter-disciplinary research is a rich area that has yet to be explored for the most part.

Chomsky's distinction between problems and mysteries is highly suggestive; however, it is not always easy to distinguish the one from the other and it is most important to keep lines of research open rather than to close them off prematurely. The proponents of the present objection have given neither a clear characterization of what

"utopianism" is supposed to be nor a clear characterization of the types of argument that they employ. The mere possibility of the existence of mysteries is not in itself any reason to close out the program based upon the explanatory theses. In recalcitrant cases it is better to "wait and see" than to jump on a skeptical bandwagon.

And finally, with regard to the second question posed above, even if, in some sense, the program is utopian, it is not entirely clear that this is a bad thing; it all depends upon the type of program physicalism is. As an empirical research project it may mean that eventually it must be abandoned; but, as a methodological ideal toward which scientific research is directed, though it can never be attained, it may be superior to any other in achieving the goals and purposes toward which science is directed. Hence, the charge of being utopian is not obviously a problem; below, we shall discuss this line of reply to the objection further.

To summarize this chapter: we have developed three sets of theses which are designed to jointly express the motivations of the physicalist program in science. The theses, (T1) - (T6), fully express those motivations and, hence, are adequate by the criteria set out in Chapter 1. With regard to their acceptability, we have considered a variety of objections, some of which we have rebutted, others of which are still pending. Specifically, the two outstanding objections are (1) that the theses of physicalism are utopian and (2) that the theses of

physicalism do not satisfy the demands they themselves make on claims to truth in natural science. In the next chapter, we shall explore ways of defusing the force of these objections.

5. THE METATHESES OF PHYSICALISM

The purpose of this chapter is to formulate a number of metatheses concerning the physicalist theses developed in the last chapter. The importance of having metatheses lies in the clarification they provide concerning (i) the kind of theses that physicalism involves (e.g., a priori, a posteriori), (ii) the kinds of considerations that are relevant to acceptance or rejection of physicalist theses (e.g., empirical developments in science), (iii) the kinds of things to which the theses are supposed to apply (e.g., scientific theories and their domains), and (iv) the ways in which physicalist theses can be used in the conduct of scientific inquiry.¹

During the past fifty years, there has not been complete agreement regarding the metatheses of the program, although there has been something like a received or dominant view. As we shall formulate it, this Received View consists of three metatheses as follows:²

- (M1) The theses of physicalism apply to natural science.
- (M2) The theses are empirical theses of natural science.
- (M3) The theses play a regulative role in the conduct of natural science.

¹ See Chapter 1 for discussion of the non-cognitive gains of the program.

² Expression of such claims as (M1)-(M3) are found in Fodor (1975), Hellman and Thompson (1975, 1977), Friedman (1975), Field (1972), Oppenheim and Putnam (1958), Causey (1977), and Boyd (unpublished).

future research; as will be seen, such revision will also provide ways of responding to the objections raised against the physicalist theses (e.g., that they are utopian). That the Received View is defective, as some critics have pointed out, is not seen by us as a disaster for the physicalist program; rather, recognition of its shortcomings leads us to a fuller appreciation of the physicalist program in science as well as to a deeper understanding of the nature of science itself.

5.1 THE SCOPE OF THE THESES

In our discussion so far, we have pretty much assumed that the physicalist program is explicitly concerned with natural science. However, this is not an assumption universally shared by physicalists; some view the doctrine as applying to all phenomena and knowledge, not just that which occurs within the confines of natural science. We shall identify two competing conceptions of the scope of physicalist doctrine as follows:

1. broad physicalism - the view that physicalist theses apply to all phenomena and all claims to knowledge.
2. narrow physicalism - the view that physicalist theses apply to all phenomena and claims to knowledge that fall within the boundaries of natural science.

Perhaps, Quine is the most prominent proponent of broad physicalism; this is how he expressed his position recently:

nothing happens in the world, not the flutter of an eyelid,
not the flicker of a thought, without some redistribution of

microphysical states.⁵

Such a claim construes physicalism as an all encompassing doctrine which applies to all claims that can be considered objectively true or false and to all phenomena that can be considered real. Although we are not entirely unsympathetic to this view, it is a view of physicalism which occasionally leads to embarrassment, as Quine himself has publicly acknowledged.⁶ Not only do the ordinary objects of our perceptual world fail to fit cleanly into a physicalist framework, but other kinds of knowledge appear to be problematic for the physicalist (e.g., aesthetic, moral): Goodman has been quite concerned to remind the physicalist of such liabilities.

Although we believe that the broad physicalist is not left with nothing to say regarding such matters, we do not intend to explore this dispute in the current project. First, whatever the outcome of such dispute, all physicalists hold to the idea that the theses apply within natural science. Second, historically, the doctrine is tied most closely to natural science. Third, from a strategic point of view, if the doctrine cannot succeed for natural science, then there would appear to be little point in considering the broader construal; and, as we have seen, narrow physicalism appears to have its hands full in defending against objections. Finally, there is reason to believe that physicalist ideas have a central place in scientific

⁵ See Quine (1978).

⁶ See Quine (1981).

thought as it now takes place; and, as we saw in Chapter 1, the motivations of the program can be viewed as being focused upon gains resulting from a certain kind of growth of science (i.e., growth toward a goal state with the structural features described by the theses). For these reasons, we have assumed narrow physicalism and will continue to do so in the remainder of this project; whether or not a broader construal of the program is viable is a matter we shall leave open. Again, if the narrow construal fails, there is little need to explore the issues that arise that are peculiar to a defense of the broader view.

Another consideration militating for the narrow construal is the prima facie implausibility of the broad view with regard to the explanatory theses. The idea of a realization theory for "beauty" boggles the mind. On the other hand, explanatory concerns are, in general, central to science, and the explanatory gains promised by the success of the physicalist program are quite tempting; these same kind of explanatory gains are not as tempting with regard to moral or aesthetic matters. And, as we have noted, they are less plausible. In science, explanatory connections at all points and in all directions are at a premium; not so in all areas of thought.⁷

⁷ It is most likely the case that physicalism underwrites a clearcut "fact-value" distinction in nature for roughly the reasons cited in the text: i.e., there are not likely to be any physicalist accounts of values to be had.

So, given our assumption of narrow physicalism, the question of greatest importance that arises is: is it possible to independently characterize nature and natural science? What hangs upon a satisfactory answer to this question is (i) whether or not the theses may be said to have a well defined content, (ii) whether or not there are clearly defined sources of confirmatory and non-confirmatory evidence for the theses, and (iii) whether or not there is a well defined scope of application for physicalist principles employed in the methodological roles supposed by physicalists (cf., (M3)). More specifically, satisfactory answers to this question will determine whether or not physicalist theses are vacuous or not, empirically testable or not, and usable or not. What, then, is natural science and its intended object of study?

Clearly, a full answer to such a question is well beyond the scope of the current project; in what follows, we shall briefly survey the situation and attempt to set up the problem as it pertains to physicalism more fully. Early physicalists, such as Carnap and Feigl, were concerned to distinguish natural science from formal science (e.g., logic and mathematics) and to distinguish the natural order from the abstract realms studied within the formal sciences. Thus, the natural order was construed as a spacetime manifold the extents of which put upper limits upon the phenomena with which we may be in causal contact. Not only does such a picture put limits upon what is to count as natural phenomena (e.g., mathematical objects are ruled out, for exam-

ple), but there is implicit endorsement of the idea that only those types of phenomena with which we have some kind of causal connection are legitimate objects of knowledge within natural science (e.g., again, knowledge of abstract entities is not knowledge within natural science.) Thus, knowledge concerning some phenomenon must be mediated by causal contact with the phenomenon if it is to count as knowledge within natural science.

Further, early physicalists conceived natural science as not including theory of values (e.g., moral or aesthetic phenomena). Although such phenomena are part of the natural order, knowledge concerning them is not part of natural science. (The reasons for this claim presented by the early physicalists would take us too far afield.) However, we shall take it for granted that the claim is in fact correct (i.e., that value theory is not part of natural science).⁸

Finally, Putnam⁹ has suggested a distinction between "hard" and "soft" sciences. The distinction is not clearly made, but it appears to be based upon a marked difference between the methods of the two kinds of science: soft sciences are infected with "interest relativity" whereas hard sciences are not. As a consequence, soft sciences do

⁸ If values are a part of the natural order, then this exclusion means that natural science is not concerned with all aspects of the natural order; there are other conditions besides ontological conditions which bear on whether or not something is a part of natural science.

⁹ See Putnam (1975).

not conform to physicalist standards whereas hard sciences do. We are not in a position to lay out succinctly what interest relativity is supposed to be or to judge whether it is a coherent notion. Putnam seems to think that it is and that psychology, semantics and the social sciences satisfy it, whereas physics and biology do not. We shall read Putnam as holding that it is the "softness" of certain sciences which lead to their failure to conform with physicalism; hence, the distinction he suggests does not, on our reading, depend upon satisfaction of physicalist theses. For present purposes, then, we shall remain open to Putnam's alleged distinction between sciences (we shall read "hard science" as "natural science"), but we shall not stand committed to his particular assignments to one or the other kind. Hence, it is possible that scientific knowledge includes soft and hard sciences as Putnam suggests, but that the specific sciences he claims to be soft may be "hard": at a minimum, they are hard cases to decide upon. Again, for our purposes, the division is to be seen as separating natural (hard) science from other branches of knowledge on the basis of methods; methods which involve reference to the interests of the knower and which involve certain forms of projection and rationalization are not natural science.¹⁰

¹⁰ We are inclined to agree with this claim, but think it incorrect to attribute such methods to psychology or semantics and, hence, to view them as not properly within the scope of physicalist theses.

To summarize to this point: our problem is, independently of physicalist theses, to characterize natural science and its subject matter. So far, we have gone through a series of exclusions from which we may conclude that strategy for delimiting the boundaries of natural science proceeds along two fronts: (i) ontology and (ii) method. Thus, formal science is excluded on the grounds that its ontology is not part of the natural order; value theory and the "soft sciences" are excluded on the grounds that their methods of inquiry and justification depart from those characteristic of natural science, despite the fact that values and the subject matter of the soft sciences occur within nature.

We observe that if part of the method of natural science is to require that phenomena and knowledge that fall within its domain must conform to physicalist principles, then an independent characterization of the scope of such principles is not going to be forthcoming. If, on the other hand, such principles are not part of the method, then the only burden would appear to be that of developing an acceptable characterization of the method and ontology of natural science which correctly constrain what is to count as natural phenomena and knowledge within natural science. Because (M3) requires that the theses play a methodological role in science, it is not at all clear that the physicalist can escape the objection that the scope of the theses is not independently characterized and thus that the theses are self-supporting and trivial. For the present, we shall leave open

this issue. However, we shall return to it below to consider whether or not it is possible to include physicalist theses within the class of principles which delimit natural science without trivializing the physicalist program.

5.2 THE EMPIRICAL STATUS OF THE THESES

(M2) has been a quite central tenet of the Received View. Its significance is, at least, twofold: first, the theses are viewed as scientific hypotheses subject to confirmation or refutation by arguments based upon empirical evidence and the methodological principles of natural science; and second, given (M1), the theses, being a part of natural science, apply to themselves. In what follows, we shall discuss these points in turn with an eye toward problems that may exist for (M2). As we shall see, some rather serious problems do in fact exist and later in this chapter we shall consider the possibilities for revision concerning the type of theses that physicalist doctrine is constituted by.

The alleged empirical status of the theses raises the following questions: (1) what kinds of evidence and forms of argument are pertinent to the confirmation of physicalist theses?, (2) what kinds of evidence and forms of argument are pertinent to the disconfirmation of physicalist theses?, and (3) what is the nature and status of the program for gathering evidence and presenting arguments bearing on the

theses?¹¹

As suggested by proponents of the Received View, the primary sources of evidence for the evaluation of physicalist theses are developments in science; given such developments, there are, at least, four types of argument that might be proposed in the service of confirmation of the theses. First, it might be thought that only by an exhaustive study of all cases within the structure of scientific knowledge and ontology could an adequate assessment of the theses be made; thus, only in some ideal limit of scientific development could the theses be evaluated. Only the complete unfolding of science will reveal whether or not the theses are true. Now, not only is this a totally impractical proposal because it simply defers indefinitely the empirical assessment of the theses, but it also is not a sound proposal since the structures of knowledge and ontology are "too big" for a case by case assessment conducted by humans. This method of assessment has been mentioned only to point out some of the issues that must be met by a more acceptable one: first, assessment must take into account the temporal unfolding of science and hence it must reach into the future; and second, assessment must take into account the inaccessible parts of the knowledge and ontology of science.

¹¹ Because the theses are more or less logically independent and because they are of quite different types (e.g., linguistic and non-linguistic), the kinds of evidence and arguments pertinent to their evaluation may vary considerably. Hence, a full treatment of the above questions must take this potential variability into account. In the present project, we shall avoid the full complexity of this task.

A second strategy for marshalling evidence to support physicalist theses involves some form of induction based upon developments in science. Such a strategy is perhaps the dominant one and is clearly superior to a case by case study insofar as it promises to permit access to temporally distant versions of science as well as to all points in the structure of scientific knowledge and ontology; such access is to be based upon the consideration of a finite number of actual developments. However, despite these virtues, there are problems with this approach, not all of which are easily fended off.

First, in a recent monograph,¹² Boyd has discussed what looks like a fatal flaw in the general conception of how physicalism is to be tested: if physicalism is an ontological view which is expressed in terms of the doctrine of definitional and derivational reduction of the vocabularies and theories of all branches of natural science to physics, then inductions upon successful reductions are not legitimate confirmatory arguments for the ontological view: "they commit the fallacy of arguing from the features of a formal system to features of the interpretation of the system".¹³ Boyd's point is well taken and an important insight into the problem of the empirical testing of physicalist theses. However, the critical point is not clearly pertinent to the testing of our version of physicalism since we have gone to great lengths to differentiate linguistic and non-linguistic theses within

¹² See Boyd (unpublished).

¹³ See Boyd (unpublished), p. 32.

our formulation and we urge the independent testing of such theses. The issues of whether or not this is possible and of how it is to be done if possible are outside of the scope of the current project. Boyd's point should be kept in mind when these issues are addressed, needless to say.

Second, Goodman suggests¹⁴ that the scientific developments that have in fact occurred which are supportive of physicalism are quite minimal; thus, of course, an inductive argument for physicalist theses cannot get off the ground. In response, we are here concerned only with the acceptability of certain forms of argument, not with their actual merit vis a vis physicalism. Further, Goodman's discussion is so brief as to not permit close scrutiny of his claim. Is he, for example, talking of classical reductionism or of some other form of physicalism? More importantly, what is the argument for his scepticism? Has Goodman done the survey of current science as it bears on physicalist theses that is required for his pronouncement? If not, then it is difficult to take his claim seriously. Any serious assessment of the empirical standing of physicalist theses must involve a detailed survey of various branches of science and their interconnections.¹⁵

¹⁴ See Goodman (1978).

¹⁵ See Oppenheim and Putnam (1958) for perhaps the only serious effort in the physicalist literature to conduct the required survey.

Third, there are a number of questions regarding the nature of an inductive argument based upon developments within natural science which have not been adequately addressed by physicalists. It is in the difficulty in providing satisfactory answers to these questions that the real problems for inductive strategies for confirming physicalism exist:

1. What counts as a representative sample of cases sufficient for providing a basis for an induction? (e.g., are there specific critical cases that must be included in any sample?)
2. What counts as a good case such that it belongs within the basis for an induction? (any science, mature science,...?)
3. Is there a well defined principle of induction which permits the inference from the basis to the entire structure of science?
4. What assumptions are made about the structure of science and nature which permits the application of inductive methods? Is there some assumption of uniformity made which may underwrite an induction but which may also be question-begging vis a vis physicalism? Is it assumed that all natural sciences are alike in some respect? If so, what respect?
5. Does the employment of inductive methods presuppose that the scope of the physicalist doctrine is well defined? If so, then the fate of inductive arguments depends upon satisfactory resolution of the difficulties that (M1) faces.

Full exploration of the issues raised by these questions falls outside of this project. However, we contend that no satisfactory discussion of these issues exists anywhere in the physicalist literature and that the implicit and explicit reliance on inductive forms of argument for physicalism is not justifiable at this time. This suggests that, if these questions cannot be satisfactorily answered and if no other forms of empirical argument for physicalism are provided, then the empirical status of the doctrine is dubious. We shall now look at some alternative forms of argument that have been suggested to see if any are clearly acceptable.

A third form of confirmatory argument that might be employed in support of physicalism runs as follows: if assumption of the truth of physicalist theses leads to certain kinds of scientific success (e.g., discoveries, theoretical advances), then a plausible explanation of such success is that the assumption is true, and hence, so are the theses. If, further, all other available explanations of success are inferior, then the physicalist theses would gain support via an inference to the best explanation.

The virtues of such a form of argument are similar to those of inductive arguments (i.e., it permits inference on the basis of a finite set of actual cases to the entire structure of fully developed science and its ontology). However, the liabilities of inductive strategies are also shared; analogs to the questions raised above can be easily

formulated for the form of argument now being considered. Thus, the identification of legitimate cases for which a success argument applies, the conditions and assumptions which underwrite the employment of "inference to the best explanation", and satisfactory solution to the problems besetting (M1), are all part of the burden that supporters of the present form of argument must bear. As indicated above, we are skeptical about the prospects for meeting these difficulties.

The last form of supportive argument that we shall consider has been offered by Boyd.¹⁶ He suggests that support for physicalism can be obtained from "possibility arguments" which undermine inappropriate scepticism and provide some plausibility for the theses. He cites three cases drawn from contemporary science which are alleged to demonstrate that traditional thorns in the physicalist's side are not as sharp as opponents of physicalism think. Thus, he suggests that (i) functionalist metatheories for psychology,¹⁷ (ii) the discovery of microbiological mechanisms for genetic transmission of traits, and (iii) developments in artificial intelligence research all provide grounds for rejecting stock skeptical arguments against physicalism by showing the possibility of physical realization of non-physical phenomena (e.g., intentionality, intelligence, life).

¹⁶ See Boyd (1980).

¹⁷ We observe that the promise of functionalism has significantly faded in recent years, given its apparent inability to handle mental states with either propositional or qualitative content.

We shall not consider the specific merits of Boyd's possibility arguments here; but, we suggest that as a form of confirmatory argument for physicalist theses these arguments are subject to the same difficulties faced by the previous forms. That is, although they may achieve their purpose of undermining inappropriate scepticism, it is not at all clear how much support they provide for the theses and whether or not it is possible to specify and defend the conditions and assumptions upon which such arguments can be said to confirm the theses. The principal issue facing all of the forms of argument we have considered is whether or not they can be justified as legitimate forms of argument with respect to physicalism without making assumptions which beg the question in favor of physicalism; that is, what assumptions permit the inference from a finite set of cases to all of science without begging the question?

We now turn to a consideration of disconfirmatory evidence and arguments. As we shall see, the situation is problematic in roughly the same way as for confirmatory evidence and arguments. The second question raised by the alleged empirical status of the theses can be restated as follows: what are the kinds of developments in science that would provide an evidential basis for disconfirmatory arguments against the theses of physicalism? In addressing this question, the first and principle form of argument that we shall consider is that of providing an explicit counterexample to physicalist theses. As natural as this form of argument is, it is beset with several difficulties.

The first such difficulty is that identifying legitimate counterexamples is quite problematic. There are, as Field¹⁸ has pointed out, quite probably no "crucial experiments" that would decide the fate of physicalism once and for all; thus, there are no easy routes to the discovery of counterexamples. Further, as empirical theses, the physicalist theses are alleged to possess a considerable amount of "centrality" within the corpus of scientific knowledge.¹⁹ Thus, there is a stronger conservative pressure to retain physicalist theses as compared with specific claims which constitute counterexamples to those theses. In addition, such centrality as well as the important methodological roles played by physicalist theses suggest that the theses would not be easily given up unless viable alternative views capable of filling the gaps left by physicalist doctrine were available.

Finally, as Chomsky has suggestively discussed, science may abound with "mysteries" which are beyond the scope of our capacities to find solutions. This possibility suggests that we may, in many cases, have difficulty recognizing a counterexample as opposed to a mystery or, merely, a "problem" for which we have not yet found a solution.²⁰ The existence of repeated failures at trying to establish that certain

¹⁸ See Field (1972), p. 357.

¹⁹ See Quine (1953) and Putnam (1962) for discussions of the notion of centrality.

²⁰ See Chomsky (1975, 1980) for discussion of problems and mysteries; also see Bromberger (1966) for a discussion of p-predicaments and related notions.

physicalistic relations exist may provide some grounds for believing that a counterexample exists; but such grounds will be substantial only if the alternatives mentioned here can be plausibly ruled out (i.e., only if we can rule out the existence of a recalcitrant problem or a mystery). Deciding when "enough is enough" is not a very clear enterprise at all; and, the thousands of years of scientific change should warn us against too hastily deciding to give up on a problem.

So, if the main form of disconfirmatory argument is one based upon the discovery of a counterexample, then the above considerations suggest that such a counterexample search is likely to be extremely difficult. We now turn to some considerations which suggest that such a strategy may suffer from more principled difficulties.

A further complication in regard to identifying a real counterexample to physicalist theses involves the satisfactory resolution of the problems with the scope of the theses (i.e., the problems with (M1)). Unless we have some clear idea of what counts as a branch of natural science, we will have no clear idea of whether a given putative counterexample is real or not. Something cannot be a counterexample to physicalism if it falls outside of the scope of the doctrine; insofar as the scope is unclear so will the class of possible counterexamples. Thus, for example, if mentalistic psychology is not legitimate natural science, then its failure to conform to physicalist principles is not a real counterexample to them; if it is legitimate natural science, then such failure does count as a real counterexample.

Finally, not only is it a problem for the empirical testability of the theses if their scope is not well defined; but, it is also quite problematic if physicalist theses play an important role in determining what is and what is not legitimate natural science.²¹ It would appear that if physicalism is an important criterion in deciding upon what counts as legitimate natural science, then the possibility for there being a counterexample to the theses is significantly reduced, possibly to the point that physicalism is not empirically testable because it is employed in a manner that screens out potential counterexamples. The problem here is fundamentally that of not having an independent test of what counts as natural science. Below we shall explore in more detail the methodological roles of the theses and their bearing upon the empirical status of the theses.

We shall simply mention two additional forms of disconfirmatory arguments that may be brought to bear upon physicalist theses. The difficulties of the form of argument just discussed are likewise difficulties for the forms of argument that follow: first, Boyd's possibility arguments suggest that the failure to provide such arguments would be grounds for rejecting the theses; and, second, analogous to the success arguments mentioned above, "failure arguments" involving an inference to the falsity of physicalist theses as the best explanation of a scientific failure might also have some negative force vis a vis physicalism. Both of these argument forms suffer from

²¹ See below for discussion of the methodological roles of the theses.

all the difficulties of their positive analogs.

To summarize: both confirmatory and disconfirmatory arguments suffer from both severe practical difficulties in their application as well as some potentially serious principled difficulties. As a result, the possibility that physicalist theses are not empirically testable must be seriously entertained. We shall return to this issue below, after we have discussed (M3).²²

The second point of significance regarding M2 is that, given M1, the theses, being a part of natural science, apply to themselves. As a consequence, the idea that the physicalist theses are "overarching empirical theses" of science, though quite natural and widely adhered to, raises serious difficulties that may not be easily met. In a nutshell, because the theses involve semantical and intensional notions (e.g., nomologicality, explanation, truth determination), viewing the theses as a part of natural science presupposes that these notions are "physicalistically acceptable" (e.g., that the truth values of sentences involving them are determined by the truths of physics, that there exists an RT for each such notion, etc). As we shall see, the issues here do not lead to definitive objections to physicalism, but they do set the task of working out the physicalist program at a very

²² The third question raised by the empirical status of the theses was a question concerning the nature and status of the research program whose goal it is to amass evidence bearing upon the theses. Such an assessment falls outside the scope of the present project, and we shall only briefly comment upon it toward the end in our discussion of the acceptability of the program and future research.

high level (i.e., the success of the program depends upon incorporating these notions within natural science). And thus, as we shall see below, the issues here create a certain amount of embarrassment for Quine who has steadfastly held to both the empirical status of physicalism and the scientific unacceptability (on physicalist grounds) of semantic and intensional notions. For now, let us observe that (M2) raises the question of how physicalist doctrine is to be located in relation to natural science; and, the specific solution it expresses (viz., that the theses are an integral part of natural science), has some potentially quite serious liabilities.

5.3 THE METHODOLOGICAL ROLES OF THE THESES

The last metathesis, (M3), expresses the idea that physicalist doctrine plays a methodological role in the conduct of natural science. This is, of course, compatible with the empirical status of the theses as well as with the denial of such status (i.e., (M2) and (M3) are independent of each other, a fact that will loom large below). A clear understanding of (M3) requires us to distinguish between normative and descriptive interpretations of its content: that is, we must distinguish between the claim that physicalist theses do, in fact, play a certain methodological role in science and the claim that they ought to. Further, it is important for this purpose to distinguish between implicit and explicit functioning of the theses in their methodological employment: the theses may be operative despite the fact that they

are not used explicitly in scientific deliberations (e.g., they may inform a whole approach to a subject matter or they may be the unspoken and not even consciously thought hidden premises in a piece of scientific reasoning). The issues here concern whether or not a theory of scientific practice must be a theory of the psychology of scientists or a correct rational reconstruction of scientific practice, etc.²³ We shall not directly address these issues; rather, our approach will involve assuming that a thesis plays a role in scientific practice if a rational reconstruction of that practice says so. The explicit employment of a thesis will be taken as a sufficient, but not necessary, condition for such an attribution.

Our strategy in this section will be to address the following questions: (1) What are the roles that physicalist theses do/ought to play in the conduct of scientific inquiry?, (2) Do they in fact play such roles?, (3) Ought they to play those roles?, and (4) Are there any difficulties with the view that physicalist theses either do or ought to play such roles? In a nutshell, we shall conclude that the theses do have and ought to have a place in science; the road to this conclusion, however, is neither short nor easy.

²³ Barbara Von Eckardt has discussed these distinctions in Von Eckardt (unpublished).

What, then, is the methodological role of physicalist theses supposed to be? There are two quite distinct roles:

(R1) Physicalist theses guide research in that they define research questions and problems.

(R2) Physicalist theses provide a basis for assessing the scientific acceptability of theories advanced within science.

We shall consider each of these in turn.

In our discussion above of vertical science, we mentioned a number of the questions and problems raised as a consequence of taking physicalist theses seriously vis a vis the relations between the ontologies and doctrines of different branches of science. It is the raising of such questions and the provision of some constraints upon answers which constitutes part of the impact of physicalist theses upon the course of scientific research. From the physicalist point of view, the theses define a set of research questions and problems which provide structure to what has been called "inter-field research". Our own preference is to view the theses as defining new fields of research, and, thus, we have employed the phrase, "vertical science". Such inter-field problems (e.g., the problems of microbiology, neuropsychology, physical chemistry, etc.) arise roughly as follows: if a theory of some phenomena is developed in one of the "higher-level" branches of science (e.g., psychology), then physicalist doctrine entails that (i) the attributes posited by the theory are physically realizable and, in actual cases, are physically realized, (ii) the theory is truth deter-

mined by physical doctrine, (iii) for each of the attributes posited by the theory there is an RT which provides an account of the realization of that attribute, and (iv) there are explanations possible, based upon physical doctrine, of the occurrence of each individual phenomenon in the domain of the theory and of each instance and exception of the regularities expressed by the theory. Such consequences provide the basis for a prima facie plausible research program aimed at studying the physical basis for the phenomena with which the higher-level theory is concerned.

An example, from the study of language, proceeds as follows. Given the development of theories of natural language, theories of its structure, acquisition and use, which posit various properties of language including semantic properties, and given physicalist doctrine, a research program aiming to answer (at least) the following questions is suggested: (i) How is it possible that semantic (and the other linguistic) properties of language are realizable by physical systems?, (ii) In virtue of what physical properties and relations are semantic (etc) properties actually realized?, (iii) What is the nature of the physical systems that instantiate the regularities, their instances and exceptions, that theories of language express?. A constraint upon the answers to such questions and any others are that the higher-level theory and its ontology, and physical theory and its ontology, conform to the theses of physicalism.

It is important to distinguish between (1) the physicalist doctrine (i.e., presuppositions (P1)-P3), theses (T1)-(T6), and metatheses (M1)-(M3)) and (2) the specific projects within vertical science which are, in effect, the working out of the physicalist program in science. As conceived above, the general program, unlike classical reductionism, is quite compatible with considerable variation in the form of theories in vertical science from one area to another. The constraints are that realization theories be provided, that the determination theses be satisfied and that explanations be provided. The details of the form that these theories and explanations take are not relevant to the program as presently conceived.

A proper clarification and development of these programmatic claims would involve close scrutiny of specific examples drawn from science. Such examples might include (i) physical theories of valence (cf., Field (1972)), (ii) microbiological theories of genetic transmission (cf., Schaffner (1969), Hull (1974)), (iii) neuropsychological theories of depression (cf., Clark (1980)), (iv) fundamental physical theories of such "phenomenological" physical properties as temperature, pressure, transparency, etc, and (v) theories of the physical basis for linguistic and semantical phenomena (cf., Field (1975)). Such scrutiny of cases is not part of the current project; the reader is referred to the references cited for initial discussion of some of the examples.

So, the claim is that the first methodological role that the theses play is to guide research by defining problems and questions for research in "vertical science". It is far from uncontroversial that the theses either do or ought to play such a role in science. Philosophy of science has witnessed during the past 15 years a number of attacks on exactly this claim with respect to the employment of the theses of classical reductionism. Whether the objections apply, mutatis mutandis, to the physicalist theses proposed above is an open question given the rather significant differences between those theses and classical reductionism. With this issue in mind, we shall now address the questions of whether physicalist theses do play the alleged role in science and whether they ought to play such a role.

That physicalist theses do play the role (R1) attributed to them is best argued for in terms of specific examples in which implicitly or explicitly they guide the thought and research activity of practicing scientists. That the history of science can deliver such examples is, we think, beyond doubt; the examples cited above are a beginning to a long list of such cases.²⁴ More specifically, with regard to physicalist ontological and explanatory theses, the search for the physical bases of depression, genetic transmission, valence, phenomenological properties of classical thermodynamics and optics indicate implicit or explicit belief that such bases plausibly exist and that they can be understood and used to understand the higher level phenomena. A spe-

²⁴ Pace Goodman.

cific reconstruction of each of these examples is required to make good on this claim. Such reconstructions, however, must be deferred.

Supplementary to such positive arguments are negative arguments designed to deflect the objections of critics and place the burden squarely on their shoulders. First, there is a substantial lack of clearly articulated alternative construals of research into the physical bases of various phenomena. If physicalist theses are denied the role attributed to them by (R1), then what reconstruction of scientific activity constitutes a plausible replacement for them? The lack of such an alternative is a severe liability for the sceptics. Second, a number of the negative arguments of such critics of (R1) as Schaffner, Wimsatt, and Maull and Darden are directed at classical reductionism: they cite the lack of interest in derivational and definitional reduction by biologists as well as the highly limited construal of inter-theoretic relations that such reductions stand for. On the first count, we can only agree and claim that such lack of interest is beside the point with regard to the physicalism cited here. On the second count, we also agree, but neither classical reductionists nor contemporary physicalists claim that inter-theoretic relations are exhausted by the theses proposed. The critics have a good point if they are claiming that other relations have been neglected. But such neglect is benign and not grounds in itself for rejecting the positive claims of reductionism or physicalism. On our view, the vast majority of extant critical arguments with regard to (R1) are not relevant to

the physicalist doctrine; however, really close scrutiny of the role of this doctrine is yet to be done.

The sketchy character of this discussion needs to be supplemented by the detailed work of exploring the many cases, both cited and uncited, which would clarify whether or not the theses put forth actually play the role (R1). For now, we shall proceed on the assumption that the results of such work will yield a positive answer.

We turn now to discussion of the question whether physicalist theses ought to play the role (R1). The central preliminary issue concerns what are the best ways for arguing for or against the claim that the theses ought to play such a role. We shall lay out three different strategies that have been suggested for supporting the claim that the theses ought to be employed in the role (R1) and discuss the merits and difficulties of each one.

Perhaps, the most common line of argument is one that runs as follows. The employment of physicalist principles has been quite fruitful in leading to scientific progress; hence, the continued use of such principles is both justified and mandated, given that the alternatives are likely to be less fruitful. Admittedly, such an argument requires fleshing out and considerable support for its premises; however, the intent is clear. Physicalist principles have been used with success in the past, thus they should continue to be used. The power of this argument depends upon two things: first, that there has, in fact, been

the kind of success in science resulting from employment of physicalist principles, and second, that there is sufficient uniformity among branches of natural science such that it is reasonable to expect that what has been successful in one branch of science will be successful in others (i.e., what assumptions underwrite the employment of principles, that have been successful in one area, in other areas?).

We observe that, with regard to the question of fact, there is considerable disagreement: on the one hand, Goodman writes in The Ways Of Worldmaking that physicalism has had only limited and mostly partial success; others would contend that there is a broad spectrum of successes resulting from the methodological employment of physicalist principles. As we claimed above, resolution of this disagreement awaits a detailed survey of the empirical facts regarding success or failure in many areas of science.

With regard to the issue of uniformity in science that underwrites generalization of successful principles, issues similar to those that arose with respect to inductive support for the principles arise here. The idea that a successful principle is one that ought to be used generally should not be confused with the idea that successful principles in one area are worth trying in different areas until they fail. It is the "ought" that is problematic. It is difficult to see how one could argue for the required uniformity without assuming that physicalism is true; anything less would seem to justify giving the principles a try,

but not that they ought to be employed.²⁵

A second line of argument for the claim that physicalist principles ought to be employed in role (R1) is as follows: if physicalist theses are true then they ought to be employed in methodological role (R1) (and (R2)); thus, insofar as we have reason to believe that the theses are true, we have grounds for believing that they ought to be employed in the conduct of science. The burdens of this argument are quite heavy. First, as we have seen there is dispute about the kinds and amount of support that the theses have - the truth of the doctrine is very much up for grabs. Second, even if the doctrine were known to be true, it does not follow that it ought to play methodological roles in science. Such a further claim involves detailed argument of a very practical nature. Truth, by itself, does not guarantee that a thesis will be usable in science or that it will be productive: these things depend upon further facts about science and scientists. The point here is that this line of argument must be considerably developed before it

²⁵ Chomsky has contended in his debates with Quine regarding indeterminacy that physicalism does not seriously constrain the activity of practicing linguists and psychologists: that their activity can and does proceed without any thought about whether their theories conform to physicalist principles. It would appear to follow from this that it is false that physicalism ought to play the roles alleged by the metatheses. But, this is, of course, not a consequence of Chomsky's view as can be seen by the fact that elsewhere he takes quite seriously the idea that research into the physical bases of mind has an important place in the overall research program in psychology. His point is that the employment of such principles has its time and place; in psychology the time is not now because of its immature status. Similar points apply to (R1) and (R2) as we shall see below.

leads to the desired conclusion.

The final line of argument that we shall consider is one that we have highlighted throughout this project: viz., that the potential gains of the program provide strong motivation for pursuing it, and that in the absence of solid grounds for not pursuing it and in the absence of better alternatives, the program ought to be pursued; and, hence, the theses ought to play the methodological roles attributed to them. This is not an endorsement of a "monopolistic" physicalism; certainly, other programs are possible and of potential value. It is to say, however, that if the premises in the argument are true, then there is some prescription for pursuing the program. The burden is, of course, to secure support for those premises. Since our purpose here is only to sketch the arguments and point out the burdens, we shall not develop this argument now (it is, in fact, a very elaborate undertaking). What the current project is designed to do, vis a vis this argument, is to clear away a number of objections to the program which are quite current (e.g., indeterminacy of the theses, lack of empirical status, poorly defined scope, utopian character of the theses, monopolistic character of the program). If we are successful in this, then at least some of the grounds for not pursuing the program will have been undercut. The force of this line of argument will increase insofar as we are successful.

To summarize: there are three lines of argument in support of the claim that the theses ought to play methodological role (R1): (i) the argument from success, (ii) the argument from truth, and (iii) the argument from potential gains. All are in need of substantial elaboration and defence, the first two being beset by difficulties encountered earlier when we discussed (M1) and (M2).

We shall now turn to a discussion of the second methodological role, (R2) (i.e., the evaluative role of the theses). That physicalist principles play a role in the evaluation of theories in science has been thought by a number of philosophers of science.²⁶ According to such philosophers, the theses function as a constraint upon what is to count as acceptable natural science. For example, Quine has argued for many years that it is because certain kinds of psychology and linguistics fail to conform to physicalist standards that they are to be rejected as not good science. He alleges to have demonstrated that, given the physicalist theses and given a very broad conception of the physical bases, theories in linguistics, semantics and "mentalistic" psychology fail to bear the physicalistically correct relations to the physical bases. As a result, he concludes that such theories are not appropriately considered to be a part of natural science. Such reasoning gives rise to a number of questions: (1) What is the structure of the methodological situation in science that such reasoning con-

²⁶ The list includes at least the following: Tarski, Putnam and Oppenheim, Quine, Field, Fodor, Friedman, Hellman and Thompson, and Kim.

cerns?, (2) Given such a situation, in what ways might it be resolved?,²⁷ (3) What considerations are pertinent to resolving such situations?, and (4) Why, if at all, should such situations be taken seriously? under what conditions?

Our plan is, first, to give a detailed characterization of the kind of methodological situation that the evaluative role of physicalist theses can give rise to and to delineate the various ways in which such situations can be resolved; second, to sketch the kinds of considerations that are pertinent to resolution; and, finally, to discuss the conditions under which such a situation should be taken seriously. Throughout this discussion, we shall use the debate concerning the physicalistic acceptability of semantics as an example. This example is especially relevant to the current project since it bears directly upon the objections discussed above regarding whether or not the theses were subject to indeterminacy and hence not acceptable according to physicalist standards.

What then is the structure of the methodological situation to which the evaluative role of the theses can give rise? Because it is the thesis that Quine has focused upon in recent years, we shall restrict our discussion to the thesis of truth determination; it is clear, however, that any of the other theses would be equally appropriate and that everything we say applies mutatis mutandis to them. Now, as we

²⁷ Quine has, in fact, opted for one of four possibilities.

understand Quine, he has employed the physicalist thesis of truth determination in an argument with the following structure:

1. If translation theory is not truth determined by physical theory, then it is not acceptable natural science,
2. Given current conceptions of translation theory and physical theory, truth determination doesn't hold,
3. Thus translation theory is not acceptable natural science.

As we shall construe the violation charged by Quine, it is that given a version of physical theory (i.e., all the truths of physics) and, given a view about the nature of translation (i.e., a realization theory for translation),²⁸ specific translation between any two languages is not determined (i.e., there are equally acceptable translations that are incompatible with each other and equally compatible with the physical truths). This is a violation of physicalist doctrine insofar as truth determination requires that the physical truths fix a unique translation. The key to the indeterminacy here is that, given the physics and given the realization theory for translation, there is a multiplicity of translations that are possible: this is what indeterminacy relative to the physical basis consists in.²⁹

²⁸ For example, a translation between two languages, L and L', is a meaning preserving mapping from the sentences of L into sentences of L', where 'meaning' refers to stimulus meaning. See Quine (1960) for discussion of such a theory of translation. See also Putnam (1975) for an alternative theory.

²⁹ The indeterminacy here is quite different from the indeterminacy discussed above in chapters 3 and 4 which resulted from the existence of alternative physical bases, alternative realization theo-

The general methodological situation might be viewed as follows: at some point in the course of scientific development (i) given a high level (i.e., non-physical) theory, T, which appears to satisfy scientific standards of adequacy (e.g., compatibility with available evidence, simplicity relative to its subject matter, explanatory power superior to its known rivals, etc), (ii) given all lower level theories (i.e., all physical theories), P, at that time, and (iii) given the known realization theories for all constructs in T, it appears that the theses of physicalism are not satisfied for T (e.g., T is not truth determined by P).

Because, in its second methodological role, physicalist doctrine requires that such theses be satisfied (and, hence, that the relevant physicalist relations hold between P and T), something must give in the situation described. There are four possible ways of resolving the conflict: (i) reject T, (ii) reject P, (iii) claim that it is false that T and P do not satisfy physicalist theses, and (iv) reject physicalism. It is interesting that each of these options has been taken by some party or other in the recent debates regarding semantics; we shall now turn to a discussion of (i)-(iv) in relation to those debates.

ries for attributes or from alternative mappings from the ontology of a special science into the ontology of physics. These latter forms of indeterminacy are completely general in science and not subject to physicalist constraint; they require resolution on other grounds. Physicalist indeterminacy, on the other hand, assumes resolution of these sources of indeterminacy and exists despite such resolution.

That the high-level theory (e.g., reference assignments, mentalistic psychology, translations)³⁰ should be rejected in the above described type of situation has been the favored option of Quine all along. The apparent failure of translation to be physicalistically acceptable, given current conceptions of physics and of the nature of translation, has seemed sufficient for its rejection as a branch of science: it is not objective on physicalist grounds in the way that scientific enterprises ought to be. There are, at least, two problems with Quine's view that he has not resolved: first, his discussion of translation is based exclusively on a behavioral theory of meaning and, second, he has not offered reasons for why it is the rejection of the high-level theory rather than one of the other options that should be adopted. The issues here are quite complicated and cannot be discussed fully now; but, Quine would certainly reply to the first problem that the burden of proof is clearly on the shoulders of someone who thought he had an alternative theory of meaning or translation.³¹

³⁰ The issues regarding translation are to be sharply distinguished from those concerning reference and psychology, although they may eventually be related in certain strategies of reply (e.g., reductive theories of meaning where meaning is reduced to either reference or mental state). It is the general structure of the situation that we are focusing on; the discussion of the different areas will vary considerably in detail.

³¹ See Putnam (1975) for explicit statement of this reply in his defence of the indeterminacy thesis.

The second option, viz. to reject P, has had two proponents; first, Hellman and Thompson have viewed physicalism as a constraint upon the adequacy of physics.³² Given their extensionalist leanings, the crisis with translation may not arise in a serious way; however, it appears that they would consider the rejection of physical theory as a viable option in situations of conflict although they, like Quine, have not elaborated very clearly what the rationale for preferring one of the various options over the others might be. Second, Chomsky likens the situation with translation to that of electromagnetic theory at the turn of the century and suggests that, if necessary, physics could be altered to incorporate a new fundamental type of phenomena. Again, when it is appropriate to take this line over others has not been clearly characterized.

The third option, perhaps the most interesting, has a number of variants. First, as Field and Friedman have both suggested, it could be that, although it has not been discovered yet, T and P do relate to each other in the physicalistically required way: showing this is an as yet unsolved problem in science. Solving such a problem may involve simply showing that the existing RTs are sufficient for relating T and P correctly or it may involve developing new RTs. Second, however, it could be that the task of establishing that T and P are appropriately related is a mystery and that, although there is not in fact a violation of physicalism it is not within our cognitive reach to know this

³² See Hellman and Thompson (1975, 1977).

or to show it. How to make such a move without its being just a cheap defense of a position in the face of serious difficulties is not at all clear. But, the idea that we should keep trying to solve recalcitrant problems rather than opt for easy and premature rejections of either T or P is an important one.

Finally, it has occurred to some that it is physicalism that is the trouble maker in the situation described above and it should be abandoned as a not very viable program in science. Goodman has declared the program to be pretty much of a failure; while, as we have seen, Chomsky has declared the program to be trivial, insignificant and irrelevant. Putnam appears to reject physicalism as not correct for reference, although it is not clear whether he is saying that the doctrine is irrelevant, false or utopian.³³ Much of the burden of the current project is to show how many of the objections to physicalism fail and how the conception of the program might be altered in a way that meets the other objections but retains the significance of the program for science. The claim that the program, however conceived, is a failure, is at best controversial and surely quite premature.

³³ See Putnam (1975). It is now apparent that Putnam rejects physicalism as a monopolistic doctrine that purports to resolve all problems of underdetermination in our knowledge; construed this way, as a brand of "metaphysical realism", it is clearly unacceptable. We do not view physicalism in this way.

Given this range of options, let us look at the situation regarding semantics to see how they apply. Translations between languages, reference schemes for languages, content assignments for mental states, etc are not, according to Quine, determined by physical doctrine; on our view, this means that, given the physical basis for doctrine and given the realization theories for the attributes involved, there is not a unique translation³⁴ compatible with them (i.e., there are multiple translations that are compatible with physical doctrine and the realization theories). As noted above, Quine has opted for the rejection of translation on the grounds of its failure to satisfy physicalistic conditions for objectivity in science. Putnam also appears to take this option. The idea that physics needs revision in the face of this claim has not to my knowledge been seriously entertained, although it is a possibility that Hellman and Thompson, Chomsky and others would entertain under some circumstances: what those circumstances are is quite difficult to pin down. According to Goodman, the situation suggested probably does not seriously arise (although he has other grounds for questioning semantic phenomena) since physicalism appears to be a bankrupt program: the failure of a well established area of knowledge to fit into a physicalist scheme provides additional grounds for rejecting physicalism. So, if translation were to be established firmly on the basis of other scientific criteria, then the program, rather than translation, would be found lacking.

³⁴ We shall focus on translation although the points apply to all the attributes and theories mentioned.

Thus, there is some feeling that translation ought to be rejected from science, no serious feeling that physics ought to be rejected, and some feeling that physicalism ought to be rejected. The final alternative, i.e., to hold off on any rejection notices and to explore more deeply the nature of the connections between physical and semantical phenomena, has been endorsed by Field and Friedman: it seems clearly the best course. Their view³⁵ appears to be that if we keep on working we will eventually see that, appearances to the contrary, translation is determinate; what we need to develop is the right realization theory for translation and then to see how, given that theory, translation is determinate.³⁶ As noted above, there are two ways of pursuing this line: (1) the RT is correct and it is a matter of seeing how the physics and the RT determine the translation, (2) the known RTs are not correct and it is a matter of finding the right RT and then establishing the connections. Field and Friedman appear to adopt (1) while (2) appears to be more appropriate to the situation. Notice also that the other line in the wait and see strategy is to consider the possibility that we have a mystery on our hands: meaning and reference could fall under that heading. Hence, it could be that, although we will not ever understand it, translation is determinate.

³⁵ See Field (1975) and Friedman (1975).

³⁶ Friedman does not put things quite this way although they are suggested by what he says; his primary failing is to not appreciate the importance of realization theories in these issues.

The situation with respect to translation appears to be something like what Sylvain Bromberger calls a "B-predicament", roughly a situation in which every possible answer that we can think of to a given question can be refuted on the basis of our background knowledge and the constraints imposed on correct answers to the question. In this case, we are asking, "What is the nature of translation?". The constraint imposed is that, given the nature of translation (i.e. given an RT for translation), translations between languages are physicalistically determinate; at present, every answer offered fails to be acceptable. It is unclear whether this is because one of the known RTs is correct and translation really is physicalistically indeterminate or because we have not solved the problem of discovering a correct RT for translation or because the nature of translation is a mystery forever beyond our epistemic reach. The "wait and see" strategy has the liability that one never knows if one is waiting too long.

At the present time, interest has flagged to some extent in the issues concerning the physicalistic standing of translation, partly because we have run out of things to say, partly because interest has turned to other issues concerning reference and mind and partly because we have adopted the wait and see approach. The detailed work required for constructing linguistic, biological and psychological theories leaves little room for speculation about indeterminacy. However, although it may not be appropriate now to worry about such matters, with increased understanding the issues will eventually come to the

fore: at that time a more definitive statement regarding the four options cited above may be possible. In preparation for that time, we now turn to a discussion of the kinds of considerations relevant for choosing among them.

To deepen our understanding of the methodological situation that physicalist principles can give rise to, we shall review, first, some of the considerations that are pertinent to deciding whether or not such a situation has in fact arisen and, second, some considerations pertinent to how such situations might be resolved. Because physicalist doctrine constitutes a description of a "goal state" in science, the issue of when to apply such a description during the activity of doing science is important: it may not be appropriate at every stage of scientific growth to apply principles that characterize an ideal state of development. Thus, it is important to clarify when such application is or is not appropriate. And, further, as we have seen, even if it is appropriate, it may be quite unclear in given situations what to do when conflict exists.

First, the methodological conflict envisioned above should be taken seriously only when the theories, T and P, are "reasonably mature"; it is a notoriously difficult problem in current philosophy of science to make this notion precise, but the notion is not without content.³⁷

³⁷ The development of such notions as that of a working hypothesis, a "paradigm", a research program, and, the development of tools for representing and assessing research programs will contribute to adding content to the idea of a mature science.

Second, no real assessment of the physicalistic connections between branches of science can get off the ground without extensive study of the "inter-branch connections" (i.e., study of how T and P relate to each other). The importance of the development of "interfield" theories as research areas in their own right is only now coming to be fully appreciated. As we have suggested, the formulation of realization theories and of theories of lower level mechanisms that underlie higher level processes and functions is crucial to the full development of physicalist science.

Third, there should not be either any good grounds for believing that the study of the connections between T and P is beyond our cognitive reach or any good arguments for the indeterminacy of T relative to P (e.g., arguments like Quine's, but good arguments). That is, there should not be any good reason for believing that the issue is not resolvable or that it is closed.

Fourth, a clear understanding of how physicalism works should be developed (the point of the current project) so that premature and inappropriate judgements of "non-reducibility" are blocked. The recent discussions in the philosophy of science have been seriously marred by a failure to appreciate the nature of physicalism in contrast to classical reductionism. This is really to say that we can employ the theses in their methodological role only if we are clear on what they require; not an outrageous demand, but one which, in more or less subtle ways, is sometimes not honored.

Fifth, it is appropriate to consider physicalism as a serious constraint on scientific development only if there are good reasons for pursuing the program (i.e., it is a motivated program) and only if there are grounds for believing that it can be worked out.

The above five conditions are quite minimal with respect to the appropriateness of taking seriously the physicalistic acceptability of inter-theoretic relations in science; but, it is obvious that much debate in recent years concerning the indeterminacy of semantics as well as issues concerning reduction in biology and psychology have failed to honor them. Given these conditions, the conclusion of the last section that the "wait and see" approach is superior to the other options, in the case of translation, receives additional support because: (i) the relevant higher level theories (i.e., linguistics, psychology) are immature, (ii) the theory that meaning is just "stimulus meaning" is unacceptable, (iii) there is some reason to believe that meaning and translation may be conceptually beyond our reach,³⁶ (iv) research based upon a clear conception of physicalist demands has rarely been done, and (v) the evidence on the justification of pursuit of the program has not been evaluated.

³⁶ We are surely in a "B-predicament" with regard to the question "What is meaning?"; all the known possible answers are unacceptable.

Let us now assume that the methodological situation described above has appropriately arisen; what considerations are pertinent to its resolution? First as we have discussed above at length, any modifications made to physics (i.e., option 2) ought not to be simply ad hoc. The revision of physical theory so as to establish a physicalist relation between the relevant branches of science, must be accomplished in a way which preserves the integrity of that body of theory and which is appropriate to the questions addressed in the research program of physics.

Second, if option 1 is pursued (i.e., modification of T), then principles of conservation ought to be adhered to: i.e., modification or rejection of T ought to leave no large gaps in scientific knowledge that are not fillable (or for which strategies for filling them are at least conceivable). In the case of mentalistic psychology the wholesale rejection of representational theories of mind would, at the current stage of development, constitute an unacceptable scrapping of what Fodor is fond of calling "the only game in town". The alternatives to such theories are either incoherent or unacceptable on other grounds.

Third, a serious decision regarding the status of realization theories for attributes must be capable of being made.³⁹ At present, with

³⁹ For example, questions of the form "Are the available RTs for attribute A correct?" and "Are we in a B-predicament with regard to RTs for A?" must be assessable.

regard to semantics, both Quine's behavioral theory of meaning and the popular "projective theories of interpretation of mental states"⁴⁰ are unacceptable on grounds that are independent of the issues concerning physicalism. Further, functionalist and "physicalist" realization theories with respect to the nature of mind are likewise unacceptable. The significance of these facts however is not clear. An adequate resolution of the methodological conflict depends upon how such facts are understood (e.g., that we currently do not have a coherent and acceptable conception of meaning and mind; that there are mysteries or unresolved problems regarding the nature of meaning and mind, etc). If, for example, it is decided that the nature of meaning is simply an unsolved problem, then a "wait and see" approach would make most sense. However, if it is decided, following Quine, that a behavioral theory of meaning is the only one we are likely to be able to develop, then the indeterminacy arguments will go through and linguistics and psychology will be ultimately rejected from natural science.

Fourth, the rejection of physicalism should not leave a large gap in our understanding of the nature and goals of scientific activity. As we have been at pains to emphasize, physicalism provides a set of

⁴⁰ See Dennett (1971), Putnam (1975), Stich (unpublished), Quine (1960), Lewis (1973), and Davidson (1970, 1973). Such a theory of interpretation has it that, unlike hypotheses in physics, biology, etc, an interpretive hypothesis is not an empirical hypothesis concerning the states of an individual. But rather, it is a "rationalization" of the person's behavior in relation to the environment which is sensitive to the interests, goals and cognitions of the rationalizer in ways that normal empirical hypotheses are not.

principles that capture a view of nature and the structure of science that is deeply ingrained in most scientist's understanding of science and its object of study; such a view constitutes a deep understanding of how phenomena relate to each other and how theories of those phenomena ought also to be related to each other. To reject this view, and it is conceivable that such could be done, would be to leave a large gap in the scientist's and the philosopher's understanding of what science is about. At a minimum some alternative conception should be available which is a serious alternative; without such an alternative, it is not clear that pursuing the old program is not better than not having a program.⁴¹

Fifth, in deciding on a course of action, the justificatory status of the program to date should be understood. That the status is poor is Goodman's claim; whether it is or not now is unclear. But, it is true that if it is poor, then the option to reject physicalism is more plausible. This is the obvious point that, in considering a range of options, the epistemically least valued one is, other things being equal, the first one to be rejected. As we observed in the last consideration, other things aren't always equal.

⁴¹ There are two ways of construing these issues: first, what ought to be our conception of science?, second, what is the scientist's actual conception of science? In either case, the burden of rejecting physicalism is to provide some alternative understanding of science that is better than the physicalist understanding. Those who have advocated rejection of physicalism have been quite cavalier in doing so, paying little attention to this problem. It is almost inconceivable to many scientists that physicalism should be rejected; this in itself places a burden on the opponents of the program.

The above sets of conditions on appropriateness and resolution are offered to convey just how complex the issues are that are raised by the methodological employment of the theses. That the theses have been so used is undeniable, but the point here is that such use must take place under certain circumstances only, and even then, the outcome of their use is subject to complex considerations that presuppose much auxiliary knowledge. It is pretty safe to say that many actual uses of the theses in recent years have been inappropriate and untutored. Further, at the present time, the situations of appropriate use may be severely restricted given the manifest failure of many sciences to satisfy the conditions of appropriateness and given the notable lack of much of the needed auxiliary knowledge required for resolution of conflict situations. Nonetheless, the idea that such methodological employment ought never occur seems to be seriously mistaken; and it is to this final issue regarding (R2) that we now turn, briefly.

As we have discussed above, there are at least three kinds of grounds for taking seriously the methodological employment of physicalist theses: (i) past fruitfulness of such use, (ii) heavy empirical support for their truth, and (iii) the promised gains if their use is successful. These grounds would be sufficient for taking the program seriously; and, relative to the conditions of appropriateness, the theses ought to be used if the program is taken seriously. But, as we have seen, although (iii) is not problematic, (i) and (ii) are. The reasons for taking the theses as true have serious problems as we have

seen and the past fruitfulness of their use is the subject of debate regarding both how much they have been used and how that use generalizes to other domains. It is also of some concern whether or not the theses are usable: e.g., it is not clear how the thesis of truth determination can be effectively used if stronger reductive theses are false. The most we can say at this point then is that if the program is accepted, then the theses ought to be used when it is appropriate to do so; the issue of acceptance of the program, however, is still quite problematic.

This concludes our discussion of (M3). That the theses are supposed to play methodological roles in science is clearly part of the physicalist doctrine; the issue of whether they ought to play such roles in science comes down to whether the program is operating within science. The main goal of our project is to provide an adequate formulation of the doctrine and to defend that formulation against objections; if we are successful in this, then a large step toward the acceptability of the program will have been taken. The remaining steps involve the working out of the program in science, and assessing the degree to which it is successful. But, as we have seen, there have been a number of objections that have not been satisfactorily responded to (e.g., indeterminacy, empirical status, utopianism, scope); in the next section, we shall round out our discussion by making a proposal for how these objections can be plausibly dealt with.

5.4 PROPOSED MODIFICATION OF THE METATHESES

Our discussion to this point has left us with the following catalogue of difficulties for the physicalist doctrine we have formulated: (1) the scope of the theses is not clearly and non-circularly defined, (2) the theses are not clearly empirically testable, (3) the theses are utopian in character, (4) the theses, concerning semantical and explanatory relations, may not be acceptable natural science for physicalist reasons, and (5) the constraints on the physicalist bases do not guarantee that non-physical entities (e.g., mental entities) will not be in the bases.

Our problem in this section is to show that each objection is either true but not a problem or false. In doing this, we shall reexamine the kind of program physicalism is and challenge a basic assumption that most physicalist philosophers have made: viz., that the theses of physicalism are empirical theses of natural science (i.e., (M2)). Our strategy for dealing with the objections will be to reject (M2) and offer an alternative construal of the theses; our main problem will be to do this without becoming committed to some unacceptable bifurcation in our knowledge (e.g., the analytic-synthetic distinction).

With the exception of some suggestive comments made by some physicalists that the theses are "regulative ideals" in science, the majority of physicalists have appeared to view the theses as empirical

theses of natural science. This, as we have seen, is a claim with a number of problems: (i) the identification of confirmatory and disconfirmatory empirical arguments is fraught with difficulties, (ii) the theses may be self supporting insofar as they are used as methodological principles, (iii) the theses may be in violation of their own prescription of what counts as acceptable natural science. The other metatheses, viz., that the theses apply to natural science and that they play a methodological role, are crucial to capturing the intent and motivations of the program; thus, their rejection would constitute giving up the point of being a physicalist at all. This, we contend, is not true of the alleged empirical status of the theses. So, if it is possible to modify (M2) while retaining (M1) and (M3), in such a way as to fend off the objections to the program, then the original formulation would incur minimal losses while overcoming major obstacles to the acceptability of the program. It is this line that we propose to follow. We shall begin by showing how rejection of (M2) provides a means of responding to most of the objections listed above.

Let us, then, reject the idea that the theses of physicalism are empirical theses of natural science while retaining the ideas that they characterize natural science and that they play the roles (R1) and (R2) in the conduct of natural science. Thus, we might, as has been suggested, describe the theses as "regulative ideals" for natural science which structure scientific knowledge and our scientific view of nature. What happens to the objections discussed above?

First, regarding the scope objection, the theses of physicalism become part of whatever set of principles there are for delineating the boundaries of natural science; it is, on this view, constitutive of what natural science is that it and its object of study be structured by physicalist principles. Appearances to the contrary, this does not trivialize the theses of physicalism by any means: it is entirely possible that science cannot both be structured by physicalist theses and succeed in its other goals. Such a development, were we sure that we were presented with it, would be grounds for reconsidering whether or not we wanted to be physicalists. What the proposal does mean is that, although a complete characterization of natural science has not been given, this is no longer a concern for physicalism: whatever natural science is, it satisfies the theses (i.e., they are necessary conditions for anything being natural science.) The current proposal does not have the untoward consequences that the empirical status and correct methodological employment of the theses are threatened by circularity or ill-definition of the scope of the doctrine. Thus, rejection of (M2) frees us of the need for independent specification of what natural science is in order to assess and use the theses.

Second, with regard to the utopianism objection, it is certainly true that physicalist theses pose difficult, possibly too difficult, problems for scientific inquiry. As empirical theses this might appear to put assessment of their truth out of reach and thus make their acceptance quite speculative. But, with this much said, what is the

force of the objection, given that we have withdrawn empirical status from the theses? Physicalism does appear to be a very demanding master for scientific inquiry; but, is this an objection? We think not; rather, it means that science, on a physicalist construal, is faced with very difficult but potentially quite rewarding problems if they can be solved. What we have argued above is that anything less than the physicalist program as we have construed it could lead to a much inferior brand of science. In this regard, the onus is very much on those who would reject physicalism in science in favor of a much weaker alternative conception of how science is structured.

Third, the objection that physicalist theses could be unacceptable natural science on its own terms arose because (1) the theses concern semantical and explanatory relations between different parts of science, (2) such relations may not be capable of being "physicalized" and (3) the theses concerning them may not be truth determined by the the physical truths. The problem here can be conceived of as either the doctrine's entailing that semantic and explanatory relations and theses are physicalistically acceptable or the doctrine's potentially playing a role in its own rejection. Neither prospect is part of the

⁴² It surely is not intended by physicalists that the doctrine can succeed in being acceptable only if semantics and explanation are physicalistically legitimate parts of natural science; most current physicalists appear to view the fate of physicalism and semantics to be quite independent. The point here is that this isn't possible if physicalism is part of natural science and hence applicable to itself.

intention of the physicalist.⁴² It strikes us as just a mistake⁴³ to construe the theses as a part of natural science and, hence, applicable to themselves. Quine, especially, is faced with tension in his views here: he cannot jointly maintain that (i) physicalism is an empirical thesis of natural science, (ii) semantics is physicalistically indeterminate, and (iii) physicalism plays the methodological role, (R2), and hence provides the grounds for rejection of semantical claims from science.

Our suggestion⁴⁴ is to reject (M2); as a consequence, neither of the construals of the current objection arise. And, thus, it can be maintained that the theses play a role in the structuring of scientific knowledge without requiring, what is at the least highly problematic, that semantics and explanation be incorporated into physicalistic science as part of its subject matter.

The final objection affected by rejection of (M2) concerns the difficulties associated with the empirical testability of the theses. These, obviously, are not problems for the view that the theses are not empirical theses. However, it does raise the question of what the measures of acceptability of the theses are, and, whether or not those measures are in any interesting way different from those involved in

⁴³ A mistake that, for some, was born of the fear of creating untenable divisions in our knowledge.

⁴⁴ A suggestion that does not take Quine off the hook entirely since he maintains the "monolithic" character of our knowledge.

viewing the theses as empirical. We shall return to this and related issues below.

Having pointed out how most of the outstanding objections to the doctrine can be fended off by rejecting (M2), we now turn to a discussion of the issues that arise concerning our reconstrual of the nature of physicalist theses. As we have indicated, our proposed alternative to (M2) is:

(M2') The theses are regulative ideals for natural science.

The question that immediately arises is, "what is a regulative ideal?". In the present context, a regulative ideal (RI) is a principle which structures our scientific representation of nature, but which is not itself a part of that picture. Alternatively put, it is a part of the metatheory for scientific knowledge. Thus, for example, the claim that the physical truth determines all the truth in science is a claim about scientific knowledge but is not itself part of that knowledge.

With regard to the regulative character of RIs, such principles describe goal states of knowledge in science (e.g., our ultimate characterization of the intended interpretation of scientific knowledge ought to exhibit all attributes as supervening upon the physical attributes). As such, RIs play an explicit regulative role at appropriate times in the conduct of scientific inquiry.⁴⁵

⁴⁵ This does not mean that RIs are not always in force; it means that their useful application can only take place under certain condi-

The above two considerations strongly suggest the RIs are not appropriately viewed as being true or false as much as successful or unsuccessful. As part of the metatheory of science, they can be viewed as true of science; but at the level of scientific knowledge, they are not part of science and do not constitute scientific truths. Rather, they are claims about how we are going to structure our knowledge of the world; thus, the issue of assessment of such claims is more along the lines of whether knowledge so structured is forthcoming: i.e., Is the program, the goal of which is to construct knowledge with this structure, successful or not?

Thus, RIs are not to be viewed as a priori truths nor truths by convention; they stand apart from our scientific theory of nature. Nor should they be thought of as instances of Carnap's external framework principles; his commitment to the analytic character of such claims is not in the spirit of the current proposal. RIs are not analytic truths; they are principles which structure a representation but which are such that, if they are not satisfied, no logical violation or nonsense has been committed.

On the current proposal, we are distinguishing RIs from the empirical knowledge that constitutes the natural science to which they apply. This distinction is not based upon the analytic-synthetic distinction at all: the theses are not construed as analytically true.

tions that we have specified above.

Further, we are contending that the theses are not to be construed as part of scientific knowledge that is "highly central" within the system of such knowledge. Our argument against this specific interpretation is briefly stated as follows: if the theses are part of natural science, then it follows that the semantical and intensional notions employed in their formulation are physicalistically acceptable; but, this is surely unacceptable to all "physicalists" for whom it is an empirical matter whether any non-physical notions will fit into a physicalist body of knowledge. The only way to avoid this difficulty is to not include the theses among the claims which are subject to the requirements formulated by those theses. Hence, for physicalists who reject semantical and intensional notions from science as well as for those who think it an independent matter whether such notions are acceptable in science, the view that the theses are highly central parts of our corpus of scientific knowledge is clearly unacceptable.

What we are seeing is that, if we are going to be physicalists, then we cannot hold to the view that there are no interesting cleavages in our knowledge. The notions of analytic, a priori, and conventional do not exhaust the possibilities for the kinds of principles that may be distinguished from the claims constitutive of the empirical knowledge of natural science. We suggest that what we are calling "regulative ideals", which are not analytically, conventionally or a priori true parts of our scientific knowledge, but which stand apart from that knowledge as principles concerning its ideal structure, pro-

vide another alternative.⁴⁶

In summary, what we have done in this section is to reject (M2) in favor of (M2') with the result that four of the five outstanding objections to the doctrine we have formulated are defused. The remaining objection, i.e., that "non-physical" entities may appear in the physicalist bases despite all the constraints imposed, will be discussed further below. A consequence of our discussion is that physicalists must seriously reexamine the idea that there are no interesting epistemic bifurcations in our knowledge; more specifically, the distinction between empirical truths of science and regulative ideals for science is a motivated one and deserving of a serious role in the philosophy of science.

5.5 SUMMARY AND CONCLUDING REMARKS

To this point, the principal steps we have taken are as follows:

1. In Chapter 1, we clarified the central motivations of the physicalist program as it has appeared in recent years;
2. In Chapter 2, we reviewed and rejected many of the extant formulations of physicalist doctrine on the grounds that they are not sufficient for realizing the motivations of the program;

⁴⁶ As we indicated above, we are not saying that there are no empirical issues associated with RIs; there are such issues concerning the success or failure of programs based upon them. What we are saying is that it is important to distinguish RIs from the empirical truths formulated within a system of knowledge structured by them.

3. In Chapter 3, we addressed the problem of the principled identification of the physicalist bases and we formulated three presuppositions of any adequate formulation of physicalist theses; the problem that the bases could include paradigmatically non-physical entities (e.g., mental entities) was not fully resolved, although all other objections were seen to be harmless;
4. In Chapter 4, we formulated a set of physicalist theses which were designed to avoid many of the sources of inadequacy found in previous formulations; the objections that the theses are "utopian" and that they may be unacceptable on their own terms (i.e., being concerned with semantics and explanation, they may exhibit indeterminacy or some other failure to satisfy physicalist demands upon acceptable science) were not completely defended against;
5. In Chapter 5, we have formulated a set of three metatheses that express the "Received View" among physicalists concerning the nature of physicalist theses; but, in the light of the three objections discussed in earlier chapters in addition to two further objections (i.e., the scope of the theses is ill-defined and the empirical status of the theses is doubtful), we rejected the claim that the theses are empirical theses of natural science in favor of the claim that they are "regulative ideals" that stand apart from the body of scientific knowledge;

as a result, all but the objection concerning the bases were defused.

In closing, we shall consider three final issues: (1) the alleged monopolistic character of physicalism, (2) the problem, raised in Chapter 3, regarding the contents of the physicalist bases, and (3) the status of the program with regard to its acceptability.

First, we have emphasized throughout this project that physicalism is not a "monopolistic" doctrine purporting to resolve all cognitive conflict or to determine all aspects of our knowledge. The view we have taken is that physicalism is, roughly, an "architectural" doctrine concerning the structure of the formal system of science and concerning the structures that provide interpretations for that system. As such, physicalism has a limited scope of application, being restricted to natural science. Further, this brand of physicalism does not purport to be an instance of what Putnam has referred to as "metaphysical realism": i.e., it does not purport to provide an account of physicalistic science as constituting "the" true theory of the ultimate nature of reality. Rather, to borrow Goodman's term, science is a "version" of reality; according to physicalist doctrine this version is structured in certain ways which, as we have argued, have a number of epistemic rewards.

It is true that physicalist science may also attempt to provide an account of semantics and of how representations relate to what they

represent; but, this should not lead one to think that science may ultimately become metaphysically realistic in character. All such theorizing occurs within science and in its own terms; hence, even if such theorizing is successful, it can never provide an account that would succeed in fixing exactly one interpretation of the language of science and it can never be more than just a version among many possible ones which compete for our attention and use on the grounds of success in achieving cognitive and non-cognitive goals. Thus, a successful physicalist account of semantics and explanation would provide us with an account of how our theories work and how they attach to the world; but, such an account would be an "internal" account. It would not, under any circumstances, warrant the attempt to parlay such success into a completely general account that would resolve all cases of underdetermination in our knowledge (e.g., those cases encountered in chapters 3 and 4) and that would uniquely fix all interpretation of languages: a physicalist account of semantics and explanation can only get off the ground after such issues have been resolved on non-physicalist grounds.⁴⁷ Thus, it is not conceivable, on our view, that physicalism could ever become a completely monopolistic doctrine in the ways that Putnam and Goodman charge some physicalists with dreaming

⁴⁷ We note, as we have pointed out earlier, that theorizing about semantics and explanation in physicalist science may fail because such phenomena are understood to be radically different from the phenomena studied in science or because understanding such phenomena is just too difficult. Physicalist doctrine has been located outside of natural science to allow for the possibility of such failures while still leaving room for us to be physicalists about science.

about.⁴⁸

Next, we take final note of the deep significance of Chomsky's objection regarding the principled identification of the physical bases for our construal of the physicalist program. It must be allowed that, given our essentially "architectural" approach to physicalism⁴⁹ and given our strategy for characterizing the physical bases,⁵⁰ there is room for a certain development in science which is unacceptable to the spirit of the physicalist program: i.e., that certain entities that physicalists have always been concerned to keep out of the bases (e.g., mentalistic entities, semantical entities) could be in them. This is because the only demands we have made on what is in the bases is that something play a motivated role in theorizing in the research program of physics; thus, a mentalistic construct might find its way into physics in the same way that electromagnetic constructs did.⁵¹

⁴⁸ See Goodman (1978), Putnam (1979).

⁴⁹ That is, our taking physicalism to prescribe certain structural relations for an ideal formulation of scientific knowledge.

⁵⁰ That is, characterizing a research program for basic physics and developing the bases in terms of a theory which satisfies the characterization and is empirically acceptable.

⁵¹ We have argued that this is not achievable in as easy a way as Chomsky suggests, although we do allow that it is possible; and, that, of course, is problem enough for the program. What we have opposed is that the boundaries of the physical bases are completely malleable and arbitrary subject to socio-historical forces or ad hoc modification by disparate physicalists.

We agree that this is an unacceptable possibility for the physicalist program where 'physicalist' retains its classical content. There are two possible courses of action at this point: first, one could reject the classical motivation of the program to keep out of the bases certain entities, perhaps on the grounds that the bases are now so unrecognizable as "physical" in any traditional sense that there is no need to be concerned about the contents of the bases. What is important, on this view, is that physics is "basic science" no matter what turns out to be a motivated component of its theories or domain. Thus, physicalism is just an architectural doctrine concerning structural relations between all branches of science and the basic science. A second course one could pursue, however, would be to preserve the classical motivation by introducing further conditions of adequacy for formulations of physicalist doctrine: e.g., that mentalistic, semantical, biological and spiritual phenomena not be elements of the bases. This move has the virtue of preserving the classical ontological significance of the program, but at the cost of being ad hoc. We do not propose to choose among these options in the current project, although our inclination is toward the first (i.e., to view physicalism as a purely structural doctrine).

In closing, we shall briefly discuss the status of the program with respect to the issues of acceptability. In this regard, the following objection raised by Goodman and Putnam is pertinent: the physicalist program is like the phenomenalist program in that both are worthwhile

because they have important goals but both are bogged down in unfulfilled promises that do not appear to be likely of fulfillment. We agree that both have important goals and observe that they are different: the phenomenalist program is epistemologically motivated, while the version of physicalism put forth here is concerned with ontology, objectivity and explanation. However, we disagree with the claim that the two programs are equally bogged down and equally unlikely of success. It is quite clear that, whereas the phenomenalist program is not part of current scientific practice and thought, the physicalist program is; the scientific exploration of the physicalist basis of all phenomena studied in science is, at least from the point of view of the scientific community, a live project. Finally, both programs are constructional in a broad sense, although as we construe physicalism, the criteria of correctness of constructions are quite different in the two programs.⁵² How this difference effects the relative prospects of success is not clear. Our tentative conclusion is that there are enough differences between the physicalist and phenomenalist programs that the simple analogy suggested by Putnam and Goodman is of little value. Further, since the actual assessment of the success or failure of the physicalist program in science has not been conducted by any-

⁵² It might be argued by Goodman and Putnam that any criteria employed by the physicalist is available to the phenomenalist. However, as a matter of fact no phenomenalist has employed the kind of criteria of correctness we have employed in our version of physicalism; and, it is not at all clear that, for example, the notion of a realization theory is a coherent one for a phenomenalist constructional system.

one, it is hard to take seriously the contention that it is failing as badly as the phenomenalist program.

Consideration of the acceptability of the formulation of physicalism presented above reveals that we have an adequate formulation with respect to which there is one objection that cannot be readily defended against; we have left it open whether this objection is ultimately devastating to the formulation. The development of detailed information that would allow an assessment of the likelihood of success of the program is, as we have observed at various points, lacking. We take this to be, not a measure of a failure to fulfill promises, but as indicative of the immaturity of many sciences, especially the inter-field disciplines. Thus, we cannot now say how likely the program is of success. It is the development of such information that is the next step in the process of assessment of acceptability.

With regard to the actual working out of the program in science, work in biology, psychology and linguistics/semantics is especially critical. It is clear that the current project has not solved the problems traditionally thought to stand in the way of the success of physicalism: namely, the problems of finding a place for life, mind and meaning in a physical world. Fortunately, this was not the goal of the current project. Rather, the version of physicalism put forth in this project construes those problems in a somewhat different way: namely, the problem facing physicalist science is to determine the

boundaries of knowledge of a certain kind (i.e., knowledge of the kind that satisfies physicalist principles). The issue is, how broad is physicalist science? Unlike the older distinction between the natural sciences and the social sciences which was based upon differences in method, the current distinction is based upon ontology, objectivity and explanation in relation to the physicalist basis. The goal of the physicalist program is not to subsume all that is legitimately called knowledge under physicalist principles; rather, it is to delineate a body of knowledge that hangs together in a certain way and yields a particular kind of understanding. That this goal is worthwhile is beyond dispute; whether it can be attained is beyond our current capacity to say. Nonetheless, this kind of understanding is prized and sought after widely in science and the project of pursuing it should not be abandoned in haste.

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