PERSONS AND PLACEBOS:
PHILOSOPHICAL DIMENSIONS OF THE PLACEBO EFFECT

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Joseph F. Hanna
Major professor

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ABSTRACT

PERSONS AND PLACEBOS: PHILOSOPHICAL
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By

Howard Brody

The placebo effect occurs within medicine when a patient improves after being given a chemically inert substance, under the impression that he has been given an active drug. Although this phenomenon has been studied extensively by medical scientists recently, little attention has been paid to it by philosophers. Three philosophical issues deserve exploration: 1) the meaning and breadth of the term 'placebo effect'; 2) implications of the placebo effect for the mind-body relation; and 3) ethical problems regarding the clinical use of placebos. Investigation of these issues will proceed in an interlocking fashion, so that each sheds additional light on the others.

A formal definition of the placebo effect may be approached by first reviewing the medical literature, and then analyzing the empirical data through the use of illustrative case examples designed to indicate the boundaries of the term's applicability. This procedure yields a definition with several important features. First, the placebo effect may be present even where no placebo has been used-- in particular, we may wish to speak of a placebo component accompanying the administration of biomedically active therapy. Also, two important references must be made to belief states. One is the belief state of the individual, as the placebo effect can meaningfully be said to occur only if the individual believes that he is in a healing setting receiving some sort of

therapy. The other is the belief state of medical science, as economy of explanation requires that we do not attribute a symptom change to the placebo effect if it is explainable on the basis of some other well-accepted medical theory.

The importance of the individual's belief state points out the mind-body implications of placebo phenomena. Many theories falling within the framework of Cartesian dualism, including causal interactionism, behaviorism, identity theory, and eliminative materialism, can give plausible accounts of the placebo effect; but weaknesses in all of these theories can be found on other grounds. The first three theories have serious flaws that have been well characterized by traditional philosophical arguments; eliminative materialism becomes less plausible when it is seen to involve a radical change in our form of life, one that would involve among other things the loss of any basis for moral reasoning.

An alternative to the Cartesian tradition, however, is to take the concept of person rather than the concepts of mind and body as the primitive term. A theory can be developed which treats being a person as being an animal with the capacity to use symbols in such a way that the symbols acquire meaning through the use. The capacity theory of person gives an illuminating account of the placebo effect, squares well with considered judgments about other mind-body issues, and in particular emphasizes that to be a person is necessarily to be a dweller within culture and language, a facet of human existence not adequately dealt with by other mind-body theories.

The ethical problems, the third issue area to be discussed, reduce in large part to the justification of deception in medical practice. Historically, many physicians have attempted to justify placebo use, but both deontological and utilitarian arguments can be used to establish a *prima facie* presumption against the deceptive use of placebos. The formal definition, by showing that one need not give a placebo in order to elicit a beneficial placebo effect, points the way toward alternative, nondeceptive ways of securing the benefits of the placebo effect for patients.

These philosophical discussions suggest the utility of the model of the placebo effect which takes its crucial feature to be the imposing of meaning upon the patient's illness experience; this model in turn suggests several interesting lines of empirical inquiry. Finally, by touching in a mutually supportive way upon the empirical-conceptual, the metaphysical, and the normative realms, all of which are bound up in the theory and practice of medicine, this investigation of the placebo effect provides a possible model for the discipline of philosophy of medicine.

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Introduction. Philosophical Dimensions of the Placebo Effect

Physicians have known for at least several centuries that patients often display marked improvement of symptoms when given a sugar pill, or another substance having no known medicinal properties, under the impression that it is an active drug. A biomedically inert substance given in such a manner to produce relief is known as a placebo, and the resulting influence upon the patient may be called the placebo effect. With the advent of large-scale clinical trials of drugs and therapeutics within the last three decades, placebos have become an important way of eliminating investigator bias in medical research design. As a result, a good deal has been learned about the placebo effect in the course of studying other therapies, and this in turn has stimulated study of the placebo effect directly. Investigators, as they have come to learn more about psychosomatic medicine and about psychological and social determinants of disease, have made some attempts to develop a comprehensive psychophysiological theory capable of explaining the placebo effect.

These developments in medicine give rise to three major philosophical issues. First, physicians have not devoted much attention to defining 'placebo effect' rigorously, and to delineating sharply the sorts of phenomena to which it is intended to apply. This task is a difficult one because much remains to be learned about the placebo effect.

On the one hand, one wants a definition specific enough to serve as a helpful guide in laying out research strategies. On the other hand, one wants a definition general enough so that it does not treat matters which ought to be settled empirically as conceptual issues to be settled *a priori*. Several questions in philosophy of science, including what is to count as an adequate explanation and how one establishes the truth of scientific theories, must be taken into account in seeking this balance.

Second, what is known about the placebo effect suggests that, in some way, a patient's beliefs or expectations can influence his bodily states. This appears to have implications for the relationship between mind and body; some philosophical views of the mind-body relation may allow for such a connection while others may not.

Third, the actual use of placebos by physicians in therapeutic encounters raises ethical issues-- specifically, issues within that area of normative ethics which is becoming known as the subspecialty of "medical ethics."

Each of these three sets of issues can be dealt with in straightforward fashion by the philosophical approaches already mentioned. We might arrive at a formal definition of 'placebo effect,' for example, by looking at how the words are actually used by investigators in the field, or by looking at the operational methods employed to measure it. We might adapt an existing mind-body view, such as Cartesian causal interactionism, to give an account of the placebo effect in mind-body terms. And we can handle the ethical issues within either the utilitarian or the deontological framework, as a subcategory of cases involving deception. In this way all the issues might be settled, each

in isolation from the others.

However, it is both more challenging and more satisfying to aim for a more comprehensive approach. We would like to arrive at a formal definition of "placebo effect' which not only takes care of narrowly empirical issues, but also illuminates the tasks of investigating the mind-body relation and of framing an ethical argument. And we would like to find a mind-body theory that not only accounts for the existence of the placebo effect, but also helps us to understand it further in terms of its definition and its ethical import. That is, the philosophical accounts given in response to the three sets of issues ought to be not only plausible when taken singly, but also mutually consistent and illuminating when taken together.

But we also demand more. In each of these three issue areas we already have what we may call considered judgments, many of which have little or nothing to do with the placebo effect. In the empirical realm we know, for instance, that a sugar pill has no medicinal properties according to existing pharmacologic theories, insofar as its chemical structure is concerned. In the area of mind and body, we know that we have minds-- that we are conscious of ourselves and our surroundings in a way that trees and rocks are not, and in a way that animals may share to some degree. And in the normative realm we know that it is wrong to torture others for our own amusement. These are the sorts of things we are certain of if we can claim to be certain of anything at all; we are confident that we are not led to believe these things simply because we are confused, or because we are biased by selfish interests (Rawls 1971, pp. 47-48). Therefore, we would be reluctant to adopt a mind-body theory or an ethical stance which conflicted

with these considered judgments, just because this seemed a neat, *ad hoc* way out of a specific dilemma raised by the placebo case. Thus the degree of overall "fit" we ideally aim for among our three philosophical theories and our three types of considered judgments is quite extensive, and the chances are that we will never get a perfect "fit" but only a workable approximation. And even this approximation may be upset by new discoveries, or by new types of cases which cause us to rethink our ethical positions.[1]

We can summarize this approach schematically. Let C and C' be two alternative conceptual theories for organizing the empirical data about the placebo effect. Let M and M' be two alternative metaphysical theories about the mind-body relation, and let N and N' be two alternative normative theories (of moral obligation). Also, let c_1, c_2, ...c_n, m_1, m_2,...m_n, and n_1, n_2,...n_n represent our considered judgments about conceptual-empirical, metaphysical mind-body, and normative matters, respectively, all of which are independent of our beliefs about the placebo effect. Now, if all of the following are true:

1. C and C', M and M', and N and N' are each equally plausible insofar as they are able to explain the relevant features of the placebo effect, taken in isolation;

1. The model for philosophical inquiry that I am employing is an expansion of Rawls' "reflective equilibrium" for determining principles of justice most compatible with our basic moral judgments (1971, pp. 18-22). Rawls in turn cites Nelson Goodman on the justification of principles of scientific inference, suggesting that some related concept of "best overall fit" may be applicable within philosophy of science as well. I take Lakatos' (1970) description of "research programmes" in science to involve a similar equilibrium model; scientists have characterized this model as a cybernetic or a negative-feedback one, as contrasted to the hypothetico-deductive model (Medawar 1967, p. 154).

2. Taken together, (C,M,N) is internally more consistent than (C',M,N), (C,M',N), (C,M,N'), (C',M',N), or any other combination;

3. C is more consistent with c_1, c_2,...c_n than is C', M is more consistent with m_1, m_2,...m_n than is M', and N is more consistent with n_1, n_2,...n_n than is N';

then we would have the strongest possible grounds for preferring C, M, and N over C', M', and N', respectively. Furthermore, even if C were slightly less plausible than C' when applied to the placebo effect in isolation, we might be willing to trade this off against the much greater degree of overall "fit" offered by the set of theories (C,M,N), and the agreement with existing considered judgments on other matters of philosophical importance.[2]

The approach just described arises from a particular preconception of philosophy in general, which also turns out to have important applications to the new subdiscipline of philosophy which is becoming known as philosophy of medicine.[3] On this view, we engage in philosophy in order to find a more intelligible and coherent view of the world, including our own places in the world. Technical precision in philosophy is desirable and advantageous for this and other reasons, but by itself

2. The "equilibrium" must involve changes in both directions, since if our existing considered judgments were never altered to fit attractive general principles, the system would offer little opportunity for growth. We are rather looking for general principles which, if they do not match our most basic considered judgments, "extend them in an acceptable way" (Rawls 1971, p. 19).

3. On the scope of this new subdiscipline see (Pellegrino 1976*a*). An illuminating debate on the possibility and the nature of a philosophy of medicine is found in (Engelhardt and Spicker 1975, pp. 211-234).

precision does not satisfy this basic need for comprehensive understanding which leads us to philosophy initially. And so, where a conflict arises, we may be willing to dispense with a certain amount of technical precision in order to satisfy this need. Furthermore, since knowing our own places in the world requires us to see ourselves as agents rather than always as spectators, the moral as well as the metaphysical calls for a consistent place within the world view we are constructing. We are not content to know about other people and situations in a merely descriptive way; we want to know how we ought to act towards others and in certain situations. In particular, we want to avoid any metaphysical theory such as a crude determinism, however empirically intriguing such a theory might be, if it seems to leave no room for free actions and moral thinking.[4]

This preconception of philosophy seems especially applicable to philosophy of medicine. An increased emphasis on technical precision has taken medicine a long way, but precision has been shown to have its limits, and what reflective physicians have always referred to as the "art" of medicine continues to defy precise analysis. Furthermore, medicine necessarily crosses all of the boundaries that we have tried to draw between the empirical, the metaphysical, and the moral realms. Medicine, above all other fields of study, refuses to let any of us remain spectators for long. The physician cannot merely observe and describe the course of disease; he must intervene actively, in a way

4. This preconception of philosophy is neither new nor original. On the importance of a subjective sense of satisfaction, and of taking moral as well as conceptual elements into account when seeking "fit," see (James 1927, pp. 146-148). The importance of including both metaphysical and moral considerations will be stressed in the discussion of eliminative materialism in Chapter 4.

that has dramatic impact on the rights and interests of other people. And the physician himself is liable at any moment to switch roles and become the patient. When medicine is viewed in this way, the variety of issues raised by the placebo effect is seen to represent in a microcosm the larger sweep of philosophy of medicine. For this reason, our investigation of the placebo effect will touch upon a number of points of importance to philosophy of medicine generally.

The view of philosophy stated above requires some additional comment. The notions of "overall fit" and "equal plausibility," for instance, require considerable amplification if the account is to be defended against possible criticisms. I certainly do not intend the notion of "fit" to be so strong as to suggest mutual logical entailment among C, M, and N. However, I think that the notions of fit and plausibility, as they will be employed below, can be understood intuitively; this at least will allow the account to serve as a rough guide for the investigation. The best way to amplify this intuitive level of understanding is actually to carry out the investigation, and then to enumerate the points of carry-over and cross-fertilization that have arisen. The account can serve as a rough guide to what follows even though all of the steps in the schematic will not be carried out explicitly.

Even with these problems aside, anyone adhering to the present Anglo-American analytic tradition is likely to look suspiciously on any search for a comprehensive philosophical overview in which internal "fit" is stressed. And, indeed, one could cite examples of imaginative and internally consistent philosophical accounts which are no more than meaningless exercises in fantasy. But the serious flaw in such accounts is not the attempt at comprehensiveness, or the value

placed on internal fit, but rather the failure to be grounded at any point on an acceptable base, empirical or otherwise. So long as our attempt takes into account our empirical, metaphysical, and moral considered judgments, there seems little reason to fear that any pernicious castle-in-the-air-building will occur. Some, of course, would insist that the empirical corner of the (C,M,N) triangle ought to be given priority over considered judgments of other types; and they should be happy to note that the emphasis on internal fit can be expected to augment the empirical content of the other two corners.

The body of this dissertation will deal sequentially with the three sets of issues arising from the placebo effect. Chapter 1 will review the empirical findings about the placebo effect from the medical literature. It will list, as a point of departure, various definitions of 'placebo' offered by medical authors. Theories explaining placebo phenomena will also be reviewed. While in no way exhaustively reviewing the available literature, Chapter 1 will summarize the major empirical points which any philosophical investigation will have to take into account.

Chapter 2 will consider these data from a critical standpoint. It will relate the placebo phenomenon more explicitly to the currently dominant theories and assumptions of medical science. Following consideration of a series of examples designed to mark out the boundaries of applicability of the term 'placebo effect,' a formal definition will be proposed. Certain features of this definition, in turn, will serve as a starting point for the other two sets of issues, the mind-body implications and the ethical questions.

The next three chapters will be devoted to the mind-body issue. Chapter 3 will give an overview of the Cartesian tradition in philosophy of mind and will consider three theories arising within this framework, behaviorism, causal interactionism, and identity theory. Chapter 4 will go on to deal at some length with eliminative materialism, which is in some ways the strongest mind-body theory; but while all of these theories are able to give some account of the placebo effect, all turn out to give rise to other philosophical problems when tested against other considered judgments. At the conclusion of Chapter 4, an alternative approach will be suggested, which involves focusing on the concept of "person" instead of the concepts of mind and body. Chapter 5 will then develop such a theory of the person, which attempts to integrate man's biological nature, man's mental states and activities, and man's participation in language and culture.[5] This view, it will be argued, seems more compatible with what we know about the placebo effect than do mind-body theories lacking this scope. In addition, the person theory will be shown to fit in well with other considered judgments about the nature of mind.

Chapter 6 will then address the ethical issues, beginning with a historical review of physicians' use of placebos and the arguments traditionally offered for and against such use. These arguments will be critically reviewed to establish a *prima facie* presumption against deceptive use of placebos. It follows from the formal definition given in Chapter 2 that the placebo effect can occur without administration

5. Throughout this dissertation I will be using the undesirable masculine noun and pronoun forms for purposes of brevity, and also in order to reserve the term 'person' to designate the particular philosophical stance described in Chapters 4-5.

of a placebo; this will suggest a line of argument which places increased stress on alternatives to the deceptive use of placebos.

The Conclusion, finally, will briefly summarize the results of these inquiries in order to show how the "reflective equilibrium" strategy described above has guided the investigation. Some further comments on medical ethics will illustrate the internal consistency of the philosophical framework that has been erected. Finally, some lines of empirical research which are suggested by these investigations will be discussed.

Chapter 1. The Placebo Effect: A Review of the Medical Literature

Physicians have only recently approached the placebo effect as a subject for formal investigation and speculation. Pepper, in a 1945 paper sometimes considered a classic in this field, admitted that he was unable to find any articles on placebos listed in two major medical bibliographic indices. Shapiro, a prolific reviewer, states that the recent interest in placebos dates from 1953 and was stimulated by the desire to design adequate double-blind therapeutic trials (1968).[1] Probably the bulk of the medical literature on placebos treats the placebo effect as a nuisance variable, worthy of notice only for the havoc it can wreak upon inadequately designed experiments. Thus one reads, for instance, that psychotherapy will become a more potent tool when it is isolated from the concomitant placebo effect, just as foxglove became a more useful medicine when the active ingredient, digitalis, was extracted (Shapiro 1964). But other writers emphasize the positive therapeutic potential of the placebo effect, and the insight it may offer into psychosomatic disease and healing.

This review of the medical literature will consider in turn the history of the term 'placebo' and definitions offered for it; the nature and scope of placebo responses; agents that can act as placebos; factors

1. In a double-blind trial, neither investigator nor experimental subject knows whether the subject is in the control or the experimental group. Thus if the experimental group is to receive a drug or other treatment, the control group must get a dummy treatment that outwardly resembles the experimental one but which lacks the ingredient under study.

influencing the placebo effect; and explanatory hypotheses that have been proposed. This will provide material for a more critical analysis in the next chapter.

1.1. History and Definition

The word 'placebo' entered the English language in the 14th century as the name for the vespers sung for the dead (Shapiro 1968). The word was derived from the Latin version of Psalm 116:9: *"Placebo Domino in regione vivorum..."* (Pepper 1945), usually translated, "I shall walk before the Lord in the land of the living," although the literal translation of *placebo* is "I shall please." From this original meaning, the word acquired both its medical application and its negative connotation. In the former instance, doing something purely symbolic for patient and relatives when nothing curative can be done, something both soothing and inexpensive, could be compared to singing a hymn (Osmond 1974). In the latter instance, 'placebo' came to be used in Chaucer's time to mean a sycophant or servile flatterer, derived from the practice of singing vespers on behalf of strangers for pay (Pepper 1945).

Motherby's New Medical Dictionary (1785) defined 'placebo' in neutral and uninformative terms as "a commonplace method or medicine" (Shapiro 1968). By contrast, *Hooper's Medical Dictionary* (1811) derided 'placebo' as "an epithet given to any medicine adopted to please rather than to benefit the patient" (Pepper 1945), as if the two were mutually exclusive goals.

Contemporary definitions offered by investigators in the field tend to avoid judgmental terms, but still show significant differences

of opinion. Pepper represents the restrictive end of the spectrum by defining 'placebo' as an agent which is totally inert (1945). Presumably he means inert in a pharmacologic sense only; if an agent produced no effect whatsoever one would hardly want to label it a placebo. Wolf clarifies this point by defining 'placebo effect' as "any effect attributable to a pill, potion, or procedure, but not to its pharmacodynamic or specific properties" (1959). And a very broad and inclusive definition is suggested by Modell's comment that the placebo reaction is "the only single action which all drugs have in common" (1955, p. 55).

Probably the most detailed definition is Shapiro's:

> A *placebo* is defined as any therapy (or that component of any therapy) that is deliberately used for its nonspecific psychologic or psychophysiologic effect, or that is used for its presumed effect on a patient, symptom, or illness, but which, unknown to patient and therapist, is without specific activity for the condition being treated.
>
> A *placebo*, when used as a control in experimental studies, is defined as a substance or procedure that is without specific activity for the condition being evaluated.
>
> The *placebo effect* is defined as the nonspecific psychologic or psychophysiologic effect produced by placebos (Shapiro 1968, p. 599).

Shapiro notes that by his definition, a placebo 1) may be pharmacologically inert or active; 2) may or may not produce the placebo effect in any given instance; 3) may produce an effect which is either positive or negative (*i.e.*, placebo side effects). The reference to "presumed effect" allows for the notion of unwitting placebo use by physicians, and is the basis for the often quoted statement, "The history of medical treatment can be characterized largely as the history of the placebo effect" (Shapiro 1968, p. 597).

The differences among these definitions, as well as specific

problems created by each definition, form the major issues requiring analysis in the next chapter.

1.2. Nature and Scope of Response

Shapiro succinctly summarizes the importance and the breadth of the placebo effect:

> Many papers have demonstrated the importance and magnitude of the placebo effect in every therapeutic area. Placebos can be more powerful than, and reverse the action of, potent active drugs. The incidence of placebo reactions approaches 100 per cent in some studies. Placebos can have profound effects on organic illnesses, including incurable malignancies. Placebos can often mimic the effects of active drugs. Uncontrolled studies of drug efficacy are reported effective four to five times more frequently than controlled studies. Placebo effects are so omnipresent that if they are not reported in controlled studies it is commonly accepted that the studies are unreliable. Increased appreciation of placebo effects is reflected in the speculation that the major medical achievement of the last decade will be recorded by future medical historians as the development of methodology and controlled experiments (Shapiro 1968, p. 598).[2]

The symptom most often thought of in association with placebos is pain; but placebos modify both subjectively reported and objectively observable symptoms. One reviewer gives the following list of conditions in which placebos have been shown to produce relief: cough, mood changes, angina pectoris, headache, seasickness, anxiety, hypertension, status asthmaticus, depression, and the common cold (Bourne 1971). Placebos can lower blood sugar levels in diabetics (Singer and Hurwitz 1967) and can shrink tumors in patients with malignant lymphosarcoma (Klopfer 1957). When a subjective symptom and its physiological

2. The use of the word "mimic" might unintentionally suggest that the placebo effect is somehow less real than the pharmacologic effect of drugs. The problem of using neutral language in describing the placebo effect, so as not unwittingly to beg the interesting questions, must be kept in mind.

concomitant (*e.g.*, nausea and disturbed gastric motility) can be observed simultaneously, placebos can be shown to affect both (Wolf 1950).[3]

Placebos can also produce toxic side effects like those of "active" drugs. One typical study reported the following side effects among 25 patients experiencing negative placebo reactions (Honzak, Horackova and Culik 1972):

Somnolence (10 cases)
Palpitations (9 cases)
Irritability and insomnia (8 cases)
Weakness, with drop in blood pressure of more than 20 mm mercury (5 cases)
Temporal headache (4 cases)
Diarrhea (3 cases)
Collapse (2 cases)
Itching (2 cases)

In addition, three of these patients developed dependence to the placebo and demonstrated withdrawal symptoms when the pill was stopped. In another study, one patient repeatedly responded to placebo administration by developing a florid rash, diagnosed as classic drug-induced dermatitis by a consulting dermatologist; it ceased immediately upon discontinuance of the placebo, which in this case was plain lactose (Wolf and Pinsky 1954).

Placebo reactions may resemble those of "active" drugs not only in the end results but also in the patterns of activity. These patterns include a peak effect a certain number of hours after administration of the drug; a cumulative effect of increasing symptom relief as

3. These findings should put a lie to the myth, still prevalent among physicians, that if a patient responds to placebo his symptoms must be either imaginary or feigned, and that a placebo can be used in the differential diagnosis of psychic symptoms from "organic" ones.

the drug is continued over time, with a carry-over effect after the drug is stopped; and a decrease in efficacy as the severity of the symptom increases. These "pharmacologic" patterns occur with placebos as well as with "active" agents (Lasagna, Laties and Dohan 1958). Some investigators have reported that placebo effects are more transitory than "real" drug effects (Lasagna *et al.* 1954), but there is enough contrary evidence to question this (Rosenthal and Frank 1956).

1.3. Agents Acting as Placebos

Essentially any treatment modality can act as a placebo, and patient reactivity will vary according to the supposed potency of the treatment one thinks one is getting. A placebo capsule, in general, is more powerful than a placebo pill; an injection works better than either; and an injection that stings is better than a painless one (Evans 1974). In one study a white or yellow capsule produced the maximal therapeutic effect, while side effects occurred most frequently with a reddish-gray capsule (Honzak, Horackova and Culik 1972). Surgery is an especially powerful placebo stimulus (Beecher 1961).

It is not always easy to distinguish a placebo stimulus from active therapy. A recent study tried to compare true acupuncture therapy with a sham acupuncture procedure for chronic shoulder pain. The "placebo" treatment consisted of pricking the skin with acupuncture needles without actually inserting them, and then tapping them on the skin (Moore and Berk 1976). But since cutaneous stimulation of any type may promote pain relief (Melzack and Wall 1965), the sham procedure cannot be considered to be physiologically inactive.

An especially intriguing study was Park and Covi's (1965)

"non-blind" placebo trial. These researchers gave sugar capsules to 15 outpatients with neurotic complaints, telling them that the pills were sugar and contained no medicine; that such pills had helped other patients in the past; and that the doctors were convinced that the patient would get relief also. Fourteen patients completed a week's trial of therapy, and all but one showed improvement of symptoms by a standard symptom inventory (the remaining patient's husband had made a suicidal gesture during that week).

The patients could be divided into three groups: those certain that the capsule was a placebo; those certain that the capsule was an active drug; and those not certain. The two groups feeling certain either way showed the most improvement. Of the first group, half attributed their improvement to the placebo, and half to their own abilities to cope-- one stated that the pill served as a constant reminder that she could do something to improve her own condition. Also, some of these patients were glad to be avoiding the addiction and overdose potentials of active medication. Among those sure that the placebo was really an active drug, most reasoned that this must be the case since they had improved. They either ignored the sugar-pill explanation, or dismissed it as a therapeutic gimmick of the physician to encourage patient self-sufficiency. Half of those certain that the pill was an active drug reported side effects, while none of those believing that it was a sugar pill did so.

A major flaw in the Park and Covi study is that the investigators were initially unwilling to offer placebo as the only treatment modality to these new patients seeking help. Therefore they explained that the placebo would be given only for one week and that subsequently other

treatment could be considered. This may have had the effect of putting the patients "on probation" for the week and thereby creating a great desire to please the doctors-- possibly accounting for the nearly 100 per cent placebo response as compared to the more usual 30 to 50 per cent response (Beecher 1955). It has also been shown that being on a waiting list to be seen at a psychiatric facility exerts a placebo effect of its own and hastens recovery (Sloane *et al.* 1975).[4] As more is written about the placebo effect, however, patients are more likely to conclude that they may have received placebos, and this may not necessarily hamper their response to therapy (Cousins 1976).

1.4. Factors Influencing the Placebo Effect

As soon as the importance of the placebo effect began to be understood, investigators began to search for personality factors that would identify the "placebo reactor," in the hopes that eliminating such subjects from controlled studies would produce clearer data. An early study claimed that the placebo reactor displayed the following characteristics: more outgoing; more anxious; less emotionally mature; more concerned about visceral complaints such as constipation; and more satisfied overall with their hospital experience. But the non-reactors in this study responded less well to analgesics as well as to placebos for pain relief, raising the question of whether the reactors simply had less severe pain (Lasagna *et al.* 1954). A later study, done in a laboratory and thus perhaps not comparable to Lasagna's hospital data, held that reactors by psychological testing were more

4. For more on this waiting-list study see the Conclusion, especially Footnote 3.

enthusiastic, outgoing, and verbal, and better adjusted, than nonreactors (Muller 1965); this picture seems to conflict with Lasagna's at several points. Yet another study found no difference between reactors and nonreactors when the same psychological test instrument was used, but a separate personality inventory showed reactors to be more neurotic and extroverted (Gartner 1961). In sum, there are so many inconsistencies among these and many other studies that one may reasonably conclude that there is no single personality type characterizing placebo reactors (Kurland 1960; Shapiro 1968). There may, however, be some evidence to suggest that patients who develop worsening of symptoms on placebo may be distinguishable on some personality measures either from positive reactors or from nonreactors (Shapiro *et al.* 1973).

In more cases than not, an individual who responds to placebo under one set of circumstances will fail to respond under other circumstances, even in the course of the same study; these inconsistent reactors generally outnumber consistent reactors and consistent nonreactors combined (Lasagna *et al.* 1954; Beecher 1955). The only study to show a nearly 100 per cent constancy of reaction or nonreaction, a 1946 study of headache, has not been replicated (Jellinek 1946).

A large number of other patient variables have shown either no correlation or contradictory correlation with the placebo response. These include age, sex, intelligence, findings on Rorschach and other psychological tests, and presence of neurosis or psychosis (Shapiro 1968).

One finding in patients that has been rather consistently correlated with placebo reactivity is stress or anxiety. Even here there are questions, however. Beecher (1955) claimed that patients with more severe pain were more likely to get relief from placebos, and suggested

that the psychic stress accompanying pain contributed to the placebo effect; but as already noted, Lasagna and co-workers found the opposite correlation between pain severity and placebo response (Lasagna *et al.* 1954; Lasagna, Laties and Dohan 1958). In treatment of anxiety neurosis, Rickels and Downing (1967) found that patients with less pretreatment anxiety responded better to placebos.

Other patient factors correlated with placebo reactivity are harder to measure, and include positive expectations, faith in the physician, motivation, and the need for emotional catharsis or for psychological defense mechanisms (such as the ritual of taking medicine as a means for reducing anxiety). A study of patients with paranoid symptoms found that those who exhibited readiness to enter into personal relationships with the therapists were good placebo reactors, while those holding back from such relationships were not (Freedman *et al.* 1967).

Expectations are commonly cited as an important factor in producing the placebo response. In a study of placebo to improve short-term memory in elderly patients, patients' expectations were highly correlated with subjective improvement, and were correlated somewhat less well with objective improvement (Nash and Zimring 1969). One detailed attempt to study the role of expectations occurred in a study of how biofeedback could increase the frequency of alpha rhythm ("relaxation") on the subjects' encephalograms (Stoebel and Glueck 1973). The investigators designed an index to measure the combination of actual learning of alpha control and placebo effect. Using this index, they showed that patients did best in the long run when expectations and active learning were kept in relative balance. For instance,

subjects with very high initial expectations tended to be discouraged by the actual results first obtained, and thus performed less well on subsequent training. However, this index is rather speculative in nature, and perhaps ought to be viewed as a predictor of long-term outcome rather than as a measure of placebo effect.[5] Also, these investigators seem not to have distinguished carefully enough between expectation and motivation; each may well contribute to a positive outcome, but through different mechanisms (Rosenthal and Frank 1956). However, if accepted, the results of this study would suggest that either too high or too low expectations could hamper placebo response.

Clearly, factors such as expectations and motivation are not patient variables strictly speaking, but could be expected to depend at least in part on the physician, the situation in which the placebo is administered, and other external factors:

> [Expectations] vary widely among patients, depending on such factors as the patient's previous experiences with physicians and medications, his personal knowledge of the physician, the reputation of his physician in the community, the community belief in the recent achievements of medical science, various relevant properties of the institution or the setting in which the physician operates, and the physician's personality and behavior and his own expectancies as to what he can accomplish (Whitehorn 1958, p. 662).

While investigators for the most part have been reluctant to switch their attention from the placebo and the patient to the entire placebo context, the accumulating data have forced this change in focus (Wolf

5. It would have been interesting to employ as part of this study a true placebo or "dummy" biofeedback (*i.e.*, the biofeedback signal being given to the subject randomly, instead of only when the subject was exhibiting alpha rhythm); unfortunately this was not done. This omission casts even more doubt on whether the index formulated by the investigators was measuring a "placebo" response.

1959, Shapiro 1968).[6]

In the absence of good objective data on the contribution of the physician to the placebo effect, a good deal is based on generalizations from other types of studies, such as studies of experimenter bias in research and of the influence of therapists' behaviors and attitudes on outcome in psychotherapy (Shapiro 1968). A classic study of experimenter bias had the experimenters being told that their rats had been especially bred either for brightness or for dullness, although all rats were in fact from the same genetic strain. The experimenters then performed learning experiments on their rats and obtained the data that conformed to whatever their expectations of the rats' behavior had been (Rosenthal 1963). If scientists can somehow communicate their own expectations and attitudes to rats, it seems reasonable to assume that physicians can unknowingly communicate expectations and attitudes to patients, altering the patients' therapeutic outcomes as a result.

One study that did document the physician's attitude as a factor compared relief of anxiety by two sedatives and by placebo. When the drugs were administered by one doctor, who anticipated that there would be no difference among the two active drugs and who was viewed by patients as more neutral and matter-of-fact in manner, there were no differences in relief among the three agents. When administered by a

6. This reluctance may stem from the desire to try simpler hypotheses with more readily measurable variables, and also from the trend in psychosomatic research in the 1940's and 1950's to define "personality types" associated with specific diseases. One might speculate that had watchmakers conducted the first experiments on hypnosis, they would have tried to correlate the trance state with the type of watch being swung before the subjects' eyes, and would have been chagrined when such a correlation failed to appear.

second doctor, who anticipated greater efficacy of the active drugs and whom the patients viewed as more optimistic and supportive, the two sedatives showed superiority to placebo. In addition, patients showed greater overall relief of symptoms when treated by the second doctor. One way of explaining these results would be that the two physicians had in fact found a way to guess correctly which pills were sedatives and which were placebo, so that the double-blind experimental design had broken down; but this was checked for and found not to be the case (Uhlenhuth *et al.* 1959).

Even less is known about the role played by other factors in the healing environment. We have already mentioned differences produced by changing the color or the route of administration of the placebo (1.3).

1.5. Theories of Placebo Mechanisms

All medical authorities speculating on how placebos might exert their influence agree on one point-- that a placebo "cannot possibly act" through a pharmacologic or physiologic route (Beecher 1955). Implicit or explicit in most investigators' definitions is that if a substance now held to be a placebo, such as lactose, turns out to have a biochemical effect, this datum will prompt the reclassification of the substance as an active drug and will not be accepted as empirical evidence to explain the placebo effect. Also implicit in most views is the assumption that as we learn more about the specific physiologic and psychologic mechanisms of drugs and other treatments, the realm of effects now attributed to placebos will shrink (Shapiro 1964)-- that is, that to call something a "placebo effect" is now as much an admission

of ignorance as a potential explanation.

Byerly (1976)[7] has classified possible placebo theories as mentalistic, conditioning, or mixed. Mentalistic theories presumably are those that make reference to the subjective states of awareness of the patient, while conditioning theories are types of behavioristic accounts, which make reference only to outwardly observable behavior.

The most commonly encountered mentalistic theories are those referring to patient expectation (*e.g.*, Rosenthal and Frank 1956; Nash and Zimring 1969); such theories are also sometimes referred to as self-fulfilling prophecy (Beecher 1955) or response-bias theory (Morris and O'Neal 1974). By all these theories the patient's expectation of symptom change is held to be causally connected to the change that occurs. Since the central nervous system, the autonomic nervous system, and the endocrine system all exhibit predictable changes in response to the person's emotional state, these are frequently suggested as the intervening psychophysiologic mechanisms (Wolf 1959).

Theories that are almost purely mentalistic hold that the placebo effect works solely through alteration of the patient's subjective reaction to illness. In pain relief, the placebo is said to act strictly by relieving anxiety, which in turn produces relief of pain (Evans 1974); or pain itself is said to consist of a sensory component and a subjective-processing component, with the placebo affecting the latter and not the former (Beecher 1955).[8] But these theories ignore

7. To my knowledge this paper by Byerly is the only attempt to date to approach the placebo effect from the philosophical standpoint.

8. Beecher's two-stage pain theory derives from studies of narcotics in treating war wounds; soldiers given morphine claimed still to feel pain but no longer to be bothered by it. Beecher concluded that

both the ability of placebos to relieve many other symptoms besides pain, and the documented impact of placebos on objectively observable bodily function; such data seem to render any purely mentalistic theory untenable and to require some sort of psychophysiologic view.

Another form of mentalistic theory, arising from the Freudian tradition, is transference theory (Forrer 1964). Transference is defined in psychoanalysis as the unconscious projection of feelings, attitudes, and wishes properly displayed toward a significant figure in early development (usually the parent) onto another person in the individual's current life (the doctor or therapist) (Freedman, Kaplan and Sadock 1972, p. 798). A satisfactory doctor-patient relationship invites the patient unconsciously to trust in the doctor, to submit to his wishes, and to expect him to "make it better" in a way similar to the parent-child relationship (Shapiro 1968). Transference may be seen as an adjunct to expectation theory, if positive transference encourages optimistic expectations; or it may be seen as an independent mechanism, acting through an unconscious release of psychic tensions, for example.

Some theorists have attempted to reduce placebo responsiveness to suggestibility, which may be defined as a state of compliant responsiveness to ideas or influences (Freedman, Kaplan and Sadock 1972, p. 795); susceptibility to hypnosis is a commonly cited example of suggestibility. However, Shapiro notes that patients experiencing hysterical conversion reactions (psychic symptoms), who are commonly considered

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pain consists of two phases, the sensation and the emotional reaction to it. I cannot fully evaluate the tenability of this view here; but *cf.* comments on "Pain and Suffering" by Jerome Shaffer in (Spicker and Engelhardt 1976, pp. 221-233).

to be extremely suggestible in the way most psychologists use the term, are very poor placebo reactors; he feels that this casts doubt on the suggestibility theory (Shapiro 1968). But other investigators question whether the case of the conversion hysteric is a true case of suggestibility as defined above (Kurland 1960). In one study, the more suggestible patients showed significantly greater placebo reactivity in the first week of therapy, but later showed less reactivity than other patients. The authors proposed that the more suggestible patients might "overrespond" to placebo at first, producing an apparent, relative worsening of symptoms later on; at any rate, suggestibility alone could not account for the extent and duration of the placebo effect (Steinbrook, Jones and Ainslie 1965).

In contrast to these mentalistic theories, conditioning theory takes a stimulus-response form which makes no reference to the internal, mental states of the individual. Past instances of active therapy in medical settings are seen as the stimulus, while relief of symptoms is the original, unconditioned response. As conditioning occurs, the medical setting itself becomes a sufficient stimulus and the therapeutic response becomes conditioned, so that it occurs even without active treatment (just as, after repeated presentation of food together with a bell sound to dogs, the bell alone will produce salivation) (Gliedman, Gantt and Teitelbaum 1957).

Difficulty in choosing among competing theories is illustrated by Bourne (1971), who argues that transference and conditioning suffice equally well to explain some of the commonly observed placebo phenomena:

> Finding: Placebo response is maximized by anxiety.
> Transference account: Anxiety produces a "set" for transference, by encouraging regressive behaviors harking back to an earlier

stage of psychic development.
Conditioning account: Stress causes the organism to fall back on conditioned responses instead of trying new adaptive behaviors.

Finding: Placebos often work best in diseases characterized by quiescent periods broken by periodic flare-ups.
Transference account: Recognition of experiences undergone in the past, such as a disease flare-up occuring as part of a recognized pattern, increases transference potential.
Conditioning account: Repetition of the stimulus is essential for conditioning to occur.

Finding: Placebos work best on symptoms under central nervous system, autonomic, or hormonal control.
Transference account: Such symptoms are most susceptible to changes resulting from increase or decrease in psychic tension.
Conditioning account: Such bodily changes are most accessible to conditioning, being physiologically most closely connected with sensory inputs.

An additional mechanism that has been proposed is attribution theory, which is not a conditioning theory but which does not seem to be clearly mentalistic either. It holds that placebo reactors are simply highly sensitive to subtle changes in their internal states. If a symptom lessens very slightly in severity following placebo administration, the individual will detect this and will attribute the change to the placebo. One study of placebo response attempted to measure this internal sensitivity or "openness"; they found it not to be correlated with placebo reactivity, although patients' expectations were (Nash and Zimring 1969).

Another study set out directly to test alternative theories by giving placebos labeled with either familiar or unfamiliar drug names. By conditioning theory, they reasoned, familiarity would enhance the placebo effect by providing a stronger conditioned stimulus. By attribution theory, unfamiliarity might be expected to enhance the placebo effect, as the patient would be familiar with and sensitive to the

pharmacologic effects of drugs that he had previously taken. By expectation theory, the physician's suggestion and attitude should be the controlling variable, with familiarity playing only a minor role. These investigators found no correlation of placebo response with familiarity or unfamiliarity, concluding that their results were most consistent with expectation theory (Morris and O'Neal 1974).

Finally, Byerly (1976) suggests the possibility of other theories which avoid the rigid distinction between mental and bodily phenomena; as an example he cites a view of the "symbolic reality" of medicine which treats disease as inherently a cultural construct (Kleinman 1973). Earlier placebo writers mention the symbolic aspects without making clear whether they are construing symbolic import in strictly mentalistic terms, or whether they hold, with Kleinman, that symbolic significance influences bodily health and disease:

> [T]he physician is a vastly more important institution than the drug store. The reasons for this are deeply rooted in the mainsprings of human behavior, for man in distress wants action--rational action if possible, of course, but irrational action, if necessary, rather than none at all.... [T]he pill the patient swallows, no matter what its nature, acquires potency as a symbol of faith, wisdom, and support (Findley 1953, pp. 1822-1823).

> The physician's ability to relieve the emotional, reactive aspects of a patient's illness through symbolic operations is therefore an important aspect of his healing function.... Hence the prescription, pill or injection symbolizes the physician's healing function. The prescribing of a pharmacologically inert substance may thus, through its symbolic significance, produce favorable effects (Whitehorn 1958, p. 662).

The clearest theoretical statement of a symbolic-cultural basis for the placebo effect is given by Adler and Hammett (1973) in what I shall be calling the "meaning model" of the placebo effect. Adler and Hammett identify two invariant features of healing practices in all cultures: 1) a shared cognitive system which explains illness in terms

(whether of natural phenomena or of supernatural occurrences) readily understandable to those sharing the background of cultural beliefs ("system formation"); and 2) a relationship with a socially sanctioned healer occupying a role of parental power and influence, which in turn stimulates caring responses from family and community ("group formation"):

> It is suggested here that these two factors-- group formation and system formation-- are as essential to psychic functioning as nourishment is to physical functioning, are the basic factors composing what is subjectively experienced as a feeling of "meaning," are invariably used in all successful interpersonal therapies, and are the necessary and sufficient components of the placebo effect (Adler and Hammett 1973, p. 597).

The data now available do not seem to be sufficient to exclude with certainty any of the theories that have been listed above.[9] More research needs to be done, especially research like the Morris and O'Neal (1974) study cited above which sets out to compare different theories. Future research strategies suggested by some of these theories, especially research into the healing situation as a whole and into its symbolic and cultural aspects, will be mentioned in the Conclusion. But to a great extent, interpretation of any future data will depend on getting clearer about precisely what is meant by 'placebo effect,' and about which phenomena are or are not applicable to its study; discussion of these matters will occupy most of Chapter 2. In addition, a study of the implications of some of the placebo theories for the nature of the mind-body relation might provide additional

9. It could, of course, be argued that there is not one mechanism but several responsible for the placebo effect. Beecher took the reproducibility of placebo response rates from study to study (an average of 35.2 per cent, with a standard deviation of 2.2 per cent, in 15 studies covering a variety of symptoms) as evidence supporting a single mechanism (Beecher 1955).

grounds for accepting or dismissing some of the theories; this will be considered in Chapters 3, 4 and 5.

In summary, while the word 'placebo' has been in the medical lexicon for at least several centuries, contemporary definitions still disagree on the scope of effects attributable to it. It is widely agreed, however, that the placebo is very powerful and can accomplish in some instances the majority of effects of which "active" drugs and other therapies are capable. Placebos influence both subjective and objective symptoms and can produce toxic side effects. The proportionate incidence of placebo reactions is roughly predictable from study to study, but the search for specific personality traits that will predictably pick out the "placebo reactor" has mostly failed. It appears that the same individual may or may not react to placebo depending on a complex set of internal and environmental factors, including the relationship with the physician and the nature of the healing situation as a whole. However, specific factors that do seem to be reliably related to positive placebo effects include positive expectations and perhaps some degree of stress or anxiety in the patient. A variety of theories have been put forth to account for the placebo effect; these include mentalistic theories (expectation or response-bias, transference, and suggestion theories); conditioning theories; and "mixed" theories such as those calling attention to the "meaning" of the illness experience for the patient.

Chapter 2. A Definition of the Placebo Effect

In 1.1 we reviewed several definitions of 'placebo' that had been suggested by medical authors. But these definitions lacked rigorous analysis, and were indeed mutually contradictory in some ways. Since they were formulated primarily to introduce and organize various particular medical findings, they cannot be expected to bear much philosophical weight. In this chapter I shall try to formulate a definition of 'placebo effect' that can serve as a basis for further philosophical investigation.

The first section applies Thomas Kuhn's (1970) notion of a scientific paradigm to the placebo effect; this will allow us to ask later the extent to which a definition is dependent upon a particular explanatory context. The next section gives a series of illustrative examples to determine the range of phenomena that a definition must cover. The third section begins by offering formal definitions of 'therapy' and 'specific,' and then uses these to arrive at definitions of 'placebo effect' and 'placebo.'

2.1. The Placebo Effect as Medical Anomaly

Some concepts that have utility when applied to the placebo effect arise from Kuhn's (1970) reconstruction of the history of science. Scientists conducting research rely heavily, not only on the explicitly stated laws and theories of their science, but also on a set of assumptions and explanatory presuppositions which remains implicit but which

uniquely characterizes the science that they are engaged in. These presuppositions create expectations about the world and suggest both what sorts of phenomena are most usefully studied, and how observations or experiments are best carried out. The presuppositions are thus very useful in guiding scientific research and in steering scientists away from troublesome areas not accessible to the scientific tools at hand. But invariably data will be collected which are at odds with this set of presuppositions, and which are unexpected according to the accepted set of laws and theories. Scientists will first attempt to account for these findings by making slight modifications in the existing theories; but over time more and more unexpected findings accumulate. At some point a few "revolutionary" scientists put forth totally new laws and theories, which are based on a different set of presuppositions. If this new set of theories both explains the previously unaccounted-for data, and embraces the accumulated knowledge of the old science while opening up new avenues for further research, scientists will come to adopt it, and a scientific revolution will have occurred. Kuhn terms the set of basic presuppositions and assumptions a paradigm,[1] and calls the unexpected findings, that can lead to overturning paradigms, anomalies.

An example from physics may illustrate how paradigms and anomalies

1. "Paradigm" may be used to refer either to the body of shared beliefs of a scientific community, or specifically to that community's puzzle-solving examples which have the most direct impact on research design (Kuhn 1970, pp. 174-191). I use the term here more in the former sense, referring especially to the elements of heuristic models (*e.g.* of disease causation) and values (*e.g.* what counts as a "good" explanation). However Kuhn's latter sense of 'paradigm' cannot be completely separated from this usage-- the place of Koch's postulates in contemporary medical science shows how values, heuristic models, and puzzle-solving examples are mutually bound up.

are related. The paradigm dominant in physics in 1895 led scientists to expect to find various sorts of rays, but rays that could cause a plate to glow across the room from a cathode ray tube were not among these. Thus, when Roentgen noticed such a glow, he was observing a phenomenon which had previously been created in many other laboratories, but which had not been observed because the theories and the presuppositions of physics did not tell anyone where to look for it. (By contrast, a totally expected finding might be the discovery of a new element whose properties had already been predicted by the periodic table.) Roentgen's announcement of his discovery, therefore, stirred immediate controversy. At the very least, accepting his data would require that many accepted experiments be done over, since there was now this new variable that had to be controlled. The clash with existing assumptions was so strong that some eminent physicists, such as Lord Kelvin, refused to believe Roentgen's data. About the same time, however, physics was accumulating other anomalies, including black-body radiation and the constancy of the speed of light; and so when the new paradigms of quantum mechanics and relativity appeared, which could account for these anomalies better than the old Newtonian paradigm, physics was ready to accept them (Kuhn 1970, pp. 57-61).

In medicine, underlying paradigms include theories and assumptions about the nature of disease and therapy, and about laws and regularities in human pathophysiology. The present-day, Western medical paradigm emphasizes causal mechanisms affecting organs, tissues, cells, chemical factors, and physical phenomena. Theories relating psychological and sociological factors to disease and therapy are generally less well developed and held in lower esteem-- one might feel that they

will have to do until "real" explanations in physical-chemical terms become available through further research. Within such a paradigm, the fact that a chemically inert pill can change symptoms and "organic" bodily states constitutes a significant anomaly. As was the case with x-rays, accepting the placebo phenomenon entails rejecting a major body of previously accepted data, since until recently most of what was known about therapeutics came from uncontrolled trials. A discovery such as the placebo effect is likely to be greeted with consternation among medical scientists, unlike, for instance, the discovery of a new antibiotic to treat tuberculosis; the dominant paradigm leads the scientist to expect the latter but not the former.

In the absence of an attractive alternative paradigm that can totally replace the existing medical paradigm, we see different attempts to deal with the placebo discovery. The serious physician today cannot deny the placebo data; but he can instead adopt an attitude of exclusion towards it-- he may be content merely to label the placebo effect so that it can be readily recognized and therefore excluded from research. The early attempts to determine a placebo-reactor personality type so that such subjects could be excluded from clinical trials (1.4) is an example of this approach; the scientist reasons that he might as well focus his attention on those phenomena which are most readily explainable by accepted theories, and put any anomalies he finds "on the shelf." The suggestion that the placebo effect is an impurity which ought to be removed from psychotherapeutic modalities (Shapiro 1964) is another example of exclusionary thinking. This sort of thinking may influence and may implicitly occur in the definition one adopts for 'placebo' and 'placebo effect.'

By contrast, an inclusive approach would seek new laws or causal factors, to expand or modify the existing paradigm so as to bring the placebo effect within it. The fact that the dominant paradigm has grudgingly admitted phenomena such as psychosomatic disease might lead one to think that this expansion or modification need not be a drastic one, and that the paradigm will emerge stronger for the change. Research studies such as many cited in Chapter 1, which look at the placebo effect as a phenomenon to be studied on its own grounds rather than as a variable to be controlled, exemplify the inclusionary approach.

X-rays were anomalous from the viewpoint of the Newtonian paradigm, but not from the viewpoint of modern physics. Similarly, how one construes the phenomena we have been calling the placebo effect depends on the paradigm of reference. Consider an African native village with two witch doctors who use essentially identical healing rituals; an anthropologist discovers that one is viewed by the villagers as more highly expert at his craft, and that that one achieves a significantly higher cure rate than the other. The anthropologist might conclude that 1) all healing accomplished by either is due either to the placebo effect, or to the normal vicissitudes of disease; and 2) the greater healing rate of the one is due to a differential placebo effect, produced by greater expectations on the part of the patients. But this is to view the matter from the Western paradigm, which holds treatment not explainable in our accepted theoretical terms to be biomedically inefficacious. The disease paradigm operating in that village, however, may hold that a witch doctor's cure always works unless the patient fails to follow directions exactly, or thinks impure

thoughts while involved in the ritual; all treatment failures may be explained in these terms. The villagers might then postulate that the more respected witch doctor is better able to banish impure thoughts and to command compliance from his patients. Not only does this paradigm explain the phenomenon that we would attribute to the placebo effect in totally different terms; but this paradigm indeed seems to leave no room within its explanatory model for anything like the placebo effect at all.

Thus when Shapiro claims that the history of medicine before the present century is the history of the placebo effect (1968, p. 597), he is saying that therapies then in use are deemed worthless by modern medical science, and that nevertheless patients got better at a rate not attributable entirely to the natural recuperative powers of the body.[2] But this is again to apply our present paradigm uncritically; a serious medical historian would seek rather to determine what paradigms dominated the thinking of those earlier physicians. (I will suggest later that the use of the term 'placebo effect' in Shapiro's statement can be understood only in a derivative or metaphorical sense.)

Since the placebo effect is already a rather slippery concept, as the next section will show, one might want to begin the task of defining it by accepting at least one firm reference point; and for my discussion this will be our currently accepted medical paradigm. I will, however, try to indicate explicitly the paradigm-dependent elements in

2. Respect for the body's self-healing potential is justified by such classic treatments as (Cannon 1963). However, whether modern medicine is so clearly superior to past practices is cogently called into question by (Powles 1973).

the discussion, so that they will not escape critical scrutiny.

2.2. Boundaries of the Placebo Effect

The term 'placebo effect' can be construed very narrowly so as to refer to only a few sorts of phenomena, or very broadly so as to include much of medical practice and many nonmedical occurrences as well. There are some "core" uses of the term that almost all medical scientists would agree to; and there are uses of the term that fall near the "boundaries" of its applicability that might engender considerable debate. A good way to get clear on these boundary conditions is to consider a series of illustrative examples, which show what is at stake if we draw the boundary lines at various points. This will provide a basis for the formal definition in the next section.

> Case 1. A patient suffers from pain due to periodic flareups of rheumatoid arthritis. During one such episode the physician administers sugar capsules, telling the patient that this is a new analgesic drug. The patient subsequently reports dramatic relief.

Case 1 seems to be a straightforward and uncomplicated instance of the "core" sense of the term 'placebo effect.'[3] None of the medical authors cited in Chapter 1 would hesitate to apply the term in such a case.

> Case 2. A and B both contract a cold at the same time, under similar circumstances. A is administered a sugar pill, being told that it is a potent cold remedy; B gets no treatment. Both A and B recover from their symptoms at the same rate, with the

3. Recall Shapiro's (1968) contention that the placebo effect can be either positive or negative; for simplicity the case illustrations will deal only with positive placebo effects except where noted.

same level of discomfort until their colds subside.

Would one want to say in Case 2 that A experienced a placebo effect? By current medical thinking, the recovery of both A and B can be explained on the basis of the self-limiting aspects of viral infections, immune defense mechanisms, restoration of homeostatic processes, and so forth. There are thus good grounds to regard the taking of the sugar pill as irrelevant to A's course-- all things being equal, he would have gotten better in an identical fashion without the pill. It would sound paradoxical to attribute an "effect" to an intervention which played no role in the outcome; and on this analysis we would not regard Case 2 as an example of the placebo effect. We would rather say that both A and B got better as a result of the body's natural restorative processes.

Suppose on the other hand that one wanted to argue for a possible role for the placebo effect in Case 2. Taking seriously the *ceteris paribus* assumption, this would entail the presence of the placebo effect in such a way that we could not find out about it in terms of observable outcome. I will assume that one purpose of defining 'placebo effect' is to stimulate and guide empirical research into its workings, and that adequate understanding of it will involve empirical issues as well as conceptual ones. Given that purpose, there is nothing to be gained, and some measure of clarity to be lost, in taking Case 2 to involve the placebo effect.

> Case 3. A large number of individuals are suffering from a wide variety of diseases. Half of these individuals are fed an especially nutritious diet while the other half are fed a nutritionally inadequate diet. A larger percentage of the first group recover as compared to the second, although a number of individuals in the first group do have worsening of their disease despite the diet.[4]

The effect of diet on disease resistance has some features in common with the placebo effect. The same basic diet will be effective for a large number of diseases. There is a measurable positive response to diet therapy, but it generally falls well below 100 per cent (except in cases of specific nutritional deficiencies). And the diet presumably does nothing directly to alter the basic causative mechanism of the disease (microbes, cellular malignancy, or whatever).

However, as with the so-called natural restorative powers, we can explain the results of nutritional therapy by pathophysiologic theories which appear to be independent of the placebo effect; and so postulating a placebo effect in Case 3 as it stands would appear needlessly to multiply explanations (assuming that the improvement of the first group is not "over and above" the amount that can be explained on the basis of the theories alluded to). The features that nutrition and the placebo effect have in common suggest what medical authors have in mind when they refer to "nonspecific therapies." Exercise, and modalities which enhance the efficacy of the body's immune system, might be cited as other examples of "nonspecific therapy" (which itself stands in need of formal definition). Thus the placebo effect would be one type of nonspecific therapy, but is not coextensive with that class.

> Case 4. Imipramine is the drug of choice for treating certain types of depression. Both Dr. A and Dr. B use this drug, with the same dosage schedules, on large numbers of depressed patients. Dr. A is a surly fellow while Dr. B is encouraging and supportive. Of Dr. A's patients, 75 per cent are significantly improved in three weeks while 90 per cent of Dr. B's patients are improved in that time.

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4. I will, of course, not attempt to defend the ethics of such an experiment, were it to be done deliberately.

Imipramine is certainly not an inert substance; it is both active and specific for the condition being treated. But to explain the different results (again assuming the patient populations otherwise equal) we are inclined to view the total therapy as consisting of the drug plus the emotional-psychological features of the doctor's interaction with the patient. Like many investigators, we have been forced by the data to turn our attention away from the drug itself and to look instead at the total context (1.4); and on this basis we might attribute Dr. B's increased success rate to a placebo effect. If we do so, we are using 'placebo effect' to designate the results of one component of the therapy-- a component which in the actual setting might be so intermingled with other features of the doctor-patient exchange as to be practically indistinguishable.[5] This is different from the simple sugar-pill case; but on balance it seems a reasonable extension of the term. Some medical authors (Houston 1938; Wolf 1959; Shapiro 1968) define 'placebo effect' so as to allow for this use while others (Pepper 1945) do not.

> Case 5. A person who never goes to doctors decides to improve his health by undertaking an exercise program. He develops strength and endurance, as well as a more general sense of fitness and well-being.[6]

In Case 4 we explained the total result in terms of both pathophysiological and psychological features; and we attributed the latter component to the placebo effect. The increase in strength and endurance in Case 5 can be explained as specific outcomes of exercise. On

5. The objection raised in Case 2 above does not apply here. "Practically indistinguishable" does not mean empirically undetectable in principle; it merely challenges the ingenuity of the investigator for controlling for subtle variables.

6. I am indebted to David S. Sobel for this example.

the face of it, the psychological sense of well-being one gets in addition does not seem dissimilar from the added boost that a supportive doctor-patient relationship can give to an active medication. Is there any reason not to attribute this result to a placebo effect of the exercise?

To argue for such a reason I must introduce the notion of the "healing context." This is derived from the concept of the "sick role" first introduced by medical sociologists, which has proven very useful in cross-cultural studies of response to illness.[7] One feature of the sick role is that the sick person must submit to the authority of the socially-designated healer for the purposes of attempting a cure. While the healer may be a medical doctor, an herbalist, a shaman, or whatever, such socially-designated healing roles exist in virtually every culture that has been studied. Furthermore, it is usual if not universal for a particular setting-- hospital, cave, temple, etc.-- to be identified with the healing activity, and for certain ritual behaviors (often including behaviors not tolerated by that culture anywhere else) to become associated with that setting and with the purpose of healing. This combination of the designated healer, designated setting, and designated rituals I refer to as the "healing context." It refers to something that may be present in all cultures, without referring to the specific healing practices of any particular culture or the specific beliefs of any particular medical paradigm. As a rule, of course, the psychological reaction of the patient in the

7. For discussion of the sick role from the sociological perspective see (Parsons 1951; Parsons 1961; Siegler and Osmond 1973; Friedson 1970, pp. 205-243).

healing context can be elicited only if the culture-bound features of the healing context are those of the patient's own culture.[8] Considering the universality of patterns of social response to sickness, I think it is reasonable to assume that there are important similarities, say, between the native's psychological reaction to being in the shaman's temple and the Western individual's reaction to hospitalization.

The question posed by Case 5, then, is whether we wish to impose as a boundary condition on the term 'placebo effect' that it apply only to events occurring within a healing context. Even this condition may be too weak, as Case 6 illustrates.

> Case 6. A patient who is scheduled to undergo open heart surgery, and who is in acceptable physical condition, becomes very depressed and insists, despite support and reassurance from the medical staff, that he is sure that he is going to die during surgery. The operation is begun and all is going well until, for no apparent reason, there is a sudden drop in blood pressure. All attempts to correct this fail and the patient dies.

The ability of persons to "think" themselves into otherwise unexplainable deaths is well documented (Frank 1974, pp. 50-55; Engel 1976).[9] If we follow Shapiro's reasonable convention of referring to placebo effects as being potentially either positive or negative, could we attribute the death in Case 6 to a negative placebo effect? Unlike the situation in Case 5, the events in question occur within the healing context. But the psychological effect, depression, is neither the intended therapeutic intervention, nor a concomitant of the intended therapeutic intervention (as in Case 4); indeed the doctors in Case 6

8. Hence, treatment problems arise for Western medicine when the patient adheres to folk medical beliefs of his own subculture which are not understood by his physicians (Snow 1974).

9. Psychological risk factors in open heart surgery are currently being studied by Dr. Sumer Verma (personal communication).

try deliberately although unsuccessfully to counteract the depression.

Cases 5 and 6 suggest a conceptual "slippery-slope" problem with the boundaries of 'placebo effect.' There are an almost endless number of instances where suggestion or auto-suggestion, or other psychological states, influence persons' bodily processes or their perceptions of bodily processes (Frank 1974; Kiritz and Moos 1974). The psychophysiological mechanisms by which these occur require empirical elucidation. While it would be surprising if the mechanisms by which a sugar pill can ameliorate symptoms turned out to be totally different from the mechanisms involved in these other instances, the precise degree of similarity or dissimilarity needs to be investigated; it does not seem to be a matter to be decided by definitional fiat.

I have suggested already that the task of defining 'placebo effect' can be viewed as a preparatory step toward this needed research. In what ways, then, can the choice of definition either help or hinder research? It might help if the definition called attention to similarities between the defined phenomenon and an already-known class of events, where investigators had not already discerned the possible connection. But the placebo literature shows no reluctance to view the placebo effect in light of what is known about other psychophysiologic correlations-- we have already reviewed attempts to apply such standard psychophysiologic theories as conditioning and transference to the placebo problem (1.5).

Alternatively, a definition might hinder research if it was too inclusive, tempting the investigator to pass over important differences among classes of phenomena. For instance, so long as respiratory diseases caused by bacteria, mycoplasma, and viruses were all lumped

together as "pneumonia," the investigation of the worth of penicillin in treatment was bound to be impeded. We saw, in 1.3, how much remains to be learned about the psychophysiologic phenomena that occur within and as part of the healing context. I assume that a reasonable research strategy would be to get clearer on these instances before trying to generalize the findings to other aspects of human existence. If one accepts this empirical bias and this strategic assumption, it makes good sense to exclude the phenomena described in Cases 5 and 6 from the definition of 'placebo effect.'[10]

> Case 7. A is a Christian Scientist and, despite being severely ill with rheumatoid arthritis, refuses to take any sort of drug or other medical therapy. B, who is concerned about A's welfare, knows of studies showing that arthritic patients improve when given a placebo such as lactose. B obtains a supply of pink lactose tablets; but, knowing A's aversion to medication, contrives secretly to slip the tablets into A's coffee, without A being aware of this.[11]

Case 7 points out another feature of the healing context as it relates to the placebo effect. It makes sense to say that B has slipped a sugar pill into A's coffee, but does it make sense to say that B has slipped a *placebo* into A's coffee? The latter terminology seems to involve a conceptual absurdity, regardless of whether A's condition in fact changes or not.[12] The lesson of Case 7 is that it is not enough

10. It might eventually turn out that the similarities among the different classes of phenomena were so striking that 'placebo effect' would be dropped altogether in favor of a more general term such as 'autosuggestion.' Still, the term 'placebo' would remain in use to designate a dummy medication or treatment.

11. I am indebted to Martin Benjamin for this example.

12. The absurdity here is similar to that in a story Abraham Lincoln liked to tell, about an Irishman who had taken an abstinence pledge and was forced to order lemonade at a bar on a hot day; he finally leaned confidentially toward the barkeeper and asked, "Couldn't you put a wee drop o' the creetur into it unbeknownst to me?" (Sandburg 1939, I:572, IV:158).

for the subject to be in a healing context in order to allow application of the term 'placebo effect'; the subject must *believe* that he is in a healing context. Generalizing from numerous studies, it seems that the subject need not believe that the treatment being given is efficacious;[13] it appears sufficient that the subject believe that it is treatment, that it is a deliberate intervention being given in response to his illness condition with the intent of benefit.

Suppose that we inform B of his conceptual error, and he now has to decide what to do with his large supply of pink tablets. If he uses them in his own coffee, as a sweetener, we would not want to say that he is using placebos on himself. We have already noted the empirical findings that have led placebo investigators to focus on the context of placebo use, not on the dummy treatment itself; our analysis of Case 7 adds to this empirical observation the stronger conceptual point that the meaning of 'placebo effect' is context-dependent in the way that we have noted.

If belief in the healing context is a necessary condition, is it also sufficient? We could imagine an elaborate sham in which an individual was made to believe wrongly that he was in a clinic receiving treatment from a doctor when in fact he was getting dummy pills and shots from actors on a movie set. If the victim of this subterfuge experienced a relief of symptoms attributable to this experience, we could say without contradiction that a placebo effect had occurred. We might even want to go so far as to say that the belief itself was

13. In the nonblind placebo trial (Park and Covi 1965), several of the patients responding positively had initially expressed doubts that placebos would work; if placebos can work in the face of doubt they ought to work also in the weaker case of nonbelief.

sufficient to *make* the movie set a healing context *for that individual* in his present belief state, in the same way that a witch doctor's thatched hut may be a healing context for an African native but not for a Wall Street stockbroker. However, for our present purposes we need not debate this latter point. From a practical standpoint examples such as the movie-set sham do not pose any significant problem for defining 'placebo effect,' however interesting they might be in terms of isolating the key features of healing contexts.

Before turning finally to the matter of formal definitions, it is important to emphasize the difference between the boundary conditions indicated by Cases 5 and 6 and by Case 7. The former, requiring the healing context as a necessary condition, is a stipulative device suggested because of its probable utility for research. But the latter, requiring belief in the existence of a healing context, is conceptual and points out an essential feature of the word 'placebo.'[14]

2.3. Formal Definition of 'Placebo Effect'

The considerations from the preceding section can now be used to evaluate critically the formal and informal definitions offered by medical authors, and to make suitable changes. Four major definitions, already cited in 1.1, may be summarized as follows:

1) Pepper (1945): The placebo effect is a therapeutic effect produced by a biomedically inert substance.

14. However, see the formal definitions and discussion in 2.3, below, on why a placebo need not be present for the placebo effect to be said to occur.

2) Wolf (1959): The placebo effect is a therapeutic effect or side effect attributable to a treatment, but not to its pharmacologic properties.

3) Shapiro (1968, p. 599): The placebo effect is the nonspecific effect of a therapy, which may or may not have a specific effect in addition.

4) Modell (1955, p. 55): The placebo effect is what all treatments have in common.

These definitions are listed in order of increasing breadth, and increasing range of phenomena that fall under them. For instance, Pepper's definition would hold that if a specific pharmacologic effect is present, the placebo effect *cannot* be present; Wolf's and Shapiro's, that if a specific pharmacologic effect is present, the placebo effect *may* also be present; and Modell's, that if a specific pharmacologic effect is present, the placebo effect *must* be present.

On grounds already discussed we can eliminate the most narrow and the most broad of these four proposals. Pepper's approach is ruled out by our willingness to look at different components of a total therapeutic encounter, and to ascribe a placebo effect to a nonspecific component which may accompany administration of an active treatment. Modell's all-inclusive statement is refuted by an example mirroring Case 7. In *The Moonstone* a physician, angered by statements from the hero on the total worthlessness of medicine, secretly places some laudanum into the hero's coffee; and the hero, who had previously been troubled by insomnia, slept unusually soundly that night (Collins 1868). This is a clear case of pharmacologic potency without any accompanying placebo effect.[15]

The remaining proposals are substantially similar and are roughly consistent with our previous discussions. But they make use uncritically of the terms 'therapy' and 'nonspecific,' which seem to require some elucidation. I begin by offering a definition of 'therapy':

> T is a *therapy* for condition C if and only if it is believed that administration of T to a person with C increases the empirical probability that C will be cured, relieved, or ameliorated, as compared to the probability of this occurring without T.

This definition of 'therapy' is intended to be as general as possible, embracing drugs, surgery, physical therapy, psychotherapy, and so on, even though it does not include measures aimed solely at prevention of disease. "Administration" should be interpreted to include acts of omission, such as salt restriction, and self-administration by the person himself; but it is intended to restrict 'therapy' to acts of deliberate intervention or human agency. The definition does not explicitly require that condition C be a disease or a symptom of disease; this interesting issue is not pertinent to the matter at hand.

The phrase "it is believed that" is included to allow one to speak of ineffective therapies; if this were omitted the definitions for 'therapy' and 'effective therapy' would be the same, contrary to general usage.[16] The definition also indicates implicitly when one is justified in believing that T will relieve C, by including the reference

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15. Modell elsewhere seems aware of this problem when he states that the placebo effect invariably accompanies every *prescription* of a drug (1955, p. 54).

16. *Who* believes this is deliberately left vague; it might be the person with C, the person administering T, or some third party as outside observer. The importance of specifying which of these hold for a specific case is illustrated by the witch-doctor example in 2.1 above.

to empirical probability-- either a randomized controlled study must show that T is more likely than no treatment to relieve C; or other theories of pathophysiology, which are themselves supported by empirical data, must predict T's efficacy based on known causal mechanisms for C. Anecdotal evidence or personal experience justify the belief only in a derivative sense; one must be willing to assume that a future controlled trial, if carried out, would confirm this evidence. Therefore, this definition of 'therapy' is dependent upon our present medical paradigm, which holds up the standard of the randomized, controlled trial over any other form of investigation. By the definition, we might say that physicians of other historical periods, or in other cultures, used therapies; but by our present paradigm we would not be willing to say that they were justified in considering these measures to be therapies. We could still note that these physicians could have been considered justified by reference to the paradigm under which they were operating; the problem of cross-paradigm criticism and justification is a general problem in history and philosophy of science, and is not peculiar to this definition or to the placebo problem.

By contrast, we might envision a culture which related the cause of all disease to transgressions against basic social mores, and for whom therapy was seen in terms of atonement or expiation. This disease-therapy paradigm could be internally consistent, and could have social-cohesiveness value as well, even to the extent that whether a particular therapy ever did any good for the individual patient in an empirically verifiable way might be irrelevant. This culture might offer a definition of 'therapy' which would be radically different from ours,

but which in its own way would be equally paradigm-dependent.[17]

The acceptability criterion implicit in the definition of 'therapy' has an important implication-- that one is never justified in considering an intervention to be a therapy, in the absence of an accepted theory of pathophysiologic mechanism, based on observation of only one patient. This attitude is consistent with the present unwillingness to accept anecdotal evidence in medicine. If, then, we are later to define 'placebo effect' in terms of a sort of therapy, it would follow that to ascribe the placebo effect to one patient is implicitly to formulate a hypothesis about a class of patients.

Turning to the next problematic term:

T is a *specific* therapy for condition C if and only if:
1) T is a therapy for C
2) There is a class A of conditions such that C is a subclass of A and that for all members of A, T is a therapy
3) There is a class B of conditions such that for all members of B, T is not a therapy; and class B is much larger than class A.

An example might be penicillin used for pneumococcal pneumonia. Penicillin is a therapy for this disease, since it increases the empirical probability of recovery. Pneumococcal pneumonia is one of a class of diseases (infectious diseases caused by penicillin-sensitive organisms) for all of which penicillin is a therapy; but there is a much larger class of diseases (noninfectious diseases, and infectious diseases caused by viruses, rickettsiae, etc.) for which penicillin is not a therapy. Therefore penicillin is a specific therapy for pneumococcal

17. I am suggesting here that the notion of "therapy" is connected very intimately with that of "disease," a point I cannot argue for here. Unfortunately recent philosophical inquiries into the concepts of health and disease have almost totally neglected this point.

pneumonia.

'Specific' is used in several ways in medical discourse, and this definition is consistent with what one might call the loose sense--the sense in which "specific therapy" might be contrasted with "general therapy." It should be noted that (2) does not require that C be a proper subclass of A, so C may be identical with A (*i.e.*, the therapy is specific for one condition only, such as iron for iron deficiency anemia).

There is also a stronger sense, however, in which "specific therapy" is roughly equivalent to "best therapy." By the definition given above, penicillin would be a specific therapy for *Escherichia coli* infection, since penicillin is better than no therapy. But in practice physicians would not refer to penicillin as a specific therapy for *E. coli*, since the organism is four to five times more likely to be resistant to penicillin than it is to ampicillin, for example. To deal with this usage we might introduce an additional definition for 'preferred specific therapy.' For a therapy to be a preferred specific therapy it would have to be a specific therapy as defined above; and there would have to be no therapy T' which offered a better risk-benefit ratio than T for C, taking into account both therapeutic efficacy and absence of significant side effects. However, for the placebo context we will not need this additional definition. (Nor will we need the still stronger sense of 'specific' in which a therapy is not only known to be best empirically, but could also have been predicted to be best based on an established pathophysiologic theory-- for example, vitamin B_{12} as a specific therapy for pernicious anemia.)

We can now combine these elements into a definition of the placebo

effect:

> A *placebo effect* occurs for person x if and only if:
> 1) x has condition C
> 2) x believes that he is within a healing context
> 3) x is administered intervention I as part of that context, where I is either the total active intervention or some component of that intervention
> 4) C is changed
> 5) The change in C is attributable to I, but not to any specific therapeutic effect of I or to any known pharmacologic or physiologic property of I.

The mention of belief in the healing context, and the possibility that I may be only one component of the total healing intervention, reflect the boundary conditions discussed in 2.2.[18] The definition, like Wolf's and Shapiro's, allows for both positive and negative changes in C. "Not attributable to any pharmacologic or physiologic property of I" excludes changes due to diet or other nonspecific therapies. To whatever extent psychotherapy can be shown empirically to be efficacious, it is also a specific therapy and so is excluded even though it has no "pharmacologic of physiologic" effect. The word "attributable" may be interpreted in light of our discussion of 'therapy' and the acceptability criteria implicit in the current medical paradigm; it also refers to the present state of medical knowledge, and leaves open the possibility that newly-discovered properties of I may cause us to change our view that C was modified by the placebo effect.[19] It is even conceivable, from the form of the definition, that everything we

18. Since what counts as a healing context depends on the culture of the individual, inclusion of this term in the definition means that the placebo effect is inherently culture-dependent (Riley 1976).

19. Note that I may not be known specifically-- in the sugar-pill case, the cause of the symptom change is assumed not to be the chemical content of the pill, and no other medication is known to have been used, so *some* other element of the total episode is assumed to be responsible.

now attribute to the placebo effect will someday be attributed to new laws of medicine, leaving 'placebo effect' without reference. There are thus two very different epistemic elements in our definition-- the belief state of the individual subject regarding the healing context (which is culture-dependent), and the belief state of medical science regarding what can be explained by existing laws and theories (which is paradigm-dependent).

It does seem unsatisfactory that 'placebo effect' has been defined by exclusion, as something not attributable to other things. Why not an inclusive definition, such as one attributing the placebo effect directly to the psychological component of the healing intervention? Certainly in practice "not attributable to known pharmacologic or physiologic properties" could amount simply to "attributable to psychological properties"; but it could also mean attributable to presently unknown pharmacologic properties, or to some completely different sort of property. This seems to be a matter best left for empirical research. Further, if one framed an inclusive definition but left out mention of what sort of property to which the placebo effect was to be attributed, it would reduce to a definition of a nonspecific therapy, and one would be unable to distinguish the placebo effect and the effects of diet or exercise.

Significantly, I have offered a definition of 'placebo effect' without having given a definition of 'placebo.' This is in keeping with the trend we have already noted several times, of looking at the total context instead of at the inert medication. Thus, as Case 4 showed, we can apply the term 'placebo effect' to instances where no placebo is in evidence. In such cases one can label the purported causative

component of the intervention the "placebo stimulus" to emphasize this distinction. We can then be satisfied with a more traditional, restricted definition of 'placebo' proper:

> A *placebo* is:
> 1) a form of medical therapy, or an intervention designed to simulate medical therapy, which at the time of use is believed not to be a specific therapy for the condition for which it is offered, and which is used either for its psychological effect, or to eliminate observer bias in an experimental setting.
> 2) (by extension from 1) a form of medical therapy now believed not to be efficacious, though believed efficacious at the time of use.

Clause (2) is added to make sense of a sentence such as, "Most of the medications used by physicians one hundred years ago were actually placebos." One of the epistemic elements from the definition of 'placebo effect' reappears, the mention of the present belief state of medical science. Where a placebo is used for therapy, we can assume that the second epistemic element is present also, since to have a "psychological effect" the therapy must be believed to be such by the recipient (as Case 7 illustrates). But this element of belief may be lacking in the experimental setting, which is another important use of 'placebo.'

I have argued that a definition of 'placebo effect' ought to aid and stimulate research. The definition given above fills the bill. It asks the question: if the change in symptomatology is not attributable to known pharmacologic or physiologic properties of the intervention, to what is it attributable? At the same time, it avoids closing off lines of research by *a priori* stipulations of what sorts of properties to consider. But beyond the empirical questions, I am concerned with the philosophical significance of the definition. One important line of investigation is suggested by the possibility that psychological

mechanisms might be producing bodily changes, and that the belief state of the subject is a necessary condition for this to occur; this would seem to have important consequences for theories of the mind-body relationship, which will be taken up in the next three chapters. A different line of investigation is the ethical question of the use of placebos as therapy; and Chapter 6 will show that the formal definition given above has significance for that issue also.

Chapter 3. Traditional Mind-Body Views and the Placebo Effect

Having reviewed the empirical data on the placebo effect and having formulated a definition of this phenomenon, we may now ask what implications this line of inquiry has for the mind-body relationship. By itself, the placebo effect raises interesting questions about philosophy of mind; but in addition, to the extent that *mens sana in corpore sano* is a goal of medical practice, these questions are central to any philosophy of medicine.

This chapter and the next two will be devoted to mind-body issues. This may seem to be a disproportionate amount of attention, especially since much of this present chapter will be devoted to listing possible theories only to reveal later their weaknesses and defects. But in fact, although all proposed mind-body theories have flaws, very few of them are outright nonsense; almost all theories capture some portion of the complex of intuitions that we hold about our bodies and our minds. In general, theories fail, not by failing to capture and to illuminate the intuitions to which they are addressed, but rather by failing to take into account other, equally basic intuitions. Thus, reviewing many alternative theories will place us in a much better position to examine critically the theory we will, in the end, find most satisfactory, even if in the process we are led somewhat far afield from the placebo effect itself.

Despite the large amount of space being devoted to the mind-body issues (as compared, say, to the ethical issues in Chapter 6), it will be necessary to skim rather lightly over many possible areas of controversy, and to summarize in rough-and-ready fashion philosophical arguments that are very complex in their full development. Thus, the following discussion may suggest a wider agreement on many philosophical points than is actually the case, as examination of the references cited will readily show.

This chapter begins by reviewing the "reflective equilibrium" strategy mentioned in the Introduction, as it relates specifically to the mind-body issue. The next section provides an overview of traditionally accepted mind-body theories. In the third section, several of these theories which seem plausible will be applied to the placebo effect, and the grounds for their plausibility will be explored; but in the final section, significant defects in each of the theories will be shown. It is by attempting to modify such theories to eliminate these defects that an even more plausible theory, eliminative materialism, emerges; that theory will be discussed in the next chapter.

3.1. A Reflective-Equilibrium Approach to Mind

If we accept the notion that the accumulated data about the placebo effect require some sort of explanation in terms of how the mind and the body are related, and that this is a matter for philosophical analysis rather than for additional empirical research alone, we can approach the task of explanation in different ways. One is to seek out the view of the mind-body relationship which best explains the placebo effect as an isolated phenomenon, or which at least does not conflict with any of

the known empirical findings. Whether this view accounts for mind-body issues not directly raised by the placebo data (for instance, the question of whether minds can exist apart from bodies) would be considered irrelevant by this approach. This approach is consistent with the pragmatic, task-oriented way in which physicians and medical scientists have approached the mind-body problem, when they have approached it at all.[1] Thus, one finds in the medical literature proposals for "double-language theory" (Graham 1967), holist emergentism (Wolff 1962), and "methodological dualism" (Boss 1975). But as a rule, these accounts deal with medically-related issues only at the expense of other features of a comprehensive philosophy of mind-- whether minds can exist apart from bodies, whether we can know that there are minds other than our own, and so on.[2] If a philosopher notes that a medical mind-body theory raises problems and conflicts in these other areas, it seems as if philosophy is simply raising impediments to medical research and progress. Small wonder under these circumstances that medical people might come to regard the "mind-body problem as philosophically senseless and scientifically wasteful of time and effort" (Freedman, Kaplan and Sadock 1972, p. 432).

An alternative approach is the "reflective equilibrium" strategy

1. See, for example, Engelhardt's analysis of the research-connected motivations that led the 19th century neurologist John Hughlings Jackson to adopt the doctrine of parallelism (Engelhardt 1975a).

2. An exception is the approach taken by the philosopher-physician Tristram Engelhardt. His more sophisticated theory, in the Kantian-Hegelian tradition, takes mind and body to be two separate domains of significance, such that attempts to relate them causally constitute category mistakes. On matters such as psychosomatic medicine and the placebo effect, his views seem to be a type of epiphenomenalism; but this may be my misreading of his position (Engelhardt 1973).

described in the Introduction. On this view, the task is to find the overarching theory that best makes what we know about the placebo effect hang together, in a consistent and mutually illuminating way, with other conceptual considerations regarding mind and body. Our particular concern with the placebo phenomenon should not make us forget that we have many basic considered judgments about mind and body. These might include, for example, our certainty that we need no grounds to ascribe a sensation such as pain to ourselves-- we simply *are* in pain, we do not infer that we are based on evidence-- while we do need grounds to ascribe it to others; yet we unhesitatingly treat another who is in pain as if he has the *same* sensation that we do when in pain ourselves. So we want a mind-body theory that deals adequately with the placebo effect, but we also require that our theory "fit" with basic considered judgments of the sort mentioned. We are willing to work from both ends, either giving up some fineness of grain regarding the placebo effect in exchange for better overall "fit," or sacrificing some degree of "fit" for a theory which promises to highlight the placebo effect in a particularly illuminating way. If the search for this kind of broadly-based theory fails, we may then wish to accept a narrow, medically-oriented view. But since, in the course of searching for the best "fit," we might find our considered judgments about other matters giving us new insight into the placebo effect, and *vice versa*, it would be a methodological mistake to settle for the narrow theory without making some attempt to search for a more comprehensive one first.

The mind-body theories that we will consider, then, will be looked at both from the standpoint of the placebo effect and from the standpoint of our basic considered judgments. It will turn out in many cases

that different theories give equally adequate, if equally vague, accounts of the placebo effect; thus the basic considered judgments will play the larger role in ranking the different theories according to their philosophical plausibility.[3]

3.2. Overview of Alternative Mind-Body Theories

Almost all views of the mind-body relation assume that there is a significant and basic difference between statements about sensations, volitions, thoughts, memories, etc., and statements about the structure and function of physical bodies.[4] Originally, Descartes characterized mind as thinking and unextended (*i.e.*, neither occupying nor moving through physical space), and body as unthinking and extended; a human person was seen to consist of a mind plus a body.[5] While mind was originally thought of as nonmaterial substance, difficulties with this concept have been avoided by speaking instead of mental events. Mental events differ from physical events in that we have some sort of noninferential access to some of them (*i.e.*, our own), so that as a rule we cannot be mistaken about them; and in that mental events are not localizable in space in the precise way that physical events are.

3. That mind-body theories give us vague accounts of the placebo effect should not by itself count against them; we would not want philosophical theories to fill in details that can properly be provided only by further empirical research.

4. This section follows roughly in its organization (Shaffer 1967). For an overview of significant contemporary positions on mind-body, within the Anglo-American tradition, see (Chappell 1962; Shaffer 1965).

5. For the original statement of this position see (Descartes 1927, pp. 145-165); (Spicker 1970, pp. 3-23) provides a summary of the problems that it raises.

Clearly, speaking of mental events in general requires that we lump together such very different things as smelling an unpleasant odor and thinking about a differential equation. While much of the discussion that follows will be based on such a lumping together, it will be useful to distinguish two important types of mental events, sensations and intentional states. A rough way of making the distinction is to note that sensations include events such as hearing a bell, feeling a pain, seeing a bright color, and so on; they often correspond to something "out there" but not necessarily, as in cases of hallucinations and after-images. Intentional states include believing that the Battle of Hastings was fought in 1066, thinking about the predictability of earthquakes, and fearing that you are going to hit me; these cannot be described completely without mentioning the object (often a proposition introduced by the word 'that'), and the object need not be present or may not even exist-- I can think about Moses or about unicorns. Also, as a rule, sensations are a more primitive sort of event; all sentient animals can have them while only more complex organisms can have intentional states. As we might expect, some mind-body theories give good accounts of sensations while having difficulty with intentional states, while other theories have the opposite characteristics; in particular, intentions seem more susceptible to behavioral analysis than sensations are.

If we recognize the mental and the physical as distinct and primitive types of events, we can deal with their relationship in two ways. We may choose a *monistic* theory, which either recognizes the essential reality of only one type of event, or else tries to derive one type of event from the other, or both types from some third type. Or we may

select a *dualistic* theory which recognizes both types of events as equally basic and seeks to explain their relation without slighting either.

One sort of monistic theory regards one type of event as totally dependent upon and arising secondarily from the other. *Idealism* attributes reality only to mental events and regards the physical world as totally dependent upon our mental images of it. A tree, for instance, would exist only as the object of someone's perception, and would not exist at all if someone were not at that moment perceiving it. Idealistic theories are seldom proposed today. Much more popular is *materialism*, which holds mental statements to be about certain physical events which occur in the brain. For instance, our seeing a tree consists of photons of light striking our retinas, which then excite neurons to discharge, thus exciting other neurons, and so on. When we have described all these physical events, we have said all there is to say about "seeing a tree"; there is nothing mental "over and above" these physical events. *Behaviorism*, which we can regard for our purposes as a form of materialism, seeks to reduce all statements about mental events to statements about the publicly observable behavior of organisms. The recent successes in neurophysiology research and in operant-conditioning psychology have made materialism and behaviorism, respectively, seem especially credible.

Some confusion is avoided if several forms of behaviorism are distinguished. *Methodological behaviorism* is a statement of research strategy for scientists, which says essentially that one can discover interesting, lawlike regularities by investigating the behaviors of organisms. It is fully consistent with methodological behaviorism that

there could exist mental events apart from any observable behavior; such events would simply be excluded from scientific inquiry. Thus, methodological behaviorism is of limited philosophical interest. We shall be concerned later in this chapter with *logical behaviorism*, which makes a metaphysical assertion which is held to be a general truth, namely, that mental events can be understood in a coherent way only if they are taken to refer somehow to publicly observable behavior. A still more sophisticated metaphysical thesis is *radical behaviorism* (Skinner 1974), which will be discussed in the next chapter.

Other monistic theories attempt to slight the status of neither the mental nor the physical. *Identity theory* agrees that talk about mental events cannot be reduced to talk about brain events; rather it asserts that these two kinds of talk, though having different meanings, in fact refer to identical happenings-- that the claim that mental events are contingently identical with brain events of the appropriate type is a coherent and empirically testable hypothesis. *Double-aspect theory* holds that the mental and the physical are different aspects of some third kind of substance; this theory founders on what that third substance might be like, and how mind and body can be "aspects" of it, or of anything else.

Dualistic theories are conveniently characterized by the types of causation that each admits. *Parallelism* holds that physical events can cause other physical events, and that mental events can cause other mental events, but neither cause the other, even though certain mental events seem to be constantly correlated with certain physical events. But on this theory, such a constant correlation is a highly mysterious coincidence; in the absence of causal connections, it is hard to see

why a broken bone might not be correlated with pain one time and with joy another time. Some philosophers have brought in divine intervention to explain the coincidence, but this is to offer an explanation which is even less understandable than the phenomenon being explained. Thus parallelism is usually rejected.

Epiphenomenalism holds that physical events can cause other physical events, and that some physical events (occurring in the brain) can cause mental events; but the mental events can cause nothing. Epiphenomenalism seems to acknowledge our considered judgment that our inner mental states are real occurrences, and that they are reliably correlated with certain physical events, while avoiding the sticky problem of how nonphysical, nonspatial mental events can cause physical events. But it ignores our equally basic considered judgment that our mental events, such as acts of will or of deliberation, do cause things to occur in the world. It also requires laws of physicopsychic causality to be of a strange sort, in that the postulated effects simply "dangle" and play no further role in any causal chain.

Interactionism holds that physical events can cause both other physical events and mental events, and that mental events can cause both other mental events and physical events. This satisfies our considered judgment about the causal efficacy of our mental states, but demands that we face squarely the puzzle of psychophysical causality.

We can see that some of the above theories can be dismissed more readily than others. Behaviorism, interactionism, and identity theory seem to have enough initial plausibility to warrant further consideration.

3.3. Some Initially Plausible Theories

In determining which of the mind-body theories should be investigated in depth, we might ask how they would account for the placebo effect, and which considered judgments seem to support them. To apply mind-body theories to the placebo case, we can return to the formal definition from 2.3 and restate it in the form of antecedent and consequent events. The antecedent events are that the individual has a symptom, that he believes that he is in a healing context, and that he is administered an intervention. The consequent event is that the symptom is changed. An additional observation is that the change cannot be explained on the basis of specific properties of the intervention or of pathophysiologic laws as now known. The link between antecedent and consequent will generally be construed as causal, although this need not necessarily be so.[6]

Attempts to apply classical conditioning theory to the placebo effect (1.5) suggest the possibility of a behaviorist account. Such an account would have to construe all the antecedent and consequent events in terms of publicly observable behavior. The potential problem areas are giving behavioral accounts of subjective symptoms such as pain, and of believing that one is in a healing context. The usual method is to account for these in terms of dispositions to behave, such as, "I am in pain" means "I am disposed to yell, pull away, etc." If these

6. The view that all medical thinking is necessarily causal derives from our own dominant paradigm. The entire, complex system of ancient Chinese medicine was essentially noncausal (Porkert 1977). In the paradigm dominant in 18th century Europe, recognition of disease was based on the concept of a "motionless, simultaneous picture" (Foucault 1975, pp. 3-16, 188-189).

strategies are acceptable, then the behaviorist account can be completed. Since behaviors occur within the realm of physical events, the causal connection between antecedent and consequent events presents no problem.

Causal interactionism would view the placebo effect as a case of a mental event (believing that one is in a healing context) and some physical events (the intervention, the existing bodily state) causing another mental and/or physical event (the subjective and/or objective symptom change). This entails causality between mental and physical events, and we have seen that this needs at least some further explanation.

Identity theory would essentially accept the account given for the interactionist view, but would add that the mental events referred to are in fact identical to certain physical events in the brain; seeing this eliminates the puzzle over causality. For research purposes, we would presumably want to learn which brain states are identical to the mental states referred to, so that we could then study their connections on a neurophysiologic basis; we could then learn the precise nature of the causal network. Indeed, if this research led to our adopting new "laws of pathophysiology," the placebo effect would cease to be unexplainable in terms of those laws and hence would cease to be the placebo effect as we have defined it. Given the methods of modern neuroscience, such a research program does not seem impossible.

It is not only the case that each of these three theories manages to account for the placebo effect; in addition, each can point to basic considered judgments that support it. One such judgment is that we rely heavily on the behavior of others to determine what thoughts, beliefs,

and sensations they are having. Indeed, even though we generally feel that a person cannot be mistaken when candidly reporting his own mental states, we may on occasion reject a first-person report of another on behavioral grounds, as when a person, red in the face and with fists clenched, shouts, "I'm not angry!" This seems to support behaviorism. Another considered judgment is that our increasing knowledge of neuroscience does in fact tell us interesting and informative things about the mind; in particular, it tells us that certain mental events are in some way dependent upon certain brain events, as when an electrode implanted in a selected brain site reliably stimulates a feeling of pleasure or a specific memory trace. This considered judgment seems especially compatible with identity theory. Finally, interactionism is supported by the two considered judgments referred to earlier-- that our inner mental states have undeniable reality and causal efficacy.

Thus, the three theories are each prominently but not uniquely supported by certain considered judgments. For the committed proponents of one of these theories, the importance of the considered judgments is likely to be exaggerated. Instead of being merely *a prominent* feature of what we mean by mental events, the considered judgment that supports one's own pet theory is likely to be seen as *the crucial* feature of the mental realm. Thus, it is important to subject these three theories to more critical scrutiny, especially taking note of the problem areas that have been mentioned.

3.4. Rejection of Commonly Held Theories

Each of the three theories considered in the previous section can be shown either to conflict with other considered judgments, or to give rise to troublesome conceptual puzzles. We will, it seems, have to look farther afield for a satisfactory theory to account for the placebo effect.

Behaviorism holds that descriptions of any psychological state can be reduced to descriptions of behaviors that are publicly observable in principle.[7] Therefore, if we can find any psychological states which cannot be so reduced, we will have raised serious doubts about the doctrine of logical behaviorism (however useful methodological behaviorism might remain as a working hypothesis in psychology). It is useful to focus on the mental-state report, "I believe that I am in a healing context," as our example (passing over for the moment the fact that "healing context" is an abstract concept unlikely to arise in daily conversation).

Attempted behavioral accounts of belief states commonly take forms such as dispositions to behave or dispositions to make assertions. Such accounts of our mental-state report might be:

1. I am disposed to follow instructions given by the healer, to allow examination of my body, etc.
2. I am disposed, if asked, "Are you in a healing context?" or the equivalent, to answer affirmatively.

7. Such behavior need not be readily observable in practice for the behaviorist to make his metaphysical case. For example, some have attempted to analyze thought in terms of subvocal laryngeal contractions.

But these accounts as they stand are incorrect. I may believe that I am in a healing context but not be disposed to act in the appropriate ways if, for example, my fears of the medical procedures outweigh my desire to be cured. And I might believe that I am in a healing context but not be disposed to answer a question to that effect if, for instance, I have a desire to deceive the questioner. We could, it is true, expand our account to include such qualifiers: "...disposed to... *if* I have no desire to deceive, *if* I understand the question put to me, etc." But such an expanded account is no longer behavioristic, since mental terms such as "deceive" and "understand" have crept into it. If in turn we try to give a behaviorist account of "deceive," we will have to add similar qualifiers which include mental terms of their own, and so on. Thus it would seem that any behaviorist analysis of this sort will either be incomplete, or will include unreduced mental terms in the analysis itself (Chisholm 1957, pp. 168-173).

Further reflection suggests that this problem reflects a general feature of behaviorism, and is not the result of the particular examples that we chose. For instance, "knowing that..." involves being disposed to answer certain questions correctly *if I want to, if I am not confused,* etc.; and "wanting to answer," in turn, involves being disposed to answer correctly *if I know the answer, if there is nothing else I want more,* etc. It seems to be a necessary feature of psychological states that they can be characterized *completely* only in terms of their relations to other psychological states, although they can and indeed must be characterized *in part* in terms of observable behavior. Thus, no psychological term can be characterized adequately in such a way as to eliminate all psychological terms from the explanation

(Putnam 1964)-- any more than we can describe the relation, "the tree stands to the right of the boulder," merely by describing the structure or the behavior of the tree itself.

If behaviorism must be rejected as an adequate account of belief states, we must also reject classical conditioning theory, with its simplisitc stimulus-response characterization, as an adequate placebo explanation. This runs counter to the assertion that experiments showing a "placebo effect" in animals provide empirical support for conditioning theory (Byerly 1976). Can an animal believe that it is in a healing context? We can attribute to animals concepts whose presence can be manifested by non-verbal recognition; a dog can show by his behavior that he believes that his master will be coming home soon (Kenny 1976, p. 51). But the concept of a healing context seems to be an abstract concept not open to this possibility. We must conclude that what was seen in the animal experiments was not the "placebo effect" as we have defined it. It may still be the case, however, that certain limited features of the healing setting can become conditioned stimuli, evoking responses in both animals and humans.

The problems with behaviorism are avoided by interactionism, since the latter theory explicitly includes mental terms. But interactionism gives rise to two problems of its own. One, already alluded to, is the puzzle of causality between the mental and the physical. We are used to accounting for causation in terms of one body exerting a force on another, or in terms of transfer of energy. But if one event occurs in a body which has mass and can move through space, and another event occurs in something nonsubstantial and nonspatial, it is hard to see how any causal connection could exist.

The notion of 'cause' used here is essentially a Newtonian one; and Gasking (1955) has suggested that this is not the primitive or the root meaning of 'cause.' This primitive meaning he takes to be that of a recipe for producing a certain effect-- A causes B when one can produce a state or event of the A sort as a means to producing a state or event of the B sort. The sense of 'cause' that appears in the Newtonian or scientific context is properly viewed as a special case of this root sense.[8] But the price we pay for adopting a looser sense of 'cause' is to give up the powers of explanation and prediction that accompany 'cause' in the stricter sense.

Still, the causality puzzle might be tolerable if there were not another serious problem with interactionism. If I consist of a mind plus a body, and if thought and consciousness are properties only of the former, it is quite possible for me to conclude that my mind is the only one that exists. I do in fact see many other persons, but I see only their bodies and never their minds; for all I know they may be cleverly constructed automata which have no thoughts or consciousness. But certainly the possibility that I could have grounds for thinking this runs counter to our basic considered judgments. It has been argued that I know others have minds by analogy from my own case; but such a use of analogy would be inappropriate. Having seen, for example, the internal wiring and gears in many railway semaphores, I could conclude by analogy that the next semaphore I encounter will have such an internal structure. But since minds have no physical substance, I could never in principle check out my assumption about other minds existing,

8. But see 5.2, below, for a refinement of Gasking's position, suggesting that there is no one "root sense" of causation.

in the way that I can check out a railway semaphore (Ryle 1949, pp. 51-56). Interactionism, then, seems to take a considered judgment about which we feel firmly convinced (*i.e.*, that other people have minds like ours) and to relegate it to the status of something we must take purely on faith and can never in principle be certain about. Any reasonably plausible mind-body theory that avoids this troublesome other-minds problem would therefore be preferable to interactionism.

Identity theory, in turn, avoids the problems that attend both behaviorism and interactionism; but it avoids these by postulating an identity relation of a sort that requires considerable scrutiny. An important feature of the identity relation is that anything that can truly be said of one term of the relation can truly be said of the other. We can say "the Morning Star is identical to the Evening Star" because any property of the Morning Star (size, position in space, etc.) can truly be predicated of the Evening Star, and *vice versa*. But the mind-body problem has arisen precisely because things that can truly be said of mental events (nonspatial, noninferential access to our own, etc.) cannot be said of physical events. We might try to reformulate our concepts of physical and mental events to remove some of these differences, but we would succeed only by either "mentalizing" physical events or by "materializing" mental events (for example, by adopting a linguistic convention that allows us to locate mental events precisely in space). In either case, how one sort of event could take on properties of the other would be as puzzling an issue as how the mind is related to the body; so we would not have succeeded in clarifying the mind-body problem.

Another feature of identity relations is that two things can be said to be identical only if they are of the same sort. This follows

from the way that we define physical space-- two things of the same sort cannot be in the same place at the same time, unless they are identical. Two things of different sorts can occupy the same space at the same time-- a tree may be in the same place as an aggregate of cellulose molecules. But in this case we would say that the tree is constituted of cellulose molecules, not that the tree is identical to the aggregate of molecules. For one thing, we can truly ascribe properties to the tree that we cannot to the molecules, and *vice versa*; we can, for instance, talk of the mean kinetic energy of the molecules but not of the tree. Also, the tree and the aggregate of molecules have different conditions for survival through time. If the tree is cut up into logs the aggregate of molecules survives but the tree does not; if the tree is pruned and the clippings burnt, the tree survives but the aggregate of molecules does not (Wiggins 1968).

Thus, for two things to be identical there must be some "sortal concept" that applies to both; for the Morning Star and the Evening Star it is the concept "planet." The sortal concept is important because it tells us where to look to see if the identity statement is true or not.[9] To see whether the Morning Star is identical to the Evening Star, we first trace one planet through space, then the other, to see whether we have traced the same planet or two different ones. But what sortal concept could serve this function for mental events and physical events? It cannot be a very vague concept such as "event" or "phenomenon"; because then we have no clear idea what to trace. Something that occurs, such as raising my arm, could be viewed equally well

9. I owe my understanding of this refinement of Wiggins' analysis to an unpublished paper by Martin Benjamin.

as one event or many events, depending on our purposes (it could be one arm movement, or the simultaneous contraction of many muscles). But if the sortal concept is made definite enough to trace through space or time, it would have to take on either physicalistic or mentalistic properties, and hence would not apply equally well to the two terms of the identity statement.[10] We must conclude from this that the proposed mind-body identity assertion, despite its straightforward appearance, in fact conceals a number of sticky problems; it is not at all clear that the assertion is a coherent or meaningful one.

We have thus found serious problems with all three of the mind-body theories that seemed initially plausible. But this does not rule out the possibility that one or more of them could be modified so as to avoid some of the criticisms. By making some major modifications in behaviorism, on the one hand, or identity theory, on the other, one can arrive at a position called eliminative materialism, which agrees well with the considered judgments noted above and which is immune to several of the criticisms we have listed. This will be the focus of discussion in the next chapter.

10. James Cornman, "The Identity of Mind and Body," in (Borst 1970, pp. 123-129) argues for "cross-category" identity, such as "the temperature of the gas is identical to the mean kinetic energy of its molecules." But if the identity is truly cross-category, there can be no common sortal concept, and the identity statement is incoherent. Indeed, in Cornman's example, "identical to" seems strained at best; "directly proportional to" is much more natural.

Chapter 4. Eliminative Materialism

The refutations offered in the previous chapter for behaviorism and for identity theory will not satisfy many defenders of these theories, who might object that we have looked at these theories only in their weakest forms. More recent authors, it will be stated, have modified these theories so as to make them immune to refutation on the grounds we have mentioned. This chapter will be largely devoted to an analysis of this claim.

The first section looks at features of these modified theories, under the title of "eliminative materialism," once again borrowing from Kuhn's idea of paradigms as stated in 2.1. The second section attempts to provide arguments against eliminative materialism as a satisfactory mind-body theory. One argument in favor of eliminative materialism is that the only alternative to such a theory is one of the types of dualism that were found to be unsatisfactory in the previous chapter; so the final section will show that another alternative is available, by going outside of the Cartesian tradition. The alternative approach, Strawson's concept of 'person,' will provide a logical framework for more detailed development in the next chapter.

4.1. Features of Eliminative Materialism

What I will be calling eliminative materialism has developed out of identity theory and behaviorism, in response to some of the criticisms mentioned in the previous chapter. Identity theorists, noting the

failure of attempts to translate mental-state talk into brain-state talk, and observing the sort of *ad hoc* reformulations of mental and physical characteristics that would be required to make the identity assertion appear coherent in its original form, have moved to a "disappearance form" of identity theory. On this view, as we learn more about the neurophysiology of the brain, we will simply adopt the language of science in talking about our own internal experiences, and traditional mentalistic talk will "disappear." Instead of saying, "I have a pain," we will say, "My C-fibers are firing"; talk about pains will drop out of our language in the same way that talk about demons has dropped out of our talk about disease. And the new language will offer greater economy, as the same terms which we will use to describe our everyday experiences will also function in scientific observation and theory-building.[1]

A similar advance has been made in behavioristic thinking. As operant-conditioning theories have become more sophisticated, views of what is to count as "behavior" have broadened to include various "inner" bodily states, and the past history of the organism has been taken into account along with present states. An example of such a sophisticated theory is the "radical behaviorism" of B. F. Skinner (1974).

A follower of Skinner, for example, would argue that in refuting classical conditioning as a plausible placebo theory (3.4), we have in effect demolished a straw man, since psychologists have long since

1. Representative papers on the "disappearance form" are Richard Rorty, "Mind-Brain Identity, Privacy, and Categories," and Paul Feyerabend, "Materialism and the Mind-Body Problem," (Borst 1970, pp. 187-213, 142-156).

abandoned classical conditioning for the more refined operant conditioning. An operant-conditioning account of the placebo effect might go something like this. Suppose that there is a certain bodily state (analogous to alpha rhythm, for example) such that the self-healing powers of the body work best when the body is in that internal state. Upon repeated exposures to the healing context, achievement of that state will be positively reinforced by quicker relief of symptoms. Over time, the individual might become conditioned to achieve that state upon being presented with the healing context as a stimulus; this will occasionally result in relief of symptoms even if no active intervention is given in the healing encounter. Further, instead of asking the circumstances needed for the individual to believe that he is in a healing context, we might ask about the degree of "stimulus generalization" present in this case of conditioning; the latter question is open to precise study and quantification.[2]

Despite important differences, it is useful to consider the "disappearance form" of identity theory and radical behaviorism together. First, it must be seen that the "disappearance form" is really no longer a form of identity theory at all. Using the demon analogy, replacing demons as the purported causal agents in disease with a germ theory is not to say that demons are identical to bacteria; it is to say that, in the past, when we talked about demons, we were hopelessly confused; and we should change our account so that it reflects the facts as we now know them. In both radical behaviorism and the disappearance form, the suggestion is made that we *eliminate* our traditional mentalistic

2. I am indebted to Joseph Hanna for pointing out to me the possibility of such an account.

talk in favor of language that (it is asserted) is more scientifically correct. (Radical behaviorism does attempt to give new meanings for our present mentalistic terms and advocates retaining such terms in the language; but the change in meaning is so drastic that it amounts practically to eliminating the terms as we use them.)[3] And this new language will be *materialistic*, in that it will make reference only to physical states and events and will seek to explain human behavior in terms of deterministic laws akin to the laws of physics and chemistry. Hence the title, "eliminative materialism," for the combination of both theories.[4]

Eliminative materialism must be understood as a radical reconstruction of our notion of mental events, as contrasted to previously discussed theories, which were rather attempts to explicate the notion. It is this radical-reconstruction feature that allows eliminative materialism to escape the criticisms leveled at behaviorism and identity theory in 3.4. Behavioristic attempts to deal with the problem of belief states, for example, failed, because the behaviorist attempted to give an account that would capture all of what we presently mean when we talk of beliefs as mental states. And the identity theorist, in order to make his identity statement seem coherent, was tempted to try to

3. Obviously the radical behaviorist's use of mentalistic terms with radically modified meanings makes argument in this area especially difficult; and to some extent, as we will show below, the plausibility of the behaviorist's position depends on this ambiguity. This sort of language problem is a general feature of cross-paradigm debates in science (Kuhn 1970, pp. 198-204).

4. Further justification for combining the two theories is the fact that radical behaviorism seems especially strong in accounting for intentional states, while the "disappearance form" is most credible in dealing with sensations.

impose mentalistic features on physical events, or physical features on mental events. By being able to drop mentalistic talk completely, the eliminative materialist can avoid being backed into such corners. The objection, "But what you have just described doesn't include everything that is included when we talk about beliefs (or sensations, or thoughts, etc.)" is simply no longer relevant.

What the eliminative materialist is proposing may usefully be compared to the idea of a paradigm shift in science (2.1). For example, to say that when chemistry adopted the oxygen theory of combustion in place of the older phlogiston theory, chemists adopted a new terminology, is to miss the actual extent of the revolution in thinking. There is an important sense in which the oxygen chemists were observing different data and studying different problems as contrasted with the older state of the science. Further, since one cannot work within a paradigm without accepting its set of basic presuppositions, cross-paradigm disputes are at least to some extent insoluble. Neither the oxygen chemists nor the phlogiston chemists could, in this sense, win over the other side by arguments as to the superiority of their theory, since they would in effect be arguing about two different things; each side could accuse the other of question-begging in the way that they have stated their theory.

Replacing "I am in pain" with "my C-fibers are firing" represents a similarly radical paradigm shift. For instance, if we were to object that I can be mistaken about "my C-fibers are firing" while I cannot be mistaken about "I am in pain," the eliminative materialist would reply that the only reason we regard incorrigibility as an important feature of mental events is because we are totally immersed in our present mentalistic language. Our objection is analogous to one the phlogiston

chemist might raise: "Your oxygen theory is very nice as far as it goes, but it can't be correct because there is no room in it for the existence of phlogiston." However, despite the problems of cross-paradigm debates, we will see if some telling points against eliminative materialism cannot be raised.

While eliminative materialism is a strong theory precisely because it engages in this radical reconstruction, its supporters may sometimes be tempted, as a debating tactic, to downplay this feature of their theory, and to talk as if eliminative materialism were, after all, nothing but a minor modification of identity theory. After all, if the Morning Star is identical to the Evening Star, we have nothing important to lose by agreeing to call the planet by one name whether it appears in the morning or the evening; we do not have to give especially strong reasons for making this shift in language. Similarly, the eliminative materialist might play upon the confusion of the "disappearance form" with identity theory proper, to convince us that replacing "I am in pain" with "my C-fibers are firing" is a similarly inocuous terminological shift. But, as we will be arguing, we cannot let the eliminative materialist off the hook so easily when a radical paradigm shift is at stake. It will not do to say that elimination of our present mentalistic language ought to be carried out simply because such an elimination is conceivable.

Another point in favor of eliminative materialism is that the considered judgments listed in 3.3, which individually supported behaviorism, interactionism, and identity theory, respectively, combine mutually to support eliminative materialism. The theory accounts both for the emphasis on behavior in determining mental states, and the

importance of neurophysiological discovery in elucidating the "mind." And, assuming that the elimination of our mentalistic language can be carried out, we will be free to recognize the reality and the causal efficacy of the firing of our neurons. Furthermore, as already noted, eliminative materialism seems to be immune to the objections raised against the other three theories. Finally, and importantly from the medical standpoint, eliminative materialism, in calling for use of a more scientific language and for reduction of psychological explanations to deterministic and materialistic explanations, seems consistent with trends in contemporary medical science.

Given its sophisticated nature and the problems of cross-paradigm debate, it would seem difficult to launch a strong attack against eliminative materialism. The next section will take up this matter.

4.2. Objections to Eliminative Materialism

Essentially two kinds of arguments can be raised against eliminative materialism. The first tries to make it seem less plausible that brain-state talk could replace mental-state talk as readily as we might think; this type of argument does not directly confront the radical paradigm-shift feature of the theory. The second does confront the paradigm shift, and asks directly what we stand to gain or lose by making it. In particular, we might challenge the materialist emphasis on the purported scientific advantages of making the shift; in line with our reflective-equilibrium approach, we might demand that moral considerations be taken into account as well.

Replacing talk about mental states with brain-state talk seems most likely to succeed if there is a one-to-one correspondence between brain

states and what we now call mental states; at the very least, there ought to be a many-one correspondence, with any one of a set of brain states corresponding to a single mental state. But, at least with regard to intentional states, this "correspondence hypothesis" seems highly questionable. Goldberg (1968) takes the example "thinking about George Washington." Suppose a teacher asks three students to write down the name of the first President. A hears the question and immediately writes the words, "George Washington." B first has a mental image of the picture on the dollar bill, then recognizes it as the face of George Washington, and writes "George Washington." C has the same mental image as B, but fails to recognize the person by name, and so writes down nothing. If we are asked which students were thinking of George Washington, we must say that A and B were; although C had the right mental picture, we would not want to say that one can be thinking about George Washington and simultaneously not know that one is thinking about George Washington. At best, C was thinking about a picture of George Washington. But if we ask which students had the same thing going on in their heads (and presumably in their brains), the most likely answer is B and C. Thus we might well doubt that the relation between brain states and mental states is of the sort that makes the eliminative-materialist program a likely venture.[5]

Another line of argument can be raised against the suggestion that descriptions of the structure of the brain can replace psychological

5. The materialist might weaken this example by suggesting that it does not make us doubt the existence of a one-one or a many-one correspondence; it merely makes us doubt our ability to articulate it in ordinary language.

explanations with mentalistic content. Fodor argues that a psychological explanation must consist of both an analysis of behavior in functional terms, and a description of the underlying structure or mechanism that makes the behavior possible. Describing only the mechanism will not suffice, because for any functional description, there are an indefinite number of mechanisms capable of producing it (Fodor 1965). It so happens, for example, that the firing of C-fibers is the mechanism that corresponds with pain sensation in all human beings studied to date. But we could easily imagine some elaborate series of switches and wires that could be implanted in a body to serve the same function; and the number of different mechanisms we could postulate would depend only on our ingenuity. There is no necessary connection between the functional description and any specific one of these functionally-equivalent mechanisms. Further, a description of one such mechanism only would be merely a description of the interactions among the parts of the mechanism, and "would fail to describe the role of these interactions in the production of behavior" (Fodor 1965, p. 177).[6]

With regard to this point a moral consideration arises as well. We might imagine making contact with creatures from outer space, who might turn out to have psychological states analogous to our own, insofar as we could tell from their conversation and other behaviors, but whose bodily physiology might be completely different. Are we to regard these creatures as our moral equals, and accept moral duties not to kill them or to cheat them? Or do we regard them as wholly alien

6. Fodor here does not deny that functional explanations play a role within neurophysiology itself; but *vis-a-vis* the behavior of human beings, all neurophysiological explanation, whatever its internal form, is mechanistic.

life forms, refusing to believe that they could have psychological states similar to ours because their structural form is so different? Eliminative materialism seems to steer us toward the latter course.[7]

But these lines of argument will be rejected as irrelevant by the committed eliminative materialist. Both the correspondence argument and the functional-explanation argument, he might insist, still misperceive the radical paradigm shift, and reply to the materialist as if he were trying to give an explication of our traditional mental-state talk. Once the task is seen as one of radical reconstruction rather than explication, it will be seen to make no difference what the brain states correspond with, or what form psychological explanations ought to take. Thus we have to face the materialist proposal head-on, and ask what it would be like to make the required paradigm shift. We might raise two problem areas-- what sort of attitude we would have to adopt towards ourselves, and what sort of attitude we would have to adopt towards others. Following Wittgenstein in taking a language-system to constitute a "form of life" (1958, I, 241), we could ask how the form of life under the materialist program would differ from our present one in these two respects.

Since talk in neurophysiology and in operant-conditioning psychology is essentially the talk of spectators witnessing an event without participating in it, the new life form would involve looking upon one's own inner states only in the role of spectator-- or, put another way, regarding our present and future behavior in the same way that we

7. This argument applies to the "disappearance form" but not to Skinner's radical behaviorism, to the extent that the latter includes functional explanations of behavior.

regard our past behavior. This new life form precludes the role of being an agent in the world, and certainly undermines our moral thinking; in what sense can we be said to be responsible for our future behavior if we have no more control over it than we have over our past behavior that is over and done with? And to the extent that being a scientific observer presupposes the subjective experience, and the agency, of the "I" who is doing the observing, this life form undercuts scientific thinking as well (Platt 1972).[8]

The eliminative materialist might reply that there is nothing new about any of this. We have already been forced to reexamine our moral thinking as we have become more knowledgeable about how we are conditioned by our environment and our early upbringing. But here the materialist is waffling between two positions-- on the one hand he is claiming that his position represents a radical paradigm shift; on the other he is claiming that his position is merely a logical extension of features of our present paradigm. But he cannot have it both ways. It is true, within our present paradigm, that we have had to reconsider the *scope* of our free agency in light of new knowledge of conditioning, unconscious impulses, and the like. But such a reconsideration still presupposes the *possibility* of free agency as a background condition; without this condition moral discussion would simply make no sense. The radical paradigm shift would remove the very possibility of free agency; it would not be merely an extension of our present moral thinking.

8. Support for Platt's assertion comes from the trend in philosophy of science to view observation as inherently theory-laden, thus emphasizing the role of the scientist as an active participant in what he studies (*e.g.*, Hanson 1958).

Skinner is guilty of this waffling when he advocates reforming language by eliminating mentalistic terms such as "freedom" and "dignity," and reforming life by more conscious use of behavior-modification techniques (Skinner 1971). This call for reform seems to suggest a role for choice and action within Skinner's world view, when in fact Skinner's deterministic metaphysics makes such choice and action, in the sense that we speak of them, impossible. Skinner himself would say that we do not choose to accept his reform proposal, if we do so; we rather are caused to adopt it by a pattern of deterministic reinforcers. One might claim that this is simply "choice" and "free action" as interpreted within the new paradigm; but if so it is not at all clear that the new paradigm leaves any room for moral thinking.

These points are reinforced by looking at the attitudes towards others that the new paradigm would have us adopt. Strawson (1968) notes two different types of attitudes that we presently adopt towards others under our present form of life. First, there are what we might call participatory attitudes, which are responses to the attitudes that others have towards us. These include attitudes such as resentment and gratitude, which in turn are closely bound up with the more general attitudes of moral indignation and moral approval. We also have what might be called objective attitudes, which regard others as things to be manipulated rather than as persons. Towards some special classes of humans (*e.g.*, small children and the insane) we have objective attitudes all the time. We also on occasion have objective attitudes towards some normal individuals, for purposes of scientific inquiry, or for furtherance of policy, or simply as a respite from the emotional involvement that accompanies participatory attitudes. But,

as these examples show, when we do adopt objective attitudes towards others we do so for particular reasons. Participatory attitudes, by contrast, are the norm for human encounters; when we have such attitudes it does not make sense to ask for the reasons why we do. (That is, we might ask for reasons why one has one participatory attitude and not another-- "Why did you have such strong resentment to such a silly insult?"-- but not for reasons why one has participatory attitudes, in general, instead of objective ones.)

Strawson then argues that a deterministic thesis, of which eliminative materialism is an example, would require us rationally to adopt objective attitudes towards all people at all times, in effect giving up participatory attitudes completely. However, all interpersonal relationships as we know them, aside from purely instrumental relationships, are based on the context of participatory attitudes that makes up the norm for our form of life; and to suspend all participatory attitudes as the deterministic thesis would require is to remove the possibility for interpersonal relationships. To think that we could even have a choice in this matter is grossly to misperceive the nature of our human commitment to the form of life that we presently live. It is to think that somehow the *universal context* of participatory attitudes can come up for review, in the way that we can review *specific instances* of application of these attitudes. We will, it is true, revise our attitudes toward a burglar once we learn that he was motivated by kleptomania; but we cannot in the same way revise our views on whether we should have participatory attitudes at all.

Strawson compares this commitment to participatory attitudes to our commitment to inductive reasoning (1968, p. 94). Could we give up

inductive reasoning? Induction pervades our form of life, influencing us every time we pick up the phone when it rings, confident that there will be a voice at the other end; and every time we turn the page of a book, confident that the printing will continue on the next page. We could speak of doubting whether induction is justified, but this would be mere verbal expression of such a doubt; we have no idea how to live our lives except in a way that presupposes the validity of inductive reasoning. But none of this restricts us from questioning specific uses of induction, or from trying to revise and refine our rules for applying inductive principles to specific cases.

But if this statement of the nature of our commitment to our present life form does not impress the materialist, let us suppose that we have become somehow able to make the choice between our present way of life and life under the materialist's new paradigm-- the choice, that is, that we have just argued is outside of our ability to choose. We would then have to choose either to continue with our present life form, or to make the radical paradigm shift. Presumably we would want to argue this choice based on the gains or losses involved in the change; and presumably the eliminative materialist would want to argue that the gains outweigh the losses. But what do we have to tell us what counts as a gain and what as a loss, except the background context of our interpersonal relationships? Our notion of benefit and loss presupposes that background context. The materialist, for example, tells us that it would be more "rational" if we were to make the shift and adopt objective attitudes to the exclusion of participatory attitudes. But our concern must be not with what is "rational" in the abstract, but with what is to count as rational behavior towards others; and our

interpersonal behavior is rational or not depending on the nature of our interpersonal relationships-- it is rational to act towards my wife in ways that it would be irrational to act towards a supermarket clerk. But again, our relationships presuppose the background context of participatory attitudes, of our attitudes towards others and others' attitudes towards us. It seems that the materialist cannot even join in this debate over gains and losses without implicitly accepting the framework of participatory attitudes, and the life form, which he is urging us to dispense with. And this, in turn, lends further support to our previous conclusion, that giving up our commitment to this life form cannot be a matter for rational choice.

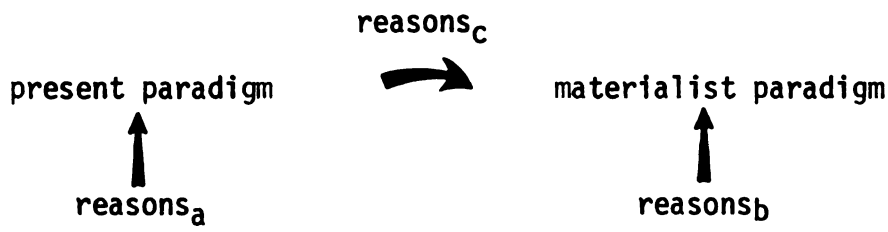
The eliminative materialist still has a reply. It seems as if all this talk about background contexts and forms of life has created a smokescreen around what the materialist originally wished to claim. And that is simply that if we are confronted with an organism whose behavior is determined in lawlike ways by its internal physiologic functions and the stimuli that it receives from the environment, our attitudes towards it is rationally what Strawson calls objective attitudes. This conclusion seems completely plausible. Furthermore, "rational" is a mentalistic term and also has to be radically reconstructed to fit the materialist paradigm. Under this paradigm, rational behavior would simply mean behavior that enhances the survival probability of the individual or the group.

But as soon as we investigate the plausibility of this reply more closely, we find that the plausibility rests on our implicitly assuming that our encounter with such an organism takes place as a special case against the background of our normal human relationships-- indeed, that

is the only way that we *could* conceive of such an encounter, given the human commitment we spoke of earlier. Thus in understanding how we would react to such a case we are dependent on the background context, just as we cannot understand kleptomania as a special case unless we first understand theft as a free action done for reasons and motives. Again, as Strawson says about our commitment to inductive reasoning, we can argue about the rationality or irrationality only of our judgments about specific cases. Our commitment to the universal background context of participatory attitudes is non-rational-- it precedes and underlies our criteria for determining rationality or irrationality.

The eliminative materialist always has a final reply-- since what he is proposing is a radical paradigm shift, it is hopeless to argue with people who are so habituated to the old paradigm and its way of thinking that they can see no alternative. But, if we can force the materialist to adopt this as his final word in the matter, his position becomes much less plausible. For originally he seemed to be proposing not only that a radical paradigm shift ought to be made, but also, more importantly, that he could give *good reasons* for making the shift. If such reasons were to be both relevant and persuasive, they would have to be of such a nature as to bridge the gap between the two paradigms; they would have to show us, in effect, a way to make the transition in our thinking. And we have now seen that no such reasons are forthcoming. The reasons proposed are either completely foreign to our way of thinking and hence fail to persuade us; or else they are dependent upon our present paradigm, and hence give us no reason for making the shift.

This conclusion can be illustrated by a diagram:



We can give many reasons$_a$ to explain why we adhere to our present paradigm, such as pointing out how it allows for moral agency; and the materialist can give many reasons$_b$ for his paradigm, such as its usefulness in terms of psychological research. But reasons$_a$ and reasons$_b$ each show only the internal consistency of the respective paradigms; reasons$_b$ can never convince us to make the radical paradigm shift, and reasons$_a$ can never suffice to refute the materialist paradigm. If we could somehow imagine people living under a life form like that of the materialist paradigm, and who were considering the radical paradigm shift to our own present life form, our reasons$_a$ could never suffice to persuade them to make the shift, any more than the materialist reasons can conclusively persuade us.

The only reasons which speak directly to the paradigm shift are the "bridging" reasons$_c$. It is this sort of reason which, we have just been arguing, does not exist-- at least, none of the reasons proposed by the materialist have been found to qualify. Since reasons$_c$ are the only reasons that could possibly show why we *should* make the paradigm shift, the materialist cannot speak to us of "should." All he can say is that his paradigm is "rational" in the sense that it has survival value, and that groups that adopt the new paradigm will survive while those that adhere to our present paradigm will die out. But this is an empirical claim; all we can do is wait and see.

If the materialist, then, cannot give reasons for the paradigm shift, he can really say only two things about his proposal-- first, that it is not logically impossible; and second, that as science progresses and subtly changes the way we look at and live in the world, such a shift may come to pass. But the same two things can be said about building a bridge to the moon. If this is all the materialist has to say, we may yet choose to make the shift he proposes, but it is then clear that we cannot be said to do so for sound reasons (*i.e.*, for reasons$_c$ if any existed). At most we would be doing it as a strange sort of leap of faith-- strange because, unlike religious faith, the change undercuts rather than supports many of our most basic considered judgments.

This, then, concludes the various arguments that may be raised against eliminative materialism. The arguments have raised doubts about the theory, but we can hardly claim to have refuted it-- indeed, being the sort of paradigm shift that it is, it seems immune from refutation in any ordinary sense. Therefore, eliminative materialism remains a strong theory which we would probably be willing to adopt by default in the absence of any attractive alternatives. In particular, many materialists seem to defend their views with special vigor because they feel that theirs is the only realistic alternative to the troublesome Cartesian dualism. Therefore, if it is possible to formulate a theory which is consistent with all the considered judgments previously listed, but which avoids some of the problems that eliminative materialism raises, an important reason for adopting eliminative materialism would disappear. To see what such an alternative theory would look like, it is necessary first to challenge some of the fundamental presuppositions

of the Cartesian approach to the mind-body problem.

4.3. The Concept of Person

All of the theories that we have investigated so far share the Cartesian assumption that "mind" and "body" are the two primitive terms by which other phenomena must be explained. Even though it represents a radical paradigm shift in other regards, eliminative materialism rests on this assumption nonetheless-- it assumes that the realm of mind can be eliminated completely and that all that will be left will fall into the realm of body. One way to begin the search for alternative mind-body theories, then, is to challenge this assumption. This has been done very effectively by Strawson in his essay, "Persons" (1958).

Essentially, Strawson's conclusion is that there are two kinds of predicates, mental and physical, and two kinds of entities, (mere) material bodies and persons. Material bodies can correctly have ascribed to them only physical predicates, while persons can have ascribed to them both physical and mental predicates. When one ascribes a mental predicate to another person, one does so in the same sense that one ascribes it to oneself; and the "I" to which I ascribe mental predicates ("I am in pain") is the same "I" as the one to which I ascribe physical predicates ("I am six feet tall").

The important feature of Strawson's account is that the concept of person is logically required to be more basic than the mental and physical predicates ascribed to it. To see why this is so, consider the position of the other-minds skeptic that we encountered in discussing interactionism (3.4). As a skeptic, I might want to speak of my pain, but I

am reluctant to admit that there exist any other minds that could have pains of their own; all I can actually observe are other bodies and never other minds. Thus it makes sense for me to doubt whether anyone besides myself has pains.

But the coherence of this skeptical position presupposes that I have certain concepts. If I can speak meaningfully of *my* pains as distinct from *others'* pains, my concept of mental predicates must be that of something ascribable in principle to a class of individual entities of the same logical type. But I can ascribe mental predicates to an entity only if I can identify that entity as an individual; and I can do this only if that entity exists in the physical world. Thus, for anything at all to be the subject of mental experiences in the sense that my skeptical argument requires, there must exist individuals of this unique type of having both mental and physical predicates ascribable to them-- that is, persons (Strawson 1958, p. 342). Only by first having the concept of person can I move by abstraction to the concept of pure mind or pure consciousness (p. 341).

The power of Strawson's position lies in the fact that it is not an argument for the primitiveness of the concept of person-- rather, it is an explanation of why no such argument is needed. For the Cartesian dualist, in order to state his skeptical refutation of the existence of persons, has to presuppose the very concept he wishes to refute (p. 349).[9]

9. It will not do for the skeptic to claim that he accepts the notion of person, but doubts whether the other bodies that he observes are persons. Persons, which are not mind-plus-body in the Cartesian sense, *can* be adequately known and identified through their bodies. To take this position, the skeptic ceases to be merely an other-minds skeptic and becomes skeptical about the reality of the physical world.

Thus, by looking at the concept of person instead of at mind and body, we might avoid both a dualism in which two very dissimilar things must be brought together (interactionism), and a monism which seems to gloss over significant features of reality (behaviorism, identity theory, eliminative materialism). Moreover, if we accept Strawson's account as a very strong and a very basic way of refuting the Cartesian-dualist position, we have further grounds for refusing to adopt eliminative materialism. The materialist, after all, feared that if we allowed mentalistic terms to remain in our language, we would inevitably slip back into the unsatisfactory position of dualism. But Strawson's powerful argument rests precisely on the logical features of mental predicates; so that losing such predicates from our language would deprive us of this very strong argument against dualism.

However, Strawson's account deals with the logic rather than the characteristics of the concept of person. The account is therefore of very limited use in elucidating the philosophical features of problems like the placebo effect. It can tell us, for instance, that the person who believes that he is in a healing context is the same person as the one whose body undergoes change-- that is, that the former is not mental substance while the latter is physical substance-- and that is certainly something gained. But the fact that multiple predicates can be ascribed to the same individual tells us nothing about the relationships among those predicates, if any; and that is where the interesting questions about the placebo effect lie.

Furthermore, in looking at our other considered judgments, we find that the concept that Strawson has described does not fit well with our existing notion of 'person.' We can truly ascribe at least some mental

predicates, such as sensations, to many animals as well as to human persons. It hardly seems consistent with our usual use of 'person' to call these animals persons *merely* because we can ascribe sense-consciousness predicates to them (Frankfurt 1971).

The challenge for the next chapter, then, is to develop a concept of person that has the logical features noted by Strawson, but which avoids some of the shortcomings of his position and which is more illuminating for the placebo effect. If this can be done, the new theory must then be tested against the sorts of considered judgments noted in the previous chapter. We can then compare the degree of overall "fit" for this new theory and for eliminative materialism.

Chapter 5. A Theory of the Person

Strawson's account of the concept of person indicates some logical features that a theory of the mind must possess to avoid both Cartesian dualism and reductionistic monism. The concept could be fleshed out in a number of ways, each yielding a "person theory" with distinguishing features. One such person theory, based on statements about mind by Anthony Kenny (1973a) as considerably amplified by Marjorie Grene (1976), will be discussed at length in this chapter.[1]

The first section offers a formal reconstruction of the Kenny-Grene position. The next section shows how this person theory can be applied to the placebo effect. The following section considers the theory in light of some of the basic considered judgments mentioned in the previous chapter; and the last section lists some of the problems that the theory raises.

5.1. The Capacity Theory of Person

Essentially the Kenny-Grene theory holds that persons are animals possessing a particular capacity, the ability to use symbols in special ways. I will refer to the theory as the "capacity theory" for short.

1. Grene sees her amplification of Kenny's account as arising from the tradition of Continental philosophers Helmuth Plessner and Maurice Merleau-Ponty, and from the epistemology of Michael Polanyi. Since the Kenny-Grene account can stand on its own, I have not attempted to investigate these sources or other matters relating properly to historical background rather than to the theory itself.

The theory can be stated in terms of three major assertions, with some explanatory notes.

(1) Animals have capacities; different sorts of capacities require different sorts of explanations.

'Capacity' here is left essentially undefined by both Grene and Kenny, although we shall later consider some distinctions Kenny makes between a capacity, its exercise, and its vehicle. Grene gives three basic types of principles which may be used to explain various capacities of animals.

(a) Animals, as bodies in physical space consisting of chemical substances, obey the laws of physics and chemistry; and some of their rudimentary behavior can be explained in terms of these laws.

(b) Animals can also exhibit goal-directed behavior which must be explained in terms of teleological organizing principles. We have already reviewed arguments to show that psychological explanations cannot be reduced to explanations of structure without function (4.2). Moreover, even an inanimate machine, such as a clock, cannot be understood *as a machine* unless reference is made to such functional organizing principles-- the laws of physics and chemistry can explain it only as a collection of masses and substances, and cannot explain it as a time-keeping device (Polanyi 1958, p. 330). Grene further distinguishes first-order goal-directed behavior, in which an animal pursues a goal which is in effect already given, from second-order goal-directed behavior, in which the animal is able to choose among alternative goals. A similar distinction is made by Frankfurt (1971), who speaks of first-order desires, whose objects are to do or not to do certain things, and second-order desires, whose objects are to have or not to have certain

(first-order) desires. Many animals have the capacity for the former, while only persons have the capacity for the latter.[2]

(c) Another sort of organizing principle that can explain certain animal capacities is not teleological in form, but rather is normative or typological. A key example is the use made of species and species resemblance in biology. The explanations given are in terms of adherence to or deviation from certain descriptive norms or types.

The basic point of this enumeration is the potential richness and variety of animal capacities. We come to think of animal capacities as a restricted class of phenomena by noting only capacities that can be explained by one sort of principle and forgetting the applicability of different sorts of principles.

(2) Some animals have the capacity to acquire the ability to use symbols in such a way that the use confers meaning upon the symbol.

This description leaves out computing machines, for example, which are able to process symbols but for which the meaning of the symbols depends on outside personal agency. Grene stresses that this capacity should not be viewed as different by some order of magnitude from other animal capacities-- that is, having this capacity is a special way of being an animal in the world, not something "extra" added onto the animal nature such that the animal is no longer "only" an animal. Kenny

2. This observation and what follows leave open the possibility that non-humans, such as chimpanzees who have learned sign language, creatures from outer space, or highly sophisticated machines, could be persons. I accept this, but since our major concern is with humans, I will use "human" and "person" interchangeably. The arguments that follow also suggest that these non-human persons would necessarily be tied to their bodies in a way analogous to humans.

emphasizes that the notions of 'symbol' and 'symbol-using' have fuzzy boundaries and in fact are infinitely open, which indeed corresponds to the way that we use 'mind.' What connects different instances of symbol-using is a "family resemblance" as Wittgenstein used the term.[3]

Being a symbol-user in this way has two important implications. First, an animal with this capacity is at least a potential dweller within culture and within a language system. Second, an animal with this capacity, as Kenny points out, also has a capacity for responsible behavior. In order to choose knowingly and responsibly among alternative goals or actions, one must have ways of representing, through symbols, goals or actions not immediately present. Also, to use symbols in such a way that they acquire meaning is necessarily to have purposes and pursuits, since, as Wittgenstein observed, meaning (for at least a large class of cases) is use within a language system; and language systems, in turn, constitute "forms of life" (Wittgenstein 1958, I, 43, 139; I, 241).[4]

(3) Animals with the capacity to use symbols in this way are persons. To be an animal with this capacity is to have a mind.[5]

3. Wittgenstein contrasted the idea of multiple resemblances, which "overlap and criss-cross" among members of a class, with the idea that there must be one essential element as the common denominator among all members. "The strength of the thread does not reside in the fact that some one fibre runs through its whole length, but in the overlapping of many fibres" (1958, I, 67). See also (Kenny 1973*b*, pp. 153-163).

4. Frankfurt (1971) argues that what distinguishes humans from other animals is the capacity to form second-order desires; if the Kenny-Grene account is correct, it would seem that being a symbol-user in the proper sense is both necessary and sufficient for having second-order desires. This point requires further exploration.

5. Specifically: "To have a mind is to have the capacity to acquire the ability to operate with symbols in such a way that it is one's own activity that makes them symbols and confers meaning on them" (Kenny 1973*a*, p. 47).

To summarize, then, having a mind is a special way of being an animal in the world-- a way that entails participating in language and culture. By the capacity theory, the notion of 'person' cannot be grasped without realizing that it stands, in effect, with one foot in the biological realm and the other foot within culture and sociality. Our Cartesian assumptions tempt us to see these two feet as two different parts of personhood; but since Strawson has proved the primitiveness of the concept of 'person,' we must resist this temptation to fall back into dualism and its attendant problems. It is not dualistic, however, to recognize that different capacities of the person require explanation in terms of different sorts of principles.

The capacity theory might, indeed, be seen as a sort of revival of double-aspect theory (3.2). If "mindedness" refers to a certain capacity that an animal has, and "bodily" refers to its other capacities, then we have a way of making sense of a statement that mind and body are two different aspects of person. The difference, of course, is that originally double-aspect theory was proposed within the framework of Cartesian dualism-- while not itself dualistic, the theory implicitly accepted the Cartesian formulation of the mind-body problem. The capacity theory, on the other hand, is a theory of the person having the logical characteristics proposed by Strawson's fundamental critique of dualism (4.3). The concepts of "mindedness" and "bodily" as they apply to the capacities of persons are both necessarily derivative from the concept of 'person' itself, and no longer indicate a fundamental dualism. The capacity theory, instead of trying to answer the Cartesian question, tries instead to show that the question is erroneously framed.

5.2. The Capacity Theory and the Placebo Effect

As we have done with other mind-body theories, we must ask how the capacity theory of person accounts for and illuminates the placebo effect.

So long as we had to deal with mind and body, we had difficulties bringing together the belief state of the subject and the changes in bodily condition, which, by our definition, had to be linked in order for the placebo effect to occur. Either a mental state had to be made to impinge in some suspicious way upon a bodily state (interactionism), or a mental state had to be reconstrued in different terminology, thus denying some of its crucial features (behaviorism, eliminative materialism). Furthermore, since the mind-body relation orients us toward consideration of the individual mind-body link, the crucial social and cultural dimension of human existence tends to be lost sight of. At best, this sociocultural realm is seen as an extension of the function of individual minds, rather than as in itself a central aspect of the human condition.

The capacity theory of the person changes the picture considerably. Our subject who experiences the placebo effect is no longer a mind and a body, but is a person. Being a person entails having all the capacities of a biological organism, and in addition the special capacity to be a symbol-user and necessarily to be a dweller within culture. If being a dweller within culture is a special way of being an animal, it should not be anomalous if this characteristic were found to influence other animal capacities-- including the capacities to undergo changes in bodily status and function. Experiencing symptom change due to the placebo effect is therefore the bodily expression of the person's

participation in the healing context as a culturally determined, symbolic phenomenon.

Of course, the mechanisms by which this symbolic-cultural event finds its bodily expression need to be studied empirically; the capacity theory cannot answer such questions on an *a priori* basis. The theory does suggest, however, that what is to be studied is the relationship between various capacities of the person, not relationships between two radically different substances, or between two categorially different domains of meaning. We are still likely to need different sorts of explanations for different capacities-- the physical-chemical laws which explain tissue damage, for instance, will not suffice for explaining how culture influences the person's belief states. Perhaps we will even have to develop new, bridging principles to connect different sorts of explanations, before our account of the placebo effect will be complete. But this problem is quite different from that of relating "mind" and "body" as traditionally conceived.

The notion of the person as symbol-user also suggests an additional sense of 'cause' which may be operating in the placebo effect, and which has very different characteristics from the scientific sense of 'cause' mentioned in our discussion of interactionism (3.4). Kenny states, "[T]o use something as a symbol and not as a tool is to use it in such a way that any effect which it may have on the environment lacks the immediacy and regularity characteristic of physical causality" (1973*a*, p. 47). Since using symbols in and on the world has definite effects even though 'cause' in the physical or scientific sense is not applicable, we might want to speak of a sociocultural sense of 'cause.'

Consider the way in which a "no parking" sign might be said to

"cause" certain behaviors of motorists. For some motorists, no-parking behavior will follow, and will be the result of, an inspection of the sign, even though in the vast majority of cases the inspection will take the form of immediate recognition and will not give rise to any train of reasoning. For other motorists, probably those most familiar with the neighborhood, no-parking behavior will occur without looking at the sign, and indeed without any overt or conscious awareness of the sign's existence. Some motorists will park at that spot, and of these, some will receive tickets and some will not. Some will engage in a sort of compromise no-parking behavior, perhaps parking for briefer periods than they would otherwise.

Clearly the way in which the sign might be said to "cause" any or all of these is very different from the way in which the sign could be said to cause a shadow to be cast on a sunny day. Of the varied effects produced by the sign, none occur with the predictability or the regularity we expect of physical causality. And the sorts of things that would count as counterexamples for physical causality do not apply-- even if there were cars parked by the sign more often than not, we would not want to deny its significance or its import. But still, all of the varied behaviors that may occur, either conforming to the no-parking norm or deviating from it, are readily explainable. If more people park by the sign this week than last, and fewer are ticketed, we might explain this as the result of a police strike. Our explanation would draw heavily on the past histories and prior states of the individuals involved, and upon unforeseen present circumstances; and this fits well with the probabilistic nature of the behavior that we actually observe.

One might object to using the word 'cause' at all in such circumstances. Don't these sociocultural cases (the sign "causing" no-parking behavior; an argument "causing" someone to change his mind) lack the constant, or at least statistical correlation between cause and effect which is a minimal necessary condition for ascribing causality? But it is not clear that there is some one central sense of 'cause' such that this condition applies. Feinberg notes that purely empirical investigation normally yields an indefinite number of "causal factors" connected with an event. In giving a causal explanation we are forced to select one or a few of these factors; and the grounds for selection depend on our purposes in seeking the explanation. These purposes may include satisfying our intellectual curiosity ("What causes the tides to rise and fall?"), making practical changes in the world ("What causes automobile fatalities?"), and ascribing moral responsibility ("What caused the death of the innocent bystander?"). Our purposes will determine the criteria we use to judge the acceptability of a proposed causal explanation (Feinberg 1970, pp. 201-207).

Gasking (1955) claimed that the "recipe" sense of 'cause,' which corresponds to the second of Feinberg's three purposes, is the primitive or root sense. But none of the three purposes seems to be necessarily more basic than the others. The thought that there must be *some* root sense of 'cause,' either Gasking's recipe sense, or the Newtonian sense, or some other, arises from looking at the causal ascription in isolation from the various human contexts in which it can arise. Thus, it is a mistake to assume that there must be some one common element, such as constant correlation, connecting all uses of 'cause'; again, a family resemblance is all that is needed (Note 3, above).[6]

The sense in which a culturally-designated healing context can cause changes in symptoms may be seen as the same sociocultural sense of 'cause.' This is also the sense implied by the term "sociosomatic," which Kleinman (1973) employs to describe medicine's "symbolic reality." Elsewhere, in a study of native healers in Taiwan, this author states:

> But our argument is that providing effective treatment for disease is *not* the chief reason why indigenous practitioners heal. To the extent that they provide culturally legitimated treatment of illness, they *must* heal (Kleinman and Sung 1976).[7]

Kleinman is referring mainly to the fact that participants in the culturally approved healing ritual will construe themselves as having been healed, even in the face of unchanged symptoms, as his data show. But some do have changes in symptoms, in a way not explainable by known specific therapeutic effects of the healing process. And for them the placebo effect has been "caused" in the sociocultural sense of the term.

5.3. The Capacity Theory and Considered Judgments

In 3.3 we saw some of the considered judgments which individually supported interactionism, behaviorism, and identity theory; and in 4.1 we saw that these same considered judgments jointly lent support to eliminative materialism, suggesting that the latter was a stronger theory than the previous three. We can now see how the capacity theory of

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6. This failure to look for alternative types of causation is seen in the Cartesian interactionist view. If the body is viewed as a sort of mechanistic clockwork, it is assumed that the mind, in order to affect the body causally, must be of the same category, *i.e.* substance. Thus mind ends up as a mysterious sort of non-mechanistic clockwork (*i.e.*, mechanical but noncorporeal) (Ryle 1949, pp. 18-20).

7. Implicit in this statement is a distinction between *disease*, the explanatory model employed by the culturally designated healer, and *illness*, the subjective experience of the sick individual. For more on this distinction see (Kleinman 1973; Cassell 1976, pp. 47-83).

person matches up with these considered judgments, and also with some others that reflect our basic views of the mind.

Two considered judgments that supported interactionism were that our mental states have an undeniable inner reality for us, and that they have causal efficacy. Judgments that appeared to lend credence to behaviorism and identity theory, respectively, referred to the importance of behavior and of neurophysiology in understanding the mind.

In order to see how well the capacity theory performs on these points, we need to introduce some further distinctions. Capacities of animals, like capacities in general, need to be distinguished both from their exercises and from their vehicles. Kenny (1973*a*) gives the example of whiskey's capacity to intoxicate. Whiskey in the bottle has this capacity, but it is exercised only when the whiskey, having been drunk, is absorbed by the body and interacts with particular cells. And the vehicle of the capacity can be specified to be the alcohol that the whiskey contains. The vehicle is that by virtue of which the whiskey has the capacity; yet it is still possible that another vehicle could be substituted-- for instance, the alcohol could be replaced by some sort of stimulant drug. The capacity is dependent upon *some* vehicle being present, but not necessarily the one particular vehicle that happens to be present in the one case.

A capacity has a real existence even though it is in a sense invisible unless it is exercised; this sort of "inner reality" fits reasonably well with our intuitive idea of having a mind, such as when we think quietly to ourselves. And since the capacity can be exercised given the proper circumstances, its causal efficacy need not be questioned. In describing this capacity and its exercise, the capacity

theory leaves us free to use our usual mentalistic language-- as indeed we have already done by referring to the "meaning" of symbols. We are free to use whatever explanatory principles, mentalistic or physicalistic, best describe the capacity we are dealing with. On the other hand, eliminative materialism was able to accommodate these considered judgments only if we made the recommended paradigm shift and dropped mentalistic talk completely from our language.

It is also the case that knowledge of both exercise and vehicle helps us to understand the capacity. Indeed it is generally by observing the exercise that we come to know of the capacity in the first place. This would then explain why observing the behavior of others gives us good grounds for knowing that they have mind-capacities and are persons. As Strawson notes, being able to ascribe mental predicates to others is a precondition for being able to use them at all, so our observations of others in everyday life "must constitute in some sense logically adequate kinds of criteria for the ascription" (1958, pp. 343-344). Furthermore, knowing more about the vehicle of the capacity adds an additional dimension to our understanding of how the capacity is grounded in the physical world, and how other factors in the world might alter it, even if that is not our primary way of knowing *of* the capacity in the first place. Thus neurophysiology has a significant role to play in providing knowledge about the nature of mind.

But the capacity-vehicle-exercise distinction does not merely account for our considered judgments about the importance of behavior and neurophysiology; it also shows us where behaviorism and materialism have gone wrong:

> Behaviourism, when it takes the extreme form of identifying mind with behaviour, is a form of exercise-reductionism: treating the complex second-order capacity, which is the mind, as if it were identical with its particular exercise in behaviour. Materialism, when it takes the extreme form of identifying mind with brain, or with the central nervous system, is a form of vehicle-reductionism: reducing my mental capacities to the structural parts and features of my body by virtue of which I possess those capacities (Kenny 1973*a*, p. 51).[8]

Furthermore, noting that a capacity is dependent upon some vehicle but not upon one particular vehicle supports Fodor's (1965) argument that a functional explanation in psychology can be realized by an indefinite number of different mechanisms.

Since the capacity theory has so far stood up well with these considered judgments, not only establishing itself as an alternative to eliminative materialism but also (from the standpoint of our present paradigm) showing in its own way how materialism is incomplete, we might want to try it against other considered judgments about the mind. We might recall that a general principle, to be acceptable, need not square exactly with our considered judgments; the principle can indeed expand the content of our considered judgments if it can "extend them in an acceptable way" (Rawls 1971, p. 19).

One considered judgment worth scrutinizing has to do with the possibility of the existence of disembodied minds. Given our present empiricist bias, such a possibility is troublesome; and theories such as interactionism which allow for this possibility are suspect as a result.

The capacity theory eliminates this possibility. It is paradoxical

8. In other cases, such as the physical sciences, vehicle-reductionism might be appropriate-- *e.g.*, "Magnetism is the alignment of molecules in certain metals." The reasons why vehicle-reductionism is inappropriate for the mind parallel the reasons for rejecting eliminative materialism (4.2). See also the discussion of reductionism in 5.4 below.

to talk of a capacity unless it is a capacity of something; specifically, the mind-capacity is a capacity of certain kinds of bodies (*i.e.*, persons). Therefore the notion of disembodied minds makes no sense (Kenny 1973*a*, p. 49). Further support for this view is provided by looking at what it means to remember correctly, as opposed to merely seeming to remember. If I can be said to remember truly talking to Sam last week, it must be possible at least in principle to ask Sam, and for Sam to be able to identify me as the same individual to whom he spoke. But we already have noted that individuals can be identified through time only if they are embodied (Strawson 1958); thus we would have no conceivable criteria for ascribing true memories of previous experiences, and hence continuity of experience, to a disembodied mind (Penelhum 1970).[9]

Another considered judgment has to do with the central role that consciousness plays in what we mean by "mind," and also the supposed "private access" we have to the workings of our own mind. But this judgment perhaps stands in need of some extension. Kenny warns us of the danger of failing to distinguish sense-consciousness from self-consciousness. Self-consciousness requires sense-consciousness, but it also requires language: "one cannot know how to talk about oneself without knowing how to talk, and one cannot think about oneself without being able to talk about oneself" (Kenny 1973*a*, p. 48). Here again

9. Strawson, who agrees with Penelhum on identity criteria, would still allow that a disembodied person would maintain his identity by virtue of having been a person (1958, pp. 341-342); presumably this person could have memories but no new experiences. Penelhum refutes even this, noting that the concept of memory is parasitic upon the concept of identity, not *vice versa*, so that we have no idea to whom to ascribe the purported memories of having been a person.

one might refer to Wittgenstein, on the impossibility of a private language accessible in principle only to one individual (Wittgenstein 1958, I, 243-363; Kenny 1973*b*, pp. 178-202). By pointing out that having a mind is having the capacity to use symbols, therefore, Kenny has given a more basic feature of being a person than self-consciousness. As Strawson noted, the logic of mental predicates requires that they be used self-referentially in the same sense that one ascribes them to others (1958, pp. 337-339); thus the ability to use these predicates self-referentially follows derivatively from the ability to use them at all.[10]

The problem of privacy or private access, then, arises mainly for sense-consciousness or perception. Two points can be made here. One is that, with the switch in emphasis from minds to persons, the private-access problem loses most of its bite: "It is true that you cannot feel my pain or my pleasure or my hate or my love; that is true also about the pains, pleasures, loves, or hates of other animals" (Grene 1976, p. 191). The second point that follows from this is that what is distinctly human or "personal" is not having perceptions, but experiencing perceptions within the framework of self-consciousness and of language--which removes the private feature as a crucial characteristic.

> I will just say that the confusion seems to me to arise from people's being over-impressed with their ability to talk to themselves without making any noise, and their ability to sketch things before their mind's eye instead of on pieces of paper. I think

10. "In order to have this type of concept [of mental predicates] one must be both a self-ascriber and an other-ascriber of such predicates, and must see every other as a self-ascriber" (Strawson 1958, p. 346). Had Strawson defined 'person' as a *self*-ascriber of mental predicates, rather than as an individual to which mental predicates may be ascribed, he would have avoided Frankfurt's criticism (1971); see 4.3 above. I owe this observation to Martin Benjamin.

> that the acquisition of the ability to talk *about* oneself is enormously significant; the acquisition of the ability to talk *to* oneself is by comparison merely a matter of convenience. A society which differed from ours only in that everyone thought aloud all the time instead of thinking silently would be perfectly conceivable, equally intellectual, only unbearably noisy (Kenny 1973*a*, p. 48).[11]

In summary, the capacity theory accounts for the placebo effect in an illuminating way and also shows good "fit" with all the considered judgments that we have looked at. The eliminative materialist has the laudable goal of avoiding the problems of Cartesian dualism and of pointing out the importance of research into behavior and neurophysiology; but he feels that this can be done only at the price of a major paradigm shift to remove all mentalistic talk, and all moral and normative thinking, from our language. The capacity theory, on the other hand, avoids dualism and provides a secure place for behavioral and neurophysiological research, without requiring such a paradigm shift. On these grounds the capacity theory must be viewed as preferable to eliminative materialism and to the other mind-body theories listed in Chapter 3.

5.4. Problems with the Capacity Theory

While the capacity theory of person provides good overall "fit" compared to other mind-body theories commonly held, given the complexity of the mind-body issue it would be highly astonishing if the "fit" were perfect. This section will list a few of the major problems that the capacity theory raises. The problems will necessarily be dealt

11. "[W]e only say that someone speaks to himself if, in the ordinary sense of the word, he *can speak*" (Wittgenstein 1958, I, 344; *cf.* Ryle 1949, pp. 27, 200).

with in relatively cursory fashion, as they lie on the borderline between philosophy of mind and other issue areas which are beyond the scope of this inquiry.

One objection to the theory might come from the eliminative materialist. It is not at all surprising, he would say, that the capacity theory explains so well the considered judgments that support materialism, since the capacity theory is nothing more than eliminative materialism in a new verbal guise. The three sorts of explanatory principles we have admitted (5.1) are exactly the sorts that the materialist would use in explaining brain processes or behavior. The fact that we have admitted that the theory would allow certain very complex computing machines to be considered persons (Note 2, above) would seem further evidence for this. If we can talk about the mind in such "materialistic" terms as capacity, exercise, and vehicle, we would seem to be carrying out the eliminative-materialist program; to claim that we are still leaving room for mentalistic talk in such a scheme is simply disingenuous.

This objection deserves to be taken seriously. We have, after all, only cited Strawson's concept of person on the way to stating the capacity theory; we have not proved conclusively that the capacity theory in fact adheres to the logical framework of Strawson's account. On more detailed analysis, the account we might give of the symbol-using capacity might in fact turn out to be reducible to purely "bodily" terms; and we will have indeed fallen back into an eliminative-materialist position, despite our verbal rejection of the Cartesian-dualist framework. If the capacity theory can be conclusively refuted, it will probably be on these grounds.

There are, however, two strictly intuitive replies to this objection that suggest that we have indeed avoided simply repeating materialism in a different verbal guise. One is the very prominent role given to language and culture in our account. Since this cultural dimension is basic to the understanding of 'person' according to the capacity theory, but never arose even peripherally in our discussion of other mind-body theories, we have some grounds for thinking that the capacity theory of person is indeed different in a fundamental way from theories arising within the dualistic framework. A second reply is the observation that, if mind is a special sort of animal capacity, we would expect to see a "gray zone" of capacities falling in between this special capacity and the purely "bodily" ones. And such a gray zone does in fact seem to exist, especially in the sophisticated behavior of higher mammals which seems to share important similarities with human behavior, even though these animals lack linguistic capacities. Indeed, it is precisely this gray zone of behaviors that gives behaviorism most of its plausibility as a mind-body theory. By contrast, had there been no such gray zone, and had the special mindedness-capacity stood sharply apart from all other animal capacities, we would have had grounds to suspect that a dualistic framework lurked at the bottom of our account.

There are, moreover, a number of assumptions that may underlie the materialist objection and which are difficult to tease out. These assumptions have to do with questions like what it means to reduce higher-order explanations to lower-order ones, such as reducing the laws of psychology to the laws of physics and chemistry; what role free will plays in our lives, and what sorts of scientific explanations of human

behavior are compatible or incompatible with the existence of free will; and the relationship between functional and structural explanations and whether the former can be reduced to the latter without loss of content. For example, the materialist might argue that our present mentalistic language implies free will; that all three sorts of explanatory principles we have listed, if strictly construed, are compatible only with determinism; and so to argue that these principles suffice to explain mental behavior is the same as overturning our present mentalistic language.

The materialistic objection seems to me to be incorrect; but a complete refutation would take us far afield, into the metaphysical issues of free will and determinism and into philosophy of the biological sciences. It is possible within the scope of this paper to make a few comments about reductionism. The reductionism issue might arise particularly from the assertion that the mind-capacity is merely a special way of being an animal, not something "above and beyond" animal nature. This, in turn, comes back to the nature of the distinction between first-order and second-order teleological explanations. But, if animals can have first-order desires while only persons can have second-order desires, isn't the latter different from the former in a more fundamental way than our theory would allow? Hasn't one in effect simply replaced the physical-mental distinction with the first order-second order distinction? This would give rise to a dilemma. Either second-order desires are irreducibly "mentalistic," so that listing them among animal capacities is mere verbal camouflage; or else we are engaged in a reduction of the mental to the biological.

When one attacks a theory as being reductionistic, one presumably

has in mind a pernicious form of reductionism, in which an explanation in terms of lower-order principles succeeds only by ignoring crucial, complex features of the higher-order phenomena being explained. If one achieves simplicity and economy of explanation without dismembering the phenomenon in this way, reductionism cannot be faulted. The most common case of the pernicious form of reductionism is the proposal that mental processes are explainable merely in terms of physical and chemical laws. But we have already argued that this proposal ignores the crucial features of psychological explanation (Fodor 1965), and that this form of reductionism will not work even for inanimate machines, let alone persons (Polanyi 1958). Malcolm has argued that if this reductionistic theory were true, no one could assert it to be true. An assertion is an intentional action, which implies purposive behavior and a belief state on the part of whoever makes it; otherwise we could not distinguish an assertion from a phrase being played on a phonograph record. But intentional actions lie outside the realm of deterministic physical laws. Thus, the sentence, "Reductionism (determinism) is true, and I assert it to be true" is internally contradictory (Malcolm 1968).

Since any reduction that is involved in Grene's (1976) list of explanatory principles is not of this type, the burden of proof is on the objector to show that it is reductionism of the pernicious sort (*cf.* Grene 1971). Certainly the second-order teleological principles differ in kind, not just in degree, from first-order ones; but as Grene has reminded us, we need different kinds of principles just to explain machines and lower-level biological organisms; in this regard, explaining how a person differs from lower animals is no different from

explaining how a clock differs from a mere collection of metal and wood pieces.

Another problem with the capacity theory is that it seems to commit one to providing some account of how the mind-capacity evolved or emerged from the purely biological capacities of organisms. Again, if such an account could not be provided, the assertion that having a mind is merely a special way of being an animal would be difficult to maintain. And the account is clearly problematic. Consider, for example, the difficulties in explaining how the concept of language could arise among non-linguistic beings; such beings would, it seems, have to possess the concept at least in rudimentary form before they could discover it.

Two replies can be given briefly. One is that many purely biological characteristics of animals present similar puzzles for evolutionary theory. The other is that the really interesting questions of human evolution have to do with the stage *after* the mind-capacity evolved. As soon as man developed language and culture, we became subject to two different kinds of evolution, biological and cultural, with the latter playing a dominant role in recent human history. To understand how a species can be subject to these two kinds of evolutionary forces, we require some concept of a being which is inherently both a biological organism and a dweller within culture. Thus, with the concept of person, we have a puzzle of how mind evolved; but without this concept, we are totally unable to make sense of the history of mankind since mind evolved.

All of these problems with the capacity theory of person are worthy of further study; but there is no compelling reason to assume that this

further study will not provide some satisfactory solutions. Given the many strong points of the theory (5.3), the problems do not by themselves give us any grounds for rejecting it. Indeed the problem areas we have just noted give the theory a sort of indirect support. The fact that, in order to criticize the theory, one must immediately run up against significant issues in metaphysics and in philosophy of science suggests that the capacity theory "covers the ground" well, and has the breadth that one would require from a comprehensive philosophy of mind. The types of issues raised by the problems above also lends indirect support to the "reflective equilibrium" strategy that we have been using, by showing that one cannot investigate one area of philosophy for long without running up against other areas.

We are left, then, with the view of a person which is firmly grounded both in biological nature and in culture and language. So long as medicine makes progress by abstracting only the person's animal features for study, the dominant medical paradigm is bound to view the placebo effect as an anomaly. But the capacity theory of person implies that no being can be *necessarily* both a biological and a cultural entity without the cultural features influencing the biological ones, and *vice versa* (as the interplay between cultural and biological evolution illustrates). By this view, the placebo effect, in which participation in a specific cultural context produces changes in bodily condition, becomes an expected and understandable, rather than an anomalous finding.

In developing the mind-body implications of the placebo effect, we have relied heavily on the formal definition from 2.3. We will

find other features of this definition to be of use in the next chapter, as we take up the ethical issues related to the use of placebos in clinical practice.

Chapter 6. Ethical Problems in Placebo Use

Having now devoted three chapters to the mind-body "corner" of the reflective-equilibrium "triangle" mentioned in the Introduction, we now come to the ethical "corner." In this chapter we will have little to say about the mind-body considerations previously mentioned. However, for other issues in medical ethics, if not for the placebo case, the notion of 'person' we have developed raises important questions, as will be noted in the Conclusion.

We will, however, be making use of the remaining "corner," the empirical-conceptual corner embodied in the formal definition (2.3), in looking at the ethical implications of placebo use. That definition construed 'placebo effect' very broadly-- in particular, it suggested that the term 'placebo effect' could be applicable even where no placebo had been used. We will see what implications the logical independence of the placebo effect from the use of an inert treatment has in discussing the ethical issues.[1]

Following a brief historical background relating mainly to placebo use in the nineteenth and twentieth centuries, arguments that have been offered for and against placebo use will be listed. These arguments will then be reviewed critically and the cases for and against placebos summarized. Arguments proposed for limited placebo use will

1. The material in this chapter represents a considerable expansion upon and revision of (Brody 1975).

then be considered. Finally, the ethical picture will be re-evaluated with special emphasis on possible alternatives to placebo use.

6.1. Historical Background

A historical survey might address the question of how widespread placebo use has been in previous times, and of whether physicians in those periods perceived any ethical questions regarding it. We might begin with the start of the nineteenth century, when definitions of 'placebo' similar to the one now accepted began to appear in medical dictionaries (1.1).[2] One early commentator on placebo use in the United States was Thomas Jefferson, who wrote to a physician friend in 1807:

> [T]o an unknown disease, there cannot be a known remedy... Here, then, the judicious, the moral, the humane physician should stop. Having been so often a witness to the salutary effects which nature makes to re-establish the disordered functions, he should rather trust to their action, than hazard the interruption of that, and a greater derangement of the system.... Or, if the appearance of doing something be necessary to keep alive the hope & spirits of the patient, it should be of the most innocent character. One of the most successful physicians I have ever known, has assured me, that he used more bread pills, drops of colored water, & powders of hickory ashes, than of all other medicines put together (Blanton 1931, pp. 198-199).

Jefferson went on to note that this was a "pious fraud," but much less harmful than the active treatments of dogmatic adherents to the therapeutic schools of the day. This apparent admission that there might at least be some question raised about placebos is somewhat unique for that era; Thomas Percival's 1804 code of medical ethics, a voluminous and

2. I have been unable to locate any work on the history of placebo use by medical historians. Shapiro's "A Contribution to the History of the Placebo Effect" (1960) is rather a history of ineffectual remedies and casts no light upon the issues raised in this chapter.

comprehensive work which dominated medical thinking for most of that century, is silent on the placebo issue (Percival 1975).

An early investigation into what would now be called psychosomatics included a review of how "Expectation or Hope" could stimulate the "beneficial Action of totally inert Substances" (Tuke 1873). Examples of the use of bread-pills were given from both the French and the British literature, most notably a series of observations dating from 1845 by a British naval surgeon, Sir John Forbes. While Forbes was enthusiastic over this remedy, Tuke added editorially:

> Whether his advice has been adopted to the extent which it deserves, may well be doubted. Nothing can justify asserting what is not true in order to gain the patient's confidence-- a course adopted in some of the foregoing cases-- but this forms no essential part of the method of treatment now referred to. At the same time it is liable to degeneration into it (Tuke 1873, p. 371).

This commentary is ambiguous to say the least. I take Tuke to be presupposing a highly questionable distinction between outright lying and "avoiding" lying by judicious silence. Thus, he seems to be saying, if one tells a patient that a bread pill is active medicine, one has violated a moral rule; but if one simply administers the bread pill, silently allowing the patient to assume that the pill is active medicine, one has not "asserted what is not true" and so has avoided blame. We will encounter a rejection of this moral reasoning below.[3]

H. C. Wood, who cited Tuke's book approvingly, emphasized the efficacy of the placebo effect without expressing any ethical reservations

3. Significantly, Tuke in his five-page discussion of hope and expectation never uses the word 'placebo.' This points out a major problem for anyone attempting a historical survey, as I have done, by searching indices or tables of contents of medical works of various periods.

(Wood 1880, p. 23). Samuel W. Gross recommended placebo in the treatment of "psychical impotence from undue sexual excitement or emotional causes,... since such cases usually remedy themselves" (Gross 1887, p. 62). An early suggestion that one can obtain a partial placebo effect without resorting to deception by use of inert substances came from Shoemaker:

> [I]f the remedy be attractive in appearance and pleasant to the taste, it will be regarded as a signal success, even though of less therapeutic activity. An agent is sometimes given merely for the mental and moral effect, without having any medicinal action directly. Such a combination is called a *placebo*.... Although placebos are rarely resorted to, patients should always be well treated, and with a little care much can be done toward making preparations pleasant (Shoemaker 1896, p. 42).

Occasionally the ethics of placebo use was addressed directly in the medical writing of the period. An editorial in the *Medical Record* in 1885 defended an earlier statement on placebos against criticism from the *Peoria Medical Monthly*, and repeated the position as follows:

> Physicians and intelligent laymen know that the former cannot always tell the plain facts to a patient without injuring him. It should be the rule of his life, however, to be straightforward and candid. Therefore, we say that placebos should be, and need be rarely, if ever, prescribed.

Going on to list the virtues of the ideal physician, the editors concluded, "We venture to say that such a man would not find it necessary to keep a polychromatic assortment of sugar pills in his closet" (Placebos 1885).

The eminent Harvard physician, Richard C. Cabot, introduced his 1909 commentary on placebos in terms that contradicted Shoemaker's assertion that placebos were "rarely resorted to":

> Now, I was brought up, as I suppose every physician is, to use what are called *placebos*.... How frequently such methods are used varies a good deal I suppose with individual practitioners, but I doubt if there is a physician in this country who has not used

them and used them pretty often (Cabot 1909, p. 158).

Cabot wrote before the era when quantification became the norm in medicine; but a 1952 *British Medical Journal* editorial hazarded the estimate that 40 per cent of patients visiting general practitioners in England were given placebo prescriptions (Editorial 1952).

Some sidelights on the extent and acceptance of placebo use in the nineteenth century are hinted at in some literary references. Louisa May Alcott in *Eight Cousins* portrayed a kindly physician compounding some bread pills for the heroine-- to placate not her but her oversolicitous aunts (Alcott 1874, pp. 44-46). A very imaginative use of inert pills shows up in the first Sherlock Holmes adventure, *A Study in Scarlet* (Doyle 1888). The criminal, seeking revenge against two Mormons who had caused the death of his betrothed, fabricates two pairs of identically appearing pills, one in each pair inert and the other containing deadly poison. Certain of the justness of his cause, he plans to offer each of his victims in turn first choice of a pill, upon which he will swallow the one remaining. The first Mormon agrees at knifepoint to this plan and, having chosen unluckily, goes to his just reward; the second balks at this early form of the double-blind randomized trial and has to be stabbed instead (leaving the pills behind for Holmes to find as a clue). Doyle was himself a physician; but if he based this fictional incident on any real-life case, his biographers are uniformly silent on the point-- despite the extensive commentary that has accumulated around nearly every other feature of the Holmes adventures (Doyle 1967).

6.2. Arguments For and Against Placebo Use

While we have seen above a few statements of position from which moral arguments could be reconstructed, we have encountered no carefully worked out justification for any of the positions taken, either for or against placebo use. A full argument is, however, contained in the work by Cabot already alluded to. Cabot (who among other titles held that of Professor of Social Ethics at Harvard) discussed the placebo issue with a sophistication rarely matched by more modern commentators.

Cabot dealt with placebos under the topic of deception in therapy, tying this in with deception in diagnosis (the inexperienced physician trying to create a false picture of competence instead of calling in a consultant when needed) and deception in prognosis (failing to tell the patient the truth about a grave or terminal disease). Cabot deplored the fact that "the great bulk of medical work, public and private, is still done by men-- high-minded men-- who believe that it is impossible to deal frankly and openly with patients" (1909, p. 118).

Cabot dismissed the false moral distinction that I have inferred from Tuke by noting, "*A true impression*, not certain words literally true, is what we must try to convey" (p. 126). What counts as a true impression depends on what people reasonably expect from various types of social encounters; and from a physician the patient has come to expect an active medication. Thus the physician who administers an inert substance without comment, or with a noncommittal remark such as, "Take this, it will help you," has created a false impression and is guilty of deceit, even though he has not told an outright lie.

To argue against placebo use Cabot then outlined arguments in both the deontological form, stressing actions from duty, and the utilitarian form, stressing actions that lead to good consequences. In the former instance, he suggested that the practice of deceit was contrary to the duties and obligations of the medical profession, if not indeed of people in general-- one may in point of fact never be found out, "[b]ut is it good for us as professional men to have our reputations rest on the expectation of not being found out?" (p. 133).[4] In a utilitarian approach, Cabot listed the negative consequences of deceit by placebo use. The short-term consequences are the loss of patient trust if the physician is found out; and the long-term consequences are the nurturing of unhealthy public attitudes:

> The majority of placebos are given because we believe the patient will not be satisfied without them. He has learned to expect medicine for every symptom, and without it he simply won't get well. True, but who taught him to expect a medicine for every symptom? He was not born with that expectation. He learned it from an ignorant doctor who really believed it... It is we physicians who are responsible for perpetuating false ideas about disease and its cure....and with every placebo that we give we do our part in perpetuating error, and harmful error at that (pp. 161-162).

Cabot concluded, "No patient whose language we can speak, whose mind we can approach, needs a placebo" (p. 169). Instead Cabot favored taking more time to explain to the patient the rationale for not using medication. The economic question can be raised of whether this increased use of physicians' time is cost-effective (Fuchs 1974, p. 125), a point we shall return to later.

4. Cabot's use of "good" here may suggest a utilitarian mode of argument, that deceit will have the negative consequence of hurting one's reputation. I take Cabot to be saying rather that a professional person must adhere to certain principles even if no bad consequences follow from failure to do so.

More recent articles (*e.g.*, Bok 1974) do not go much beyond the arguments laid out by Cabot at the turn of the century. Recent advocates of more widespread placebo use have deplored the disdain attributed to placebos as a wealth of new, scientific medications has appeared on the scene (Benson and Epstein 1975). The most frequent argument given to support placebo use cites the undeniable efficacy of placebos, and the advantages of avoiding the side effects present in potent drugs (Sice 1972; Evans 1974). But these arguments ignore not only the negative consequences cited by Cabot, but also the fact that placebos can produce deleterious side effects of their own (Bok 1974), as reviewed in 1.2 above.

Another focus of more recent concern has been the distinction between "pure" and "impure" placebos, the former referring to totally (pharmacologically) inert substances and the latter to substances that have specific medical uses, but are used either (a) for a different condition, or (b) in doses too low to be effective. Common examples in today's practice are (a) antibiotics, thyroid extract, and vitamins (used in the absence of known bacterial infection, hypothyroidism, and vitamin deficiency, respectively), and (b) low doses of minor tranquilizers. A 1946 conference on placebo use disparaged the use of impure placebos while defending pure placebos. One participant stated:

> If deception is involved in the case of the pure placebo, it applies to only one person, namely, the patient, for the physician knows that the agent is devoid of all but psychotherapeutic properties. But when we use [an impure placebo] there is the danger of deceiving two people.... The doctor may come to think that the agent has potency when, in fact, it has none. That danger is real (Conferences 1946, p. 1726).

By this argument, deception of "only" the patient is morally unimportant, while possible self-deception by the physician needs to be

guarded against diligently. The overriding concern here seems to be some sort of principle of the purity of medical science. These academic physicians in 1946 were very much aware that they were witnessing the close of an era characterized by ineffective or deleterious remedies endorsed by random or anecdotal experience, and the beginning of an era of new, potent medications supported by scientifically sound data. Any backsliding into the old, unscientific use of therapy was therefore to be stringently opposed. To this concern about scientific purity was added the observation that pharmacologically active agents, even in low doses, might be expected to produce more side effects than pure placebos. The conference concluded that 1) placebo use was to be encouraged, assuming "proper selection of cases and choice of placebo materials" (p. 1727), and 2) pure placebos were to be preferred over impure placebos. The problem of patient deception was thus implicitly dismissed as an issue.

A rather novel line of argument was suggested by Modell. Noting, as we have already done, that there is a placebo component in almost every use of an active medication by a physician, Modell concluded that since the placebo effect was already so widespread, physicians would be foolish not to put it to further use by prescribing placebos freely (1955, p. 70). Of course, this fails to take into account the fact that placebo use involves deception, while the placebo effect accompanying use of other treatment does not (a point we shall be emphasizing later).

6.3. The Arguments Summarized

All the arguments cited above which favor placebo use are notable for the cavalier attitude that they display toward patient deception. This might be an adequate reflection of medical attitudes for the 1940's; but recent reconsiderations of medical ethics have emphasized the value of patient autonomy, the "contractual model" of the doctor-patient relationship, and the right of informed consent (Ramsey 1970; Veatch 1972; Fried 1974; Brody 1976). Taking into account this contemporary work in medical ethics, I shall now summarize more formally and more critically the arguments from the previous section.

I will leave aside discussion of the use of placebos in research. The importance of informed consent in research on human subjects has been almost universally recognized (Freund 1969). Where the design of the experiment involves placebo use, there seems to be no reason in most cases why the patient cannot be informed of this. So long as the protocol is double-blind, so that the individual subject does not know whether he personally is receiving placebo or experimental drug, the scientific validity of the experiment should not be altered by imparting this general information (Bok 1974).[5] A practical problem, admittedly, would be the inability to get people to volunteer for some experiments; placebo surgery might be a case in point (Beecher 1961).

We are left, then, with the therapeutic use of placebos-- use in

5. One interesting exception might be studies of the placebo effect itself, in which it might be necessary to conceal part of the experimental design-- *e.g.*, if one were trying to measure directly the extent to which the placebo effect depends on deception in various circumstances. The important issue of when deception can be justified in research is beyond the scope of this discussion.

which the hoped-for beneficial effects are intended directly and solely for the patient. The arguments given above in favor of placebo use might be reconstructed as follows:

1. A physician ought to employ any remedy which provides significant possibility of benefit while imposing only minimal medical risks on the patient.[6]
2. Placebos offer significant possibility of benefit while imposing only minimal medical risk.
3. Therefore, physicians ought to employ placebos.

This argument makes no distinction between remedies that rely for their success on patient deception and those that do not.[7] The deception issue is important particularly because "significant benefit" and "minimal risk" are value judgments. If informed of the nature of the therapy, the patient can have at least some say as to what is acceptable risk and benefit *for him*; if deceived, the patient is totally dependent on the judgment of the physician, who may not share the patient's value outlook and lifestyle preferences.

The defender of placebo use, if deontologically inclined, might try arguing that the deception involved is not "really" deception but rather is on the order of a white lie (Bok 1974). Some justification for this might come from noting that the physician is, after all, acting for the patient's benefit and not for any selfish motive. Or, if

6. Here we are assuming proper diagnosis, selection of a remedy indicated by the patient's condition, etc.

7. In 1.3 we cited one example of successful nondeceptive use of an inert pill (Park and Covi 1965), but I am reluctant to overgeneralize from this intriguing but limited study. Suffice it to say that as most commonly utilized by most medical practitioners, placebo use involves deception.

inclined toward utilitarianism, the defender might claim that deception involves significant damage only if the physician is found out, and the chances of this are small enough so that the good to be gained from placebo use outweighs this negative consequence.[8] But neither of these replies takes into account the importance of patient self-determination and autonomy.

An additional implication of placebo deception that has not been dealt with so far is the financial cost of the prescription. Presumably, unless the patient is billed for an amount commensurate with the cost of active drugs, the deception will not succeed. But is it ethical to charge for sugar pills what one would charge for an antibiotic or a tranquilizer? Further, the empirical studies of the placebo effect would suggest that increasing the cost of the prescription, making the remedy seem more valuable and exotic, might enhance the placebo effect. Should this be done in practice? If so, should the extra sum go to the pharmacist, to the physician, or to some favorite charity? Should the price be jacked up higher for richer patients? If so, are the poor being deprived of a possibly effective remedy? Should private or government-funded medical insurance pay for the difference? The medical literature has been largely silent on these issues, but they would have to be addressed as part of any serious defense of placebo use.

Turning next to arguments against placebo use, we again find

8. This argument assumes the perpetuation of the Cabot-era relationship between the medical profession and the remainder of society, and ignores current movements toward greater "consumer input" into health matters and better patient education (*e.g.*, Rabkin 1973; Vickery and Fries 1976). Thus the chance of "being found out" is probably increasing steadily.

deontological and utilitarian variants. The former might run:

1. It is wrong to knowingly deceive someone (except in extreme circumstances, as when the SS troopers ask about the Jews hidden in your attic).
2. The therapeutic use of placebos requires deception.
3. Therefore, the therapeutic use of placebos is wrong.

The utilitarian argument against placebo use is well laid out both by Cabot (1909) and by Bok (1974):

1. A policy should be adopted only if it decreases the net amount of pain and suffering, taking into account both the short- and long-range consequences.
2. Regular use of placebos by physicians will relieve the symptoms of many patients. In some cases these symptoms will have been severe; but in many if not in most cases they will have been short-lived or trivial. Placebos will thus produce directly some diminution of pain and suffering.
3. In a few instances, the patient will discover the deception, thereby losing faith in the physician and seriously hampering any further therapeutic attempts. This will produce some short-term increases in pain and suffering.
4. In the long term, it will probably become generally known that physicians are in the habit of deceiving people. This will lead to some generalized slight mistrust of physicians, hampering some of their therapeutic activities. Taking into account the entire population, this is likely to produce a considerable increase in pain and suffering.
5. Also in the long run, the public attitude will be reinforced

that symptomatic relief with "active" drugs is the best treatment for all ills. This will lead to overuse of medications with increased morbidity and mortality from adverse drug reactions. It will also interfere with patients learning rational health habits which could better prevent disease. Again taking into account the total population, a large increase in pain and suffering could be predicted.

6. The increases in pain and suffering in #3-#5 are likely to outweigh the diminution in pain and suffering in #2.
7. Therefore, a policy of placebo use cannot be justified.

Although this utilitarian argument makes a number of empirical assumptions which need to be tested in practice, none of them seem unreasonable.

The deontological argument against placebo use would prohibit this practice in all but the most extreme circumstances (such as, perhaps, an irrational patient threatening immediate suicide). The utilitarian argument, however, addresses only placebo use as a general policy, and leaves open the possibility of justifying very limited use in selected cases. This matter requires further consideration.

6.4. Limited Placebo Use

Many of the arguments we have previously reviewed dealt with placebo use generally. However, a number of medical authors, many of whom at least implicitly recognize the ethical problems associated with deception, still feel that placebos have a particular utility in specific categories of cases, and that their limited use can be justified on those grounds. Suggested specific indications for placebos include the

following:

1) diseases for which placebos have proved efficacious experimentally (Bourne 1971);
2) diseases for which no pharmacologically active treatment exists (Frank 1974);
3) cases of narcotic withdrawal in which placebo can be substituted gradually for the narcotic (Leslie 1954; Wolf 1959);
4) instances of necessarily prolonged diagnostic testing, during which the patient, if not placated by "treatment," might become dissatisfied and not return for needed therapy (Leslie 1954);
5) anxiety states which appear to be interfering with the success of other, essential treatment (Frank 1974);
6) illnesses for which no drug or other treatment is indicated, but for which the patient demands treatment (Bourne 1971);
7) temporary situations in which placebos are used initially to placate the patient until a doctor-patient relationship can be established for more direct use of psychotherapy or emotional support (Wolf 1959; Frank 1974).

Several of these indications can be readily dismissed. In #1, limiting placebo use to those symptoms for which experimental efficacy has been demonstrated is tantamount to not limiting placebo use at all (1.2). In #3, placebos have proven in some patients to be every bit as addicting as narcotics (Bourne 1971; Bok 1974). In #5, one might ask why anxiety states should be singled out for placebo use, since both pharmacologically active anti-anxiety drugs and anxiety-lowering psychotherapeutic techniques are readily available.

In #6 a more serious question is raised. Sometimes a patient may seem almost to be asking for a placebo ("I know that you say that drugs won't help me, Doc, but surely you can give me *something*"). Assuming that there are sound reasons not to use placebos in general, such a case does not seem to differ materially from that of a patient insisting that he wants a gall bladder operation despite repeated reassurances about the absence of disease in that organ. If the patient is fully informed about risks and benefits (or indicates a desire not to be so informed), how far is the physician obligated to go in satisfying desires contrary to his own views of proper medical practice or of medical ethics? This is an important issue that must be addressed by any "contractual" view of the doctor-patient relationship, but it is not peculiar to the placebo case.

Of all the suggested indications, #7 may seem most acceptable because of its temporary nature and the fact that temporary deception is to be used as a means to achieving a state in which deception will no longer be necessary. But even this sort of use raises questions, including whether upon establishing the hoped-for relationship the fact that a placebo has been used is to be revealed to the patient. Bourne comments somewhat obscurely on this point:

> If the relationship between the physician and the patient is a strong one, the true nature of the placebo can be revealed at a later date with little danger. If such a relationship does not exist, the placebo should not have been administered in the first place (1971, p. 4).

This way of putting it seems to require clairvoyance on the part of the physician beginning treatment. What Bourne seems to mean is that if the initial relationship is so shaky that the doctor feels the need to resort to placebos, the likelihood of a strong future relationship is

very low. It could also be asked what *sort* of relationship would arise--if it is one in which the patient expects the doctor to react to each new symptom with another drug, more harm than good may have been done, as Cabot noted.

However, with placebos, as with any other proposed medication, a final answer on the use or nonuse cannot be given until alternative modes of therapy have been evaluated. And most commentators on the placebo effect have made little explicit mention of the alternatives (again Cabot is the notable exception). This matter now requires consideration.

6.5. Alternatives to Placebos

This section will attempt to demonstrate that there are acceptable alternatives to using placebos as a general policy. Nothing that will be said rules out the possibility that in a few specific instances there will be no practical alternative to placebo use. Consider, for example, the physician called to the bedside of a patient who is in great pain, who is known to be allergic to all available analgesic drugs, and who speaks a foreign language for which no fluent interpreter is available. Both deontological and utilitarian defenses could be given for placebo use in such extreme circumstances. The deontologist might propose that the patient has a right to relief from suffering which overrides the physician's duty not to deceive; any of us, in the patient's place, would prefer being deceived to experiencing continued agony. The utilitarian might note the great net increase in good over evil that is available for that patient, while the negative consequences attendant upon adopting placebo use on a widespread basis do not apply here. But

hopefully the very implausibility of this case example suggests that situations open to these sorts of justifications will be quite rare.

If it could be shown that in general, attractive alternatives to placebo use exist, then the burden of proof would fall upon the placebo user; he would be obliged to show that some special, overriding circumstances apply in the case at hand.

In addressing alternatives, we must overcome a serious medical bias. Physicians are likely to consider as viable alternatives only other drugs, surgery, and similarly interventionist, body-movement-involving modalities; they are likely to reject talking with the patient, providing education or emotional support, or watchful waiting as "doing nothing" or "sitting on one's hands" (Benjamin 1976). This may be one reason why most of the medical discussions of placebos cited so far implicitly assume that placebos provide the only alternative to doing nothing (or having the patient seek another doctor).

It is on the matter of alternatives that our formal definition of 'placebo effect' (2.3) proves most illuminating for the ethical issue. After considering various sorts of limiting cases in 2.2, we were led to adopt a definition of 'placebo effect' which contained nowhere in it the term 'placebo.' On this conceptual basis, we established that using placebos is only one means to eliciting the placebo effect-- that the latter, in fact, pervades most of medical practice even where the former are seldom if ever used.[9] While Modell (1955) noted how widespread the placebo effect was and used this fact to justify the use of placebos,

9. Here we are speaking of a positive placebo effect, recalling that our definition allows for both positive and negative placebo effects (*cf.* Shapiro 1968).

we may draw the opposite conclusion-- since the placebo effect can be elicited by other, nondeceptive means, placebos need not be resorted to.

A practical model of how the placebo effect (by our broadened definition) can be elicited without any sort of deception is provided by a study entitled, "Reduction of Postoperative Pain by Encouragement and Instruction of Patients" (Egbert *et al.* 1964). While a control group of surgical patients received routine care, the randomly selected experimental group received a special preoperative visit from the anesthesiologist, who discussed the nature of postoperative pain and instructed the patient on relaxation and postural methods to minimize it. The surgeons (who were unaware that the study was being conducted) ended up giving the experimental group half as much narcotic and discharging them two days earlier from the hospital, on the average, as compared to the control patients.

Writing some years before Adler and Hammett's 1973 paper, Egbert and co-workers anticipated nicely the "meaning model" of the placebo effect (1.5):

> We believe that our discussions with the patients have changed the meaning of the postoperative situation for these patients. By utilizing an active placebo action, we have been able to reduce their postoperative pain (p. 826).

Specifically, for these patients the idea of not-talked-about pain which one had to lie back and endure had been changed to the idea of predictable pain which one could control by learned measures.[10]

10. For a similar example of changing the "meaning" of an illness experience without the use of deception see Case 1 in the Conclusion, below. While Cassell does not use the term 'placebo effect,' his strategy of teaching the patient greater control over bodily function seems similar in principle to Egbert's (Cassell 1976, pp. 154-162).

One example such as the Egbert study, however, might not assure skeptics that alternatives to placebo use exist in a wide enough variety of cases. Therefore it may be useful to return to the seven specific indications for placebos suggested by medical authorities (6.4). If in each we can suggest ways of eliciting the placebo effect without using placebos, we might conclude that the alternative modes might well work in other instances also, where the desirability of placebo use from the medical standpoint is less clear. (We can eliminate #1, which was shown to be tantamount to unlimited placebo use.)

2) No active treatment: There is almost no disease for which supportive care and compassionate concern cannot contribute to the patient's comfort, even where cure is impossible. Further, to give placebos to make the patient think that cure is possible is to deceive the patient not only about the treatment but also about the prognosis (*cf.* Cabot 1909, pp. 138-157).

3) Narcotic withdrawal: The medical management of withdrawal symptoms, including temporary substitution of less addicting drugs (such as methadone substituted for heroin), is well developed. Psychotherapy to remove emotional need for the drug is much less successful; but placebo use, reinforcing the drug-taking habit as a way of dealing with life stress, hardly seems any more helpful.

4) Prolonged diagnostic testing or uncertain diagnosis: The doctor can level with the patient about what is known so far and what isn't; when some potentially serious diseases have been ruled out, he can give reassurance on that score ("At least we know now it isn't cancer"). What discourages the patient is probably not the uncertainty so much as the suspicion that he is getting a "runaround" from the physician.

5) Anxiety: A number of specific anti-anxiety drugs, and short- and long-term psychotherapy are all available to deal with this problem.

6) Patient demands treatment when none indicated: This situation is difficult. The demand may be symptomatic of an underlying emotional problem, such as excessive dependency needs or denial of the true nature of the illness (especially if the patient secretly fears that the illness is psychological). If these deeper problems can be confronted, the strategy of making the patient feel more in control can be especially useful (Cassell 1976).

7) Temporary use: Instead of using some temporary means until a trusting relationship is established, why not speed up the establishment of the relationship? In general the physician who takes time to hear the patient's true concerns, responds frankly to questions, and indicates to the patient what he can expect in the future, establishes considerable trust almost immediately.

Two rebuttals suggest themselves to the above examples. One is that the alternative techniques described have a high failure rate under the day-to-day pressures of medical practice, when the physician cannot always appear as unhurried and as compassionate as he would like.

Two replies are clear from Chapter 1: placebos generally have only a 30 to 40 per cent success rate in most studies; and a placebo is still less likely to work if the physician who administers it does not display compassion and concern.

The second rebuttal is the matter already mentioned of the cost-effectiveness of spending more time with patients. In the absence of solid data, one could assume that establishing trust and teaching the

patient more control over his own body takes quite a bit longer than writing a prescription for sugar pills or injecting some vitamin B_{12} (Fuchs 1974, p. 125). But one could also assume that communication with and education of the patient is in some sense an investment for the future; the informed patient may handle subsequent symptoms with minimal medical attention while the patient given placebos will continue to come back for more and different placebos. Furthermore, current strategies toward patient education emphasize the role that can be played by nurses and other health professionals in order to minimize the required time of physician contact.

Pulling all this discussion together into a compact argument, we find that we have not created an absolute moral prohibition against placebo use, but that we have established a *prima facie* presumption against it and placed the burden of proof back onto the user. Our position has both a deontological and a utilitarian rationale. Deontologically, not deceiving is preferable to deceiving. From a utilitarian standpoint, we can compare the consequences of placebo use as opposed to eliciting the placebo effect by nondeceptive means, assuming that the two techniques in general are equally effective.[11] The nondeceptive means eliminate all the negative consequences listed by Cabot and Bok (6.3). Since nothing has been concealed in the first place, the patient cannot discover later that he has been deceived; hence, the general public can never come to suspect that doctors routinely deceive people. And greater patient education, far from promoting self-perpetuating,

11. Even if nondeceptive strategies were somewhat less effective, as compared to placebo use, the gain from avoiding the negative consequences of deception could offset this in a utilitarian calculus.

counterproductive health habits, is likely to have the opposite effect; in the long run this approach may in fact prove far more cost-effective than acute illness care (Kristein, Arnold and Wynder 1977).[12]

There is yet an additional dimension to this line of argument, which arises from the notion of the person that we developed in 5.1. Being a person necessarily involves a capacity for autonomous, responsible behavior. If we are concerned about the actualization and not just the mere existence of our human capacities, we seek to maximize responsible behavior; and deceptive practices, which deprive us of the information we might need to make decisions in a responsible way, are to be avoided. Indeed, it seems that placebo use could never have become ingrained in medical practice had not physicians (with the tacit acquiescence of patients) developed a general approach of treating patients as "nonpersons" in this sense-- on the basis of the assumption that patients cannot be made to understand technical medical information, and are too emotionally distraught by the illness experience to behave responsibly. (In the Conclusion, we shall consider further points of mutual reinforcement between the capacity theory of the person and considerations in medical ethics.)

In summary, the use of placebos has a long historical tradition among physicians, who have seldom raised any ethical issues in connection with this practice. Arguments that have been made against placebo

12. Conceivably, overemphasis on self-control of symptoms and avoidance of medications and surgery could cause patients to delay in seeking needed medical attention for serious conditions; but this potential negative consequence can be avoided by additional patient education (*e.g.*, Vickery and Fries 1976).

use take the form of deontological or utilitarian condemnations of deception. Defenders of placebo use generally emphasize the efficacy of placebos and fail to take into account alternative means of producing similar results. Our formal definition of the placebo effect illuminates this debate by pointing out that placebos are not necessarily needed to elicit the placebo effect; enough practical examples can be given to warrant further study and trial of nondeceptive modes of eliciting the placebo effect. This, in turn, brings up the question of future lines of research into the placebo effect; that will comprise part of the material for discussion in the Conclusion which follows.

Conclusion. Recapitulation and Research Implications

In conclusion, I shall return to the "reflective equilibrium" strategy that was laid out in the Introduction, to take stock of the extent to which our discussion of the placebo effect has achieved those goals, and of what remains to be done to complete the program. I will then briefly indicate some of the implications that our philosophical investigation of the placebo effect has for further empirical research.

Recall that we originally set out in search of 1) an empirical-conceptual account of the placebo effect, to get clear on the nature of the phenomenon and its boundaries; 2) a theory of the mind-body relation consistent with what we know about placebos; and 3) an ethical position on the use of placebos in therapy. We desired that each of these three "corners of the triangle" (a) satisfactorily address the problems of the placebo effect; (b) contribute to our understanding of the other "corners" in turn; and (c) agree with or extend our existing considered judgments in these domains, on matters other than the placebo effect.

The empirical-conceptual corner was provided by reviewing the empirical data on placebos, subjecting it to critical analysis, and finally formulating a definition of 'placebo effect.' The mind-body corner took the form of the capacity theory of the person, which provided a symbolic-cultural dimension missing from more traditional philosophies of mind. And the normative corner essentially restated the accepted ethical arguments against deception, while amplifying them in the

placebo case by showing how unexplored alternatives exist that might replace deceptive placebo practices.

Further, each corner was found to agree for the most part with representative considered judgments. The formal definition essentially arose out of the considered judgments arrived at by looking at illustrative examples of the placebo effect and related phenomena. The capacity theory of person was found to give illuminating answers to many puzzling issues in philosophy of mind, including disembodied minds, psychophysical causality, and self-consciousness. And the ethical analysis provided answers in the placebo case that are fully consistent with both deontological and utilitarian modes of reasoning.

Finally, each corner of the triangle was supposed to shed light on the other two corners. Two prominent examples of this were seen, both arising from the formal definition:

1) The definition gave prominent attention to the belief of the subject that he is within a (culturally designated) healing context. Thus, mind-body views unable to give full accounts either of belief states, or of the cultural dimensions of mental life, were found to be less satisfactory than the capacity theory of person.

2) 'Placebo effect' was defined independently from 'placebo.' The suggestion that a placebo is not necessary to elicit the positive features of the placebo effect raised the question of alternatives to the deceptive use of placebos, a dimension which most previous ethical discussions had ignored.

But, although these were not developed in full, we have at least some suggestion of further points of mutual illumination. One is the ethical import of the notion of 'person' developed in the mind-body

context. Present thinking in medical ethics has emphasized that our concern, instead of being for life in the abstract (Clouser 1973), is more properly directed at the life of persons, a class which is not necessarily coextensive with the class of human beings (Engelhardt 1975*b*). The sense of 'person' indicated in these ethical contexts is that of a being who can properly be said to be the subject of rights and interests. If some humans, perhaps fetuses or the irreversibly comatose, turn out on analysis not to be persons in this sense, they cannot be said to have the right to life that persons have by virtue of being persons, and their deaths might be permissible if other moral values could be served thereby (*e.g.*, Tooley 1972; Feinberg 1976). And in general these arguments proceed on the basis of capacities that human beings typically have and that these special classes of humans lack. If the capacities by virtue of which a being can be said to be the subject of rights and interests turn out to be reasonably congruent with the capacities to use symbols, by virtue of which certain bodies are persons according to the capacity theory, then both the ethical and the metaphysical notions of 'person' would turn out to be mutually reinforcing. Where the congruence between the two notions is inexact or troublesome, investigation of the problems that arise would provide important insights both for ethics and for philosophy of mind.[1]

We have thus seen at least in outline form a conceptual-metaphysical, a conceptual-normative, and a normative-metaphysical "side" to our

1. Kenny's language, "capacity to acquire the ability" (1973*a*, p. 47) might suggest support for the position that fetuses are persons in the full sense. I think, however, that one must distinguish between the *capacity* to acquire the ability to operate with symbols, and the *potential* to acquire the ability; fetuses seem to possess the latter but not the former. This needs, of course, to be defended at greater length.

triangle. Of course, each instance of mutual illumination works both ways-- if our formal definition gives us important clues to the ethical problems, then the fact that it does so provides further support for the formal definition itself. Clearly all of these issues need to be explored in much more depth than has been possible here. What is important is that the placebo effect not only can be clarified by philosophical scrutiny, but can also serve as a focus for several promising and mutually reinforcing inquiries in philosophy of medicine.

Any philosophical inquiry into medical matters will be judged in part by whether it suggests opportunities for empirical research and eventually for treatment applications. Several general areas of research seem to be suggested by our discussion of the placebo effect. In some cases we might venture to suggest hypotheses which are supported by preliminary research findings; in other cases the research has yet to be carried out.

Among several proposed theories of placebo action reviewed in 1.5, the "meaning model" of Adler and Hammett comes closest to encompassing the essential features arising from our discussion. By this model, the subjective sense of "meaning" in the illness experience is broken down into two factors: 1) "system formation," or the providing of a coherent explanation of the illness consistent with the patient's world-view; and 2) "group formation," or the gathering of a supportive caring group around the patient. Together, these factors "are invariably used in all successful interpersonal therapies, and are the necessary and sufficient components of the placebo effect" (Adler and Hammett 1973, p. 597). By "system formation" these authors indicate the cultural-symbolic realm

whose importance we have stressed; and "group formation" brings into the model the sociological insights that have been gained into the workings of the "sick role."[2] Thus, the model directs research toward both the cultural and the social aspects of human nature.

The meaning model suggests an important corrective for the bulk of placebo research on personality variables, which has focused almost exclusively on the emotional states of the subjects without looking at their assumptions or systems of belief.[3] The central role that belief systems can play in the placebo effect is suggested by two case vignettes:

> Case 1. "A woman of Christian Science faith failed to heal despite the relative simplicity of the surgical procedure [to correct retinal detachment]. Afterwards, she indicated to the surgeon that having surgery was in conflict with her Christian Science beliefs. Before reoperating, the surgeon made clear to her that he was only doing a mechanical task akin to realigning a broken bone, and that her faith was the major factor in the actual healing. His statements helped her to reconcile her Christian Science beliefs with the necessity of surgery, and she healed quickly after the second operation" (Mason *et al.* 1969, p. 139).
>
> Case 2. A man with far advanced lymph node malignancy, and with readily palpable, large tumor masses in the neck, abdomen, and groins, learned of the appearance of a new "miracle cancer drug," Krebiozen, in the newspapers. At his insistence he has included in a clinical trial of the drug, against protocol regulations since his physicians felt that he had no more than two weeks to live. Within ten days he had demonstrated marked regression in the size of the tumors, and where he had previously been bedridden and gasping for air, he was well enough to be discharged from the hospital. After two months, however, news reports began to circulate carrying more discouraging news about Krebiozen, and the patient

2. For references on the sick role see Footnote 7, Chapter 2.

3. This is not to suggest that emotional factors are not important. Frank, reviewing studies of faith healing at Lourdes, noted that visitors who are emotionally involved, either as believers or as skeptics, were more likely to have symptom relief than the indifferent ones (1974, pp. 71-72). Clearly both emotions and beliefs need to be studied in any comprehensive research into the placebo effect.

> returned to the hospital with return of symptoms and recrudescence of his tumor masses. The physician then announced that it had been discovered that the first batches of Krebiozen deteriorated with storage, and that a shipment of more potent drug was about to be received. He then proceeded to give the patient injections of plain water. Again, in a short time, the tumors shrank and the patient had nearly total symptom relief.
>
> The patient remained healthy after this for some months until another news report appeared: "Nationwide AMA tests show Krebiozen to be worthless as cancer treatment." Within a few days the patient was readmitted, very depressed, and with far advanced symptoms; he died less than two days later (condensed from (Klopfer 1957)).

The focus on meaning further indicates the necessity for cross-cultural studies of the placebo effect and of the healing context. We have already shown that what counts as a healing context can be expected to be different from culture to culture. To my knowledge, anthropologists have not carried out cross-cultural studies of the placebo effect as a distinct phenomenon, even though their studies of culturally-related medical practices in general have provided valuable clues into the workings of the placebo effect (*e.g.*, Kleinman and Sung 1976). Of special interest is the conflict produced by the introduction of "Western scientific" medicine into a traditional culture; if both scientific and traditional medicine rely on the placebo effect for much of their efficacy, scientific medicine might be expected to show a clear superiority "only to the extent that scientism has become a successful ideology" within that culture (Riley 1976). Furthermore, understanding the meaning of illnesses within a culture and the social-stabilizing functions of healing practices within a society are essential in order to compare the efficacy of medical-care systems; what one defines as "disease" and what one considers to be "control" over disease can be expected to vary from culture to culture (Fabrega 1976).

In medicine, the diagnosis is the primary mechanism for conferring

meaning upon an illness event. While medical thinking has tended to distinguish carefully between diagnostic and therapeutic interventions, the meaning model suggests that diagnosis may in part also be treatment. One would hypothesize, then, that from among a group of patients with similar complaints, those given both a placebo and an understandable diagnostic label for their symptoms would have more relief than those given a placebo alone. This is important for "group formation" also: "...[W]e see how important it is that illness be given a legitimate name, that a sufferer have a mantle for his distress that society will accept" (Cassell 1976, p. 66).

The ways in which diagnostic labelling suggests meaning to the patient need to be studied more fully. It is noteworthy, for example, that people on a waiting list to be seen at a psychiatric clinic showed a cure rate significantly above the spontaneous-remission rate for their neuroses before they had actually been seen for treatment (Sloane *et al.* 1975).[4] Thus, merely being accepted as a prospective patient by a psychiatric facility may count as sufficient "diagnosis" to give enhanced meaning and symbolic coherence to the patient's subjective experience. For these patients the waiting-list itself apparently counts as part of the healing context. It would be interesting to see if such a phenomenon could be documented among patients on a waiting list to receive treatment for somatic complaints.

Certain behaviors of patients which are puzzling at present become

4. A problem with this interpretation of the study is that the subjects were interviewed by a psychiatric assessor to determine symptom severity prior to being placed on a waiting list; this "strictly evaluative" (from the authors' standpoint) intervention may have been perceived by the patients as therapy.

more understandable when the symbolic function of the healing context is taken into account. There is a growing body of medical literature on why patients often fail to comply with the medical regimen once it has been prescribed; most of the literature assumes that the patient comes to the physician to receive the prescription for the regimen, so that failure to comply is therefore irrational behavior. On the contrary, if patients come to physicians largely to confer meaning on the illness experience, this function has been completed once the physician pronounces a diagnosis and reinforces it by writing a prescription; the actual taking of the drug may be less important (Pellegrino 1976*b*).

Other situations besides the healing context can markedly change one's sense of meaning, and the meaning model would suggest that these situations would also have the power to influence physical symptoms. A growing body of research has correlated the quantity of "life change," such as changing residence, changing jobs, retirement, marriage, and death of a family member, with the likelihood of developing an organic disease in the months following (Rahe *et al.* 1964; Holmes and Rahe 1967; Rabkin and Struening 1976). An important feature of such findings is that the quantity of change is a stronger predictive indicator than whether the change is commonly viewed in positive or negative terms (*e.g.*, marriage and divorce both affect health equally).

Another focus for research might be comparisons between the placebo effect and related phenomena. An interesting parallel might be drawn, for example, between the placebo effect and psychotherapy. Jerome Frank, in his very perceptive *Persuasion and Healing* (1974), compares the various contemporary schools of psychotherapy, as well as comparing psychotherapy with the placebo effect, faith healing, shamanistic

healing rituals, and religious revivalism. He concludes that there are important shared elements, and that in terms of explaining their general levels of efficacy, the similarities among the psychotherapeutic schools are more important than their differences. Frank lists four features as common to all schools of psychotherapy: 1) the patient's confidence in the therapist's ability and desire to help; 2) a socially sanctioned healing locale, especially one in which the patient can behave in ways that would not be acceptable elsewhere; 3) a "myth" or basic conceptual paradigm to explain the patient's symptoms in broad terms; and 4) a task to perform that involves the patient actively and which, by giving initially successful results, counteracts the demoralization that most patients seeking therapy have experienced in life (pp. 325-330).

It is immediately apparent that these factors are precisely those which might be expected to enhance "system formation" and "group formation" in the meaning model-- that is, that the factors most responsible for success in psychotherapy might be the same factors responsible for the placebo effect.[5] To say this is certainly not to denigrate psychotherapy in any way. Chapter 1 provided ample evidence of the great power of the placebo effect, and anything that can claim for itself even part of this power deserves to be recognized as a highly effective therapeutic modality. One might view psychotherapy, in this regard, as a highly organized way of bringing the placebo effect to bear on a special class of patients who otherwise would be very resistant to it,

5. Frank himself denies that psychotherapy relies upon the placebo effect for its results, but this conclusion may arise from the relatively more narrow way in which he construes the placebo effect, particularly skimming over the importance of belief systems and emphasizing emotional factors (1974, pp. 136-164).

except as an immediate and limited response to very specific symptoms.

Frank's list of common factors brings up one additional point of interest, in that factor #4 emphasizes the importance of having the patient acquire a sense of mastery or control. The meaning model might be said to include mastery and control by implication, since one of the primary reasons for understanding events is to be able to control them; but perhaps mastery and control are important enough concepts so that they ought to be included explicitly as part of the model. We saw in 6.5 that techniques that increase the patient's sense of control over the illness offer attractive alternatives to deceptive placebo use (Egbert *et al.* 1964; Cassell 1976); one patient has described how feeling that he was being made a partner in the therapeutic enterprise represented a turning point in his illness (Cousins 1976).

According to the capacity theory, to have a mind is to confer meaning on the world through the use of symbols; and to use symbols is to have purposes and to engage in responsible behavior (5.1). We can see, then, how intimately the concepts of meaning, mastery, and personhood are interconnected; and one high priority for the field of philosophy of medicine ought to be the exploration of the impact of illness on the human person in light of these concepts. Cassell, for example, suggests that illness restricts in a very fundamental way one's capacity for rational behavior (1976, pp. 38-45); to what extent is this true, and to what extent does this influence how we ought to treat the sick? In this area empirical issues are closely bound up with philosophical ones; behavioral scientists might engage in a more detailed analysis of how patients move into and out of the sick role, and how their subjective sense of meaning and control is altered in accordance with

the stage of their movement (Siegler and Osmond 1973).[6]

In Chapter 2 we alluded to the placebo effect as an anomaly within the presently accepted medical paradigm. An important feature of anomalies is that once they become the focus of research, they can lead to the overthrow or at least the modification of the existing paradigm.

To many critics of modern medicine, this existing medical paradigm is characterized by failure to embrace the "whole person." Instead the individual has been reduced to a seat for pathology or to a physiological mechanism.[7] On this account, medicine has engaged in a dissection of the "person" similar to the outrage that is said to have been committed by the Cartesian dualist tradition in philosophy of mind.

But if a modified paradigm is to be offered to replace the existing one, if the "whole person" is to be more than a slogan, then some comprehensive philosophy of the person is required (Engelhardt 1975*b*).[8] This view of the person must be conceptually sound, consistent with the best empirical knowledge, and suggestive of new lines of empirical inquiry. It must embrace the person as a biological organism, as the subject of rights and interests and as the bearer of duties, as a

6. Barnlund (1976) notes that factors complicating interpersonal communication are at their height in illness contexts, and outlines research possibilities in the symbolic and communicative aspects of illness.

7. Since the current concern with "whole-patient" medicine is sometimes termed "neohippocratic," it is interesting that some of medicine's modern sins can be traced back to the concepts and practices of the ancient Hippocratics, who, "with their drive for rationalism and objectivity, were casting aside the use of the spoken word in medicine and were laying the basis for the modern physician who does not speak to his patients" (Cassell 1976, p. 56).

8. Indications of the need for a comprehensive view of the person have come also from fields outside philosophy-- *e.g.*, (Fletcher 1972; Trosko 1975).

subject of conscious self-awareness, and as a dweller within society and culture. Our discussion of the placebo effect has touched upon all of these features. Properly developed, a study of the placebo effect can do much to highlight the centrality of the "whole person" both to philosophy of medicine and to medical practice.

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