

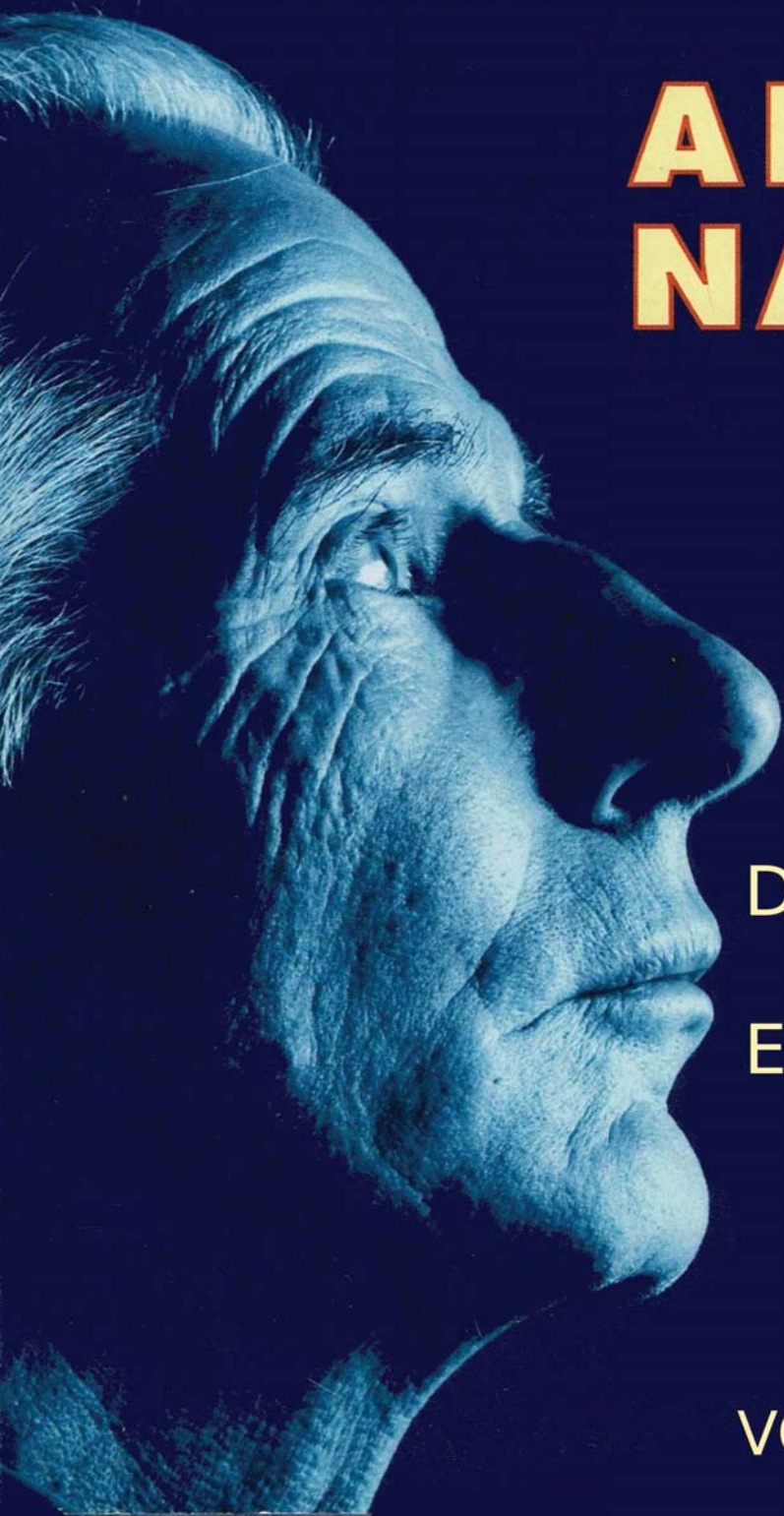
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V

Dialectics of Modern Epistemological Scepticism

Introduction

A good deal of contemporary discussion about knowledge incorporates arguments attributed to “the sceptic.” But as we have seen, this figure is a very different person from the sceptic outlined by Sextus Empiricus. Typically, scepticism is treated as a position that, if true, would be fatal for any attempted *philosophical* justification of ordinary notions of everyday or scientific knowledge. In fact, it might not be inaccurate to say that the position has been generated out of the very attempt to provide such a justification; at any rate, the significance of the modern sceptic’s arguments lies precisely in their power to demonstrate that no such attempt can succeed. Sextus Empiricus would label those who hold such a position Academicians, people who hold an absolutist kind of view that he clearly distinguishes from the “way” of the sceptic.

The modern sceptic, or epistemological sceptic, is made to argue that knowledge is impossible, to *insist* that we cannot know anything, to *affirm* that it is true or can be known that knowledge is not possible.¹ Naturally enough, although this sceptic’s arguments are taken seriously and some steps toward them are even accepted,² the position they support is not one that any flesh-and-blood philosopher is anxious to occupy. Indeed, to be forced to do so would most likely be seen as a professional defeat by those who look on the philosopher’s task as that of explaining, rather than replacing, well-established everyday beliefs. It is because the sceptic’s reasoning is considered intuitively false and yet logically impeccable that “his” arguments are subjected to close and careful epistemological scrutiny. Two features of modern accounts of scepticism are significant to our discussion, their avoidance of contextual considerations and the elaborate terminology and conceptual structures in which the arguments are framed. A typical proce-

dure is to consider with regard to some statement whether there are sources of error such that later events might cause one to retract a claim that the statement is true. If not, then the statement is "incorrigible," and we have no right to persist in refusing to affirm its truth on the grounds that it might be false. If there are conceivable sources of error, then the statement is "corrigible," and we are faced with the problem of whether or not we can "legitimately" claim to know that it is true.

To cope with this problem, a number of concepts are then introduced and developed. The effect of this is that the question of whether it is correct to use expressions of the form and normal intent of "I know that *p*" becomes a matter to be dealt with within an elaborate conceptual framework. The assumption is that such frameworks provide the terms in which a *general* question of justifying knowledge claims is to be answered. This general question is then treated as something over and above the question of particular contextual justifications for the use of expressions ("Should I have said 'thank you?'"). The epistemologist presumes his conclusions to apply in all contexts. Although he acknowledges that for all practical purposes there is no need to refer to his arguments, he thinks of them as showing us what, strictly speaking, we can or cannot say. The assumption seems to be that any well-equipped society catering to the enlightened interest of its members would make philosophical conclusions about knowledge available publicly, perhaps in reference libraries or, better, as an addition to the telephone service so that people could at any time or in any situation or stage of debate be given the authoritative view on the matter. (We might sympathize with the unfortunate man entrusted with the task of compiling such conclusions. Faced with arguments, authoritative and apparently technically proficient, to the effect that "material object" statements are/are not conclusively verifiable, that perception is a direct/indirect relation, and if indirect that "perception" statements then can/cannot be known with certainty to be true/false, that in making such statements we make/do not make inferences that, if we are to justify the claim to know such statements to be true must be/need not be valid inferences, and so on, he would almost certainly recommend suspension of judgment in his letter of resignation. Strictly speaking, of course, any headway he made would only add to the confusion.)

To deny that there were general questions about the applicability of knowledge expressions besides the considerations of context would be fruitless. It is a matter of fact that people ask them. But when they are asked and

answered in terms of explicit conceptual frameworks that have no part in ordinary usage, the relevance of the answers to ordinary knowledge claims becomes problematic.

The problem can be thus posed: does epistemology concern knowledge claims in general or knowledge claims only as they are understood by epistemologists, that is, in their own special frameworks? If the former, then we must suppose that the typical epistemological conclusion—for example, that knowledge claims about material objects cannot be justified—leaves the ordinary kinds of justification intact. The latter are all right in their place, and it is not for the epistemologist to question, say, the standards accepted among bird-watchers or people who want to know the time. He does not wish to pose as a more conscientious bird-watcher and time-teller; nor does the epistemological sceptic conclude that no bird watching or time telling can be conscientious enough. Rather, he questions whether the standards accepted by bird-watchers and time-tellers amount to valid inferences, whether they allow us to bridge “logical gaps,” and so on. The question of justification, therefore, is one that he takes up at a quite different level and in a terminology quite alien to that of the average, or even exceptionally scrupulous, bird-watcher or time-teller. His attitude, in fact, is parallel to that attributed to the sceptic when, as we noted in an earlier chapter, he is said to arrive at a *modus vivendi* by accepting that it is one thing to live (to watch birds and tell the time) and another to philosophize (to make valid inferences and legitimate claims). Then, however, we pointed out that since the Pyrrhonian sceptic accepts no philosophical framework, this *modus vivendi* is not available to him. In effect, a justification of Pyrrhonism is based on the fact that he can respond to his surroundings verbally, and in other ways, in a manner that does not require a corresponding response to propositions. Thus, he is not compelled to adopt two minds toward a proposition, one practical and the other theoretical. The propositions he is asked to accept on the basis of his experiences have a depth of intention that bears no comparison with that of his own straightforward acknowledgments of the appearances.

This suggests a totally different view of the significance of epistemological discussion, and it is this view, the second alternative, that we shall explore now. We shall look at philosophical discussions of knowledge as linear extensions, a kind of tenacious continuation, of ordinary dialogues with a stress on explicit frameworks that is not found in the initial stages. In-

stead of regarding the epistemologist as an *ex officio* member of the community, we shall see him as a participant in a comparatively rare kind of dialogue with a stress on explicit frameworks. The effect will be to present his conclusions, in particular sceptical conclusions such as "Nothing can be known" and "No statement can be known to be true," as statements in need of severe qualifications.

Standards Relative to Stage of Dialogue

Our main task will be to explore, first, the kind of debate or sets of verbal exchanges that foster or tend to foster (modern) scepticism, and second, the possibilities of rationally reconstructing such a tendency. This line is adopted because it seems to me that despite the wide acceptance of the view that we ought, as philosophers, to appeal to particular situations or occasions, there has been a sad and significant neglect of one kind of situation, namely that which occurs as a link in a long chain of discussion. Such links, and the chains they help form, bring out an important aspect of the use of language that can do much to illuminate the dialectics of modern scepticism.

Philosophically, the most familiar kind of verbal chain of discussion is the dialogue, and the points I would like to elaborate here arise from simple considerations of the way in which, after three or four exchanges of views in a dialogue, an expression like "I know" can come to be used in a number of quite different ways.³ More particularly a use of "I know that *p*" at the opening of the debate may be very different from an "I know that *p*" repeated after a long argument for and against *p*. The claim to know at the opening of the dialogue could be an innocent offhand gesture compared with a coolly insistent stand in the face of arguments and after extensive deliberation.

It is because of this stress on developing dialogues that I call my notes on modern scepticism dialectical. Perhaps I should apologize to Hegelians for this old, but not very profound or Hegelian, use of the word, and to Austrians for employing such a pretentiously solemn one.

Consider first this example of a simple dialogue and its consequences for the use of "I know":

1. A: Do you know where Mr. Jones lives?
2. B: Certainly I know. 12 Park Avenue.

3. A: Letters to him written to that address are returned by the post office.
4. B: Is that so?
5. C: Do you know where Mrs. Jones lives?
6. B: Mm, I was sure 12 Park Avenue was correct. But after what you said—I'm not so certain.
7. C: Do you know the address of *any* of the people we are going to invite?
8. B: I'm sure about some of them.
9. C: Well, this is of deadly importance. Do you actually *know* any of those addresses?
10. B: I dare say I do know.

Now, if at step (10) of the dialogue, *B* persists in using the term *know*, he is likely to have used more rigorous requirements of evidence than at (2). He is also likely to answer with higher definiteness of intention in relation to both relevance of evidence and gap (or difference) between evidence and the evidenced. Or, to put it more simply, he sees the need for evidence as he is made more aware of the divergence between his contentions and the evidence he has for them.

This shows that what an individual finds reasonable to require of an "I know that *p*" claim will show variation as we move along the steps of a dialogue. Moreover, any such variation might well be classified as "reasonable" since there seems no reason why we should describe standards of evidence that remain constant as reasonable or normal and take the fluctuations in particular cases as deviations from a stable norm.

Consider, for example, how three people, *A*, *B*, and *C*, might embark on the complicated project of giving a fairly large party. "Knowing where somebody lives" may, after some (normally) heated dialogue, rank high in regard to required *standard* of evidence, that is, in what *A*, *B*, and *C* accept as *adequate* evidence—higher, maybe if such a comparison could be given a more satisfactory meaning⁴ than the overworked chemist requires when asking his assistant (at the beginning of a dialogue): "Is the stuff to my right or the stuff to my left triamidotriphenylcarbinol? Do *you* know?" The everyday and the scientific are not clearly separated, nor the philosophical and the unphilosophical, as to level of requirements. Variation in stage of

development in any (normal) interaction between persons can show marked (yet normal) fluctuation in standard of evidence.

In order to avert a premature suspicion that in talking of variation in terms of increase in requirements I wish to push readers in the direction of scepticism, let me stress that at the end of a dialogue, requirements may very well drop. Suppose *C* is to be the financial supervisor of the party and that he tends to say, "I do not know the price; I shall have to check it and you must help me." If *A* and *B* discover that what *C* labels his "wild guesses" are correct to the penny, there will be recriminations: "You say you don't know, but you know very well." *C* has to bring his requirements down a step; his environment will legitimately put pressure on him in that direction. He has been overcautious and hampered the preparations for the party.⁵

Thus, either a lowering or an increase of standards may be the "normal" after certain phases of a dialogue. Instead of the "normal," however, I would prefer to talk here of the "rational," that is, of standards being decreased or increased for good practical reasons.

Maximum Requirements

It is quite natural that increases in requirements will be discussed in terms of how to *avoid* mistakes, how to *guarantee* truth, of how to eliminate *every* source of error. But, of course, all these strong terms must be taken with a grain of salt. In practice, *far-fetched* sources of error, such as earthquakes in regions where for a long time there have been none, will be frowned on as motives for changes in requirements. Although "every source" means every source, one must not be fanatical about it; one may, reluctantly or impatiently, acknowledge possibilities of error, yet dismiss them with an easy mind as of no significance.

In short, there is in everyday life always a nonconceptual frame of reference determined by practical purposes, responsibilities, and so on. A dialogue, however, may sometimes concentrate more and more on a conceptual frame, on a system of significations, and then every source may be taken in an absolute sense, that is to say, *every* source. Once this sense is given and assumed, it may then appear preposterous and ridiculous to ask for a guarantee of the elimination of every source of error, but the idea is still one that can be understood. That is, I can say, "I see what you mean, but to require

that *every* source be eliminated is of course to require the impossible. Who has ever asked for that?"

We should now take up the question of what the maximum increase of (rational) requirements of evidence would be in order to justify (not verify) "I know that *p*." But since here and in the rest of the discussion I shall assume that there is a requirement of evidence for justifiably saying "I know that *p*"—whatever the *p*—I should first of all offer some comments in support of this assumption.

In taking it for granted that if I say or think "I know that *p*," I can legitimately ask, at least myself, what the evidence is irrespective of the kind of *p* but not irrespective of the situation, I am assuming something that can be termed propositional universality, as opposed to situational universality. Ayer, Austin, and others have argued against the propositional universality of the evidence requirement, that is, against the view that evidence can be properly asked for whatever the proposition claimed to be true. But, although I shall not further substantiate my disagreement here, I think they have only succeeded in showing that for some statements—for example, about my own pain or where I live—a question of evidence is out of place in everyday life (see, e.g., Ayer 1961: 292). It is characteristic, however, that these cases are very near the limits of the useful application of "I know." Wittgenstein's remarks (1958: sec. 246) are relevant here, undermining the distinctions between "I know" and "I do not know," and between "I doubt that" and "I do not doubt that," in reference to such statements as "I am in pain." And if it is ever pertinent to say to someone else "*I know* I am in pain," it seems to be in just those cases in which others might have reasons to doubt. In what follows, I have for this reason not found it necessary to discuss occurrences of "I know" in which requirements of evidence are out of place.

Certain details in Ayer's exposition deserve closer attention here. In particular, Ayer defends the view "that there are some propositions that we can claim to know without having to know any other propositions as reasons for them." "If someone claimed to know that he was in pain or that he was daydreaming about being rich, it would seem absurd to ask him what evidence he had for believing that these things were so" (Ayer 1961: 292).

It would be absurd, perhaps, to expect *A* to put evidence at the disposal of *B* so that *B* could independently from *A* estimate the evidence *A* has at *his* disposal. For *A* to ask "Am I really daydreaming about being rich?" is

not absurd. He may have the suspicion that he is perhaps not daydreaming but making pertinent reflections: "What would I do if I doubled my income?" Or, he may on second thought find that he is or was dreaming about being a playboy—the question of money was not really touched on in the dream, he might have borrowed it. It is clear that as soon as one begins to become interested in exactly what happens and in classifying it by using certain terms, there are ample grounds for asking about evidence. If the evidence is expressed in words, propositions are formed, and whereas the dogmatist will claim them to be true, the sceptic will offer them for inspection.

In spite of his doubtful contention about the absurdity of asking (one-self) for evidence, the first quotation from Ayer, as far as I can see, expresses a tenable position. If I say "I know that p ," p may belong to a field in which I would not in everyday life form propositions in support of knowledge claims. The "How do you know?" may be left unanswered, or one may say, "Obviously I know," "I know that I know," and so on.

In certain dialogues, for example those concerning trustworthiness or introspective psychology, the status of knowledge that goes unchallenged in everyday life must be considered. The result always seems to be that insofar as the propositions state something at all, there are sources of error. Here the history regarding the psychology of degrees of intensity of attention affords an interesting field of study. This history shows that propositions that seemed to be based on the most direct, unquestionable introspection and that were made by trained observers, came to be considered doubtful and not acceptable as expressions of knowledge (cf. Spearman 1937).

To return now to the question of what would be the maximum increase of (rational) requirements of evidence in order to justify the expression "I know that p ," we can at least take it for granted at the outset that requirements may vary markedly, even in relation to a definite question, and that even in highly scientific contexts the requirements might be very low. Where would such a maximum increase land us? Would it *necessarily* prevent us from using the expression "I know that p "? Would it at least as a matter of fact prevent us from using it? In order to answer this, some preliminaries will first have to be discussed.

First of all, let us examine the basis for contending that there are definite requirements to be satisfied in order justifiably to say "I know." Perhaps the best way of bringing out the fact that there are such requirements is to

describe situations in which people are taken to task for having used the expression “I know” (and similar phrases) when allegedly they were not entitled to do so. From a variety of cases — some of which might be constructed, but agreed on by a panel of experts — one might then try to abstract rules. That is, one might try to construct more or less general requirement-hypotheses to fit the cases. (It would obviously be incautious to adopt general formulations proffered by people in concrete situations.) Such requirement formulations, with careful delimitation of intended range of validity, would not necessarily have any normative status vis-à-vis the language community, but they would at least represent attempts at clarifying and codifying the use of “I know” in terms of definite requirements.

It should be remarked, however, that it cannot be taken for granted that by increasing the number of cases surveyed, more and more definite requirements fitting the cases can be constructed. In general, there is in fact no good reason to believe in the “existence” of general regularities that would be susceptible to indefinite refinements in structure and content. In what follows, I shall speak as if one could abstract and extract requirements from surveying concrete cases of the use of “I know.” It is at least a useful fiction for purposes of exposition.

Maximum Strengthening of Requirements in the Face of Mistakes

Sometimes, even though the requirements of knowing have been satisfied, it is agreed a little later by those involved that there was, after all, a failure, a mistake. The normal thing to do, if it is important not to fail, is to take heed of the mistakes, that is, to increase the requirements covering that kind of situation in which, admittedly, a mistake was made. Then next time in such a situation I shall not claim that I know. My claim will be more modest, or I shall apply some tests such that requirements of a higher level are satisfied. Only then shall I say, “I know.”

Let us suppose that, unhappily, I make a mistake in the very same kind of situation in spite of applying the new, more severe tests. It will now be reasonable not only to increase requirements in the same kind of situation but also to increase them a little over a wider field. It is to some extent arbitrary how one is to assess the kind of situation at hand, and the consequences

it has for other situations. A decision will be made, however, and it can be described as affecting not just the old kind of situation but also a more general kind.

If we suppose that this unhappy trend goes on for some time, there are two kinds of adjustments that are of special interest: one in which requirements are always given an increase of rigorousness and in an expanding area, without covering any one specified area, and one in which eventually the use of "I know" is *in complete generality* taken to be incautious or inadvisable, other expressions being recommended in its place.

Let us inspect a hypothetical case study of how someone might have come to "know nothing":

On certain occasions I discovered that a proposition of whose truth I had been sure, and on adequate evidence, was in fact false. Having had adequate grounds for my certainty, I may be said to have been justified in claiming to know, but in fact did not know. This discovery of error in respect of a previous well-supported certainty occurred on numerous new occasions, and each time I felt greater reluctance to claim to know. Eventually, I came to lose all confidence in claiming to know, since I was never sure the evidence excluded sources of error. Because of my never being sure, I cannot now be said to know anything.

Now to what extent and in what way can such a report, biographical note, or psychological case study be relevant to the questions raised in the modern discussion on scepticism?

The actual use of "I know" among psychiatrically normal persons of a language community is at least relevant to conclusions about how requirements remain constant through time intervals, or are increased or decreased in various kinds of situations. It is difficult to see how one might legislate what would be a maximal increase beyond which one could not properly, or sincerely, refrain from using "I know." Therefore, if such an unwillingness to use "I know" became more and more widespread, we might envisage circumstances in which *knowing* became not only unfashionable but totally extinct, insofar as "I know" implies "I am convinced." Such a possibility is not to be excluded. After all, many other words have become employed more and more rarely and have tended to become extinct when occasions for their use grew sufficiently scarce; this might also become the case with "I know." As far as linguistic considerations are concerned, then, there are no objec-

tions to a process of development culminating in the extinction of the use of "I know."

Now let us look a little more closely at the repercussions of unexpected events on standard requirements. Repercussions on standards may be said to have three dimensions: breadth, strength, and duration. Breadth measures the class of cases or situations covered. If I make a mistake in Latin grammar in spite of satisfying the standards, I may for a time increase my standards both in Latin and in other grammars, or only in Latin. Where the failure is a damaging (shameful, embarrassing) one, I may even, for a short time, show increased standards in every subject. (All within the very elastic limits of psychiatric normality.) This is the breadth dimension. If we say that the increase may be very marked or only just perceptible, we are using the dimension of strength, which is self-explanatory.

Third, if we receive no more rude shocks we simply change back to old habits and the increase in requirements gradually disappears. Here we have a time dimension, one that may be rationalized by dividing up time in terms of situations and by making use of inductive principles.

So much for the repercussions of failures. But successes and triumphs—that is, astonishing, unexpected, and important successes in "I know" situations—also have repercussions. More often than not their effect may be a relaxing one; standards drop, and inductive thinking may be the cause. As suggested by Ayer (1956: 33), if an individual has a constant flow of successes, he may be led altogether to stop searching for evidence. This, however, would tend rather to mean that "said by Mr. So-and-so" was to be taken as adequate evidence. On the other hand, successes can sometimes result in increases. For example, I may wish to retain a reputation that if I say I know, others may be perfectly sure that it is true.

So much for individual successes and vicissitudes. There are, of course, fluctuations in collectivities, too—in communities interacting with individual experiences. During wars and other periods of intense community life and shared experiences, the collective changes are more marked and more easily break through the individual variations. (One *should* know the enemy soldiers are cruel. Whatever the incidents that in other circumstances would certainly have repercussions, one must continue to be willing to say "I know they are cruel.") In some kinds of situations, requirements for "I know" can drop toward zero; in others they might increase toward infinity.

Maximum and Abnormal Repercussions

We are tempted to classify some changes in standards as “irrational,” others as “rational.” Which classification we adopt depends to some extent on our system of values. In any case, there is always the possibility of constructing a set of rules from which we consider rational changes of standards to be derivable.

Suppose in giving a lesson in geometry I say that I know π to be 3.14158 . . . and later admit failure, being convinced by others that π is 3.14159. According to my (and many others’) criteria, I would behave irrationally if this prevented me from answering positively to the question whether I know that 5 multiplied by 6 is 30. It would be much too drastic a personal reaction to my failure for it to form the basis of a mistrust in my “knowledge” of the simple multiplication table; such a repercussion on standards would be unreasonable. But sometimes severe repercussions *are* reasonable. And even where they are not, the question of the use of “I know” cannot be solved by legislation according to the way rationalists would wish it to be used.

A succession of failures may cause me to refrain from using “I know” either in important matters or generally. The two cases are not as different as might be thought at first glance. If I renounce its use in important matters, I may still use it loosely.⁶ (“My wife never lets me out, you know.”—“I know?”—“Of course, I’m telling you.”) If reproved for doing this (“But you have just refused demonstratively to say *you* know what would be the right thing to do in our present predicament!”), I may point to the difference. (“Of course, you and I don’t, strictly speaking, know this about my wife. Have you lost your sense of proportion?”) When one is in a bad predicament and conscious of one’s responsibility, one finds it irresponsible to say “I know.” Thus, to refrain from using “I know” in important matters is in certain senses to give it up in general, as far as cognitive, serious purposes are concerned.

I may be considered normal psychiatrically, but still refrain from using “I know” in important matters for the rest of my life. Or I may promise myself to refrain from it, but fail; an unwillingness to say “I know” might, after all, in some situations or in some communities be as embarrassing as refraining from saying “thank you.”

To refrain from using “I know” in important matters may also express one’s awareness of a *general fallibility*. According to Austin, however, general fallibility by itself cannot be a rational motive for saying “I know noth-

ing" (in the modern sceptic's way); there must be additional premises. If stakes are small and there are discoverable differences in the chances of a mistake, why should I *always* avoid claiming to know?

Yet, and perhaps in extension of Austin's point, in matters of life and death, the awareness of the importance, the awareness of the terrible consequences of failure, may rationally motivate me to renounce the use of "I know," whatever the evidence. In relation to certain subgroups of situations, therefore, "I *know* nothing" may be an adequate expression of my opinion and attitudes (Austin 1961: 65–67).⁷ As pointed out by Ayer and others, in saying I know something, I am vouching for it—thus my assertion that it has some important social implications. But then if I find it unjustifiable to vouch for (and, perhaps, to swear to) something, finding that to do so would be inconsistent with my value system, I may stick to my *personal* conviction, yet still in a rational way, and in view of conceived failures *abstain* from using "I know," or even find it justifiable to say "I know nothing."

Socially I perform my duty as a witness, for example, when I painstakingly describe the evidence I think I have, adding on request any further information about my personal conviction or belief, and my personal estimate of the chances of my being mistaken. The rest I leave to the jury and others concerned. If some will say that in the light of my total statement I know, or that I do not know, I will not object. *They* may say that I know even if I would not. Only if I were forced to take a stand would I give my reasons for avoiding the use of the distinction between known and not known in questions of life and death. If, on the other hand, I do employ the distinction and am sure about something, so long as certain other requirements are satisfied, I would be perfectly correct to *say* "I know that such and such." This would continue to be so even if subsequent events proved me wrong. For then what was wrong was what I said was the case, not my saying that it was the case. It would be misleading here, too, to say that I did not know that *not* such and such if the context suggests that this implies some degree of blame: "You should have known that not such and such, but you didn't." The circumstances in which it is right to *say* "I know that such and such," even if what I claim is subsequently shown to be false, are precisely those in which it would be wrong for me to say "I know that not such and such" despite the fact that this latter claim would have been true.

The most important point for us here is to make a distinction between having a piece of knowledge and having good, standard, or adequate rea-

son, evidence, or grounds for being certain. If I hesitate in, or abstain from, saying "I know it" because I see (am sure I see) a *remote possibility* of being wrong, this does not necessarily violate any explicit or implicit rules of ordinary language. It may be a symptom of overcautiousness, hypochondria, hyperactivity of the imagination, or inability to square up to some formidable responsibility, but all of this may still be within the range of psychiatric normality. My mind is not deranged; I am still a member of my language community.

Moreover I may be perfectly aware that others are much more free than I am in their use of "I know," and disapprove of this—all *within* the framework of our common language, which is after all not the monopoly of one type of person.

One might bring out the cognitive factor separate from the use of "I know"—its rationality in terms of inductions and in terms of limitations on the fruitfulness of terminology. If there have been more failures than were expected and are tolerated within a sphere of action or investigation, it is reasonable to increase standards in order to decrease the risk of continuing a high rate of failure. No doubt some kind of inductive conceptual framework is best suited to articulate the rationality of the increase. Second, the fruitfulness of a distinction has its limitations. Sometimes the social aspect of vouching and guaranteeing is considered unnecessary. Why then use "I know"? If what I aim at is an independent evaluation of *p* by others, I modify your wording as a rational means for reaching that end. (A: But do you *know*? B: I am convinced, but *this time* you'd better see for yourself.)

"I Know Nothing": General Linguistic Counterargument

There have been various arguments designed to rebut the claim that one can reasonably abstain from the use of "I know," or the slightly different and stronger claim that the utterance "I know nothing" can be in order.

An argument that we certainly know at least one thing is sometimes based on a kind of linguistic *a priori*: since there is "in our language" (I should prefer "in the vocabulary of our language") a distinction between "knowing" and "not-knowing," there *must* be an exemplified difference between what is known and what is not known. How else could the distinction have been introduced and understood? After all, we *learn* a difference between the terms from actual instances; so if there *are* no such instances, then

we cannot learn about such a distinction. In much the same vein it has been argued that not all things can be illusory, at least some things must be veridical. Unless this were so the notion of illusoriness would lose all its point.

But surely a child could be taught to use "illusory" perfectly correctly by instances such as the difference between the neighbors' spurious Santa Claus (being only the father in disguise) and its own family's real Santa Claus, between the neighbors' naughty boy's spurious courage and his father's genuine courage? In a similar way, generation n of adults introduces the difference in terms of their (nonillusory) ideals and the (illusory) ones of generation $n-1$. The difference between "illusory" and "not illusory," as well as those between "known" and "not-known," "veridical" and "not veridical," can be introduced and learned on the basis of the beliefs or convictions of the persons concerned. Whether the beliefs are correct or not (in an absolute sense) makes no difference whatsoever.⁸

If boy A has learned to use "illusory" and "not illusory" simply by one hundred examples, and later in life he considers the cases of nonillusory things to be similar to the cases of the illusory, we may find him saying *all* things are illusory.

There is nevertheless a way in which a paradigmatic learning theory may be important in understanding rather different positions that have been termed sceptical. Thus, when writing his famous *Why Nothing Can Be Known* (*Quod nihil scitur*), Francisco Sanches (or Sánchez) cannot properly be said to have overlooked that he knew he had (or did not have) a toothache. There are several uses of "I know that p " (and related terms), and one of them relates to "matters important to the soul," such as religious, metaphysical, and scientific doctrines. This kind of use was already well established by the time of Sextus Empiricus, and Sanches, too, was certainly aware of it. Moreover, he may be said to have developed his awareness paradigmatically, seeing it applied solely in connection with doctrines, and not in connection with utterances such as "I have a toothache." But then by applying the known versus not-known distinction within the sphere of that particular use, Sanches neither had nor did not have a toothache, insofar as neither the assertion that one has nor the assertion that one does not have a toothache constitutes a doctrine.

The point of this example is that the terms *sceptic*, *sceptical*, and *scepticism* have been applied for a very long time, and they are closely connected with uses of "knowing" that may very well not be the first uses one learns as

a child. Nevertheless, such uses of “knowing” are learned somehow. A debate on scepticism might certainly refer only to “knowing” on the infantile level, but it ought to be clear that this does not cover all interesting uses of “knowing” and especially not those important in philosophical literature.

Circularity of the Sceptic's Argument

There is another shortcut aimed at avoiding scepticism: It is claimed that the utterance “I do not know anything” is absurd or inconsistent because I implicitly assert that I know that I do not know anything. But this anti-sceptical argument is untenable. It rests on the false assumption that the ways of announcing “I do not know anything” can be reduced to one, namely, “I know that,” or to some others from which “I know that” can be derived. There are, however, a number of ways of announcing that do not fulfill this requirement, for example, “I am perfectly convinced that” or “I believe that.” Sextus Empiricus can be studied with profit on this point.

Let us now turn to the utterance that we are presuming to be made in complete seriousness, “I cannot get to know anything.” Against its tenability, considered as a statement, there are strong arguments of various kinds. Let us first consider those arguments that claim the existence of conclusive, unretractable, and irrevocable evidence.

The Conclusiveness of Conclusive Evidence: Social and Linguistic Rightness and Truth

In philosophical debate there are still symptoms of a confusion between the right to say something—for example, the right to say “It is true that p ,” “There can be no doubt that p ,” “Of course, p ,” or “In this I cannot possibly be mistaken”—and the certainty that p is true. If “I know that p ” is interpreted in this confused way, the statement may be thought to be unretractable if in fact I had the right to say it and whether or not p is later considered false. Let the relevant distinctions be repeated once more.

To assert “I have conclusive (unretractable, irrevocable, decisive) evidence” or “This closes the matter once and for all” or “Here no doubt is possible” or “It cannot conceivably be otherwise” or “This is absolutely certain,” is justified in everyday life if certain requirements are fulfilled. In the

course of a dialogue standards may vary, just as in the case of “I know.” Unexpected kinds of failures and successes can occur, and they have repercussions of various kinds. Here is a little dialogue for illustration:

- A: The two papers *are* in this room.
B: That is simply inconceivable. I have searched for them all day in this very room. It is absolutely impossible that they are here. I know they are not here. The evidence is utterly conclusive.
A: Well, look here, then. Here is one of them.
B: So it is. I don’t understand this.
A: Should we look for the other?
B: Well, yes, we’d better do that.
A: It is not inconceivable that the other is here?
B: Hmm.

If *B* had searched the room all day, one would allow that he was entitled to use expressions like “absolutely impossible,” “perfectly certain,” and “utterly conclusive.” However, there is not the slightest guarantee that he would not have to retract. The justifiability of exclaiming “conclusive!” does not exclude having made a mistake. Douglas Arner (1959) stresses that in the face of conclusive evidence, any further demand for evidence is unintelligible:

It is quite true that grounds treated as conclusive are always short of a demonstration and even occasionally prove inadequate. The important thing is that conclusive evidence *concludes*: no demand for more evidence in the face of conclusive evidence is intelligible. (Arner 1959: 88)

If Arner could only tell us what conclusive evidence looks like so that we could infallibly recognize it and avoid talking unintelligibly! But his attention soon shifts to what *counts* as conclusive:

What counts as conclusive evidence is a matter of tacit, continuing agreement among the users of the language. We learn early that we are not to claim knowledge unless we have met certain requirements in the way of qualifications and evidence. These requirements are founded chiefly on what grounds have proved adequate almost all of the time. . . . (Ibid.)

Here there seems to be no distinction made between “The evidence for p is conclusive” and “The evidence for p satisfies what counts as conclusive evidence.”⁹ The latter may be true despite the falsity of the former, that is, if “conclusive evidence” is taken to imply truth, and I think it does for most interpretations. “I have conclusive evidence for p , but p may be false” does not work.

On the most primitive everyday level, there is perhaps a good deal of tacit agreement about what counts as conclusive evidence, but it certainly seems more appropriate to reserve the conceptual distinction between conclusive and nonconclusive evidence for when there are explicit premises and conclusions. The distinction seems to belong to that level of talk. And at that level, one is aware of the difference between having the justification for *calling* some evidence “conclusive” and conclusive evidence itself. Here, as with knowing, I may be perfectly correct in saying something, but completely wrong in what I say. What counts as conclusive evidence that a person is dead is, for obvious reasons, rather strong evidence among responsible physicians, but once in a million or more cases a frightful mistake is discovered. Some people, of course, will find these cases uninteresting because for all practical, normal, ordinary, everyday purposes they just do not occur, but others will find them extremely interesting.

“This evidence is (already) conclusive. May I, please, have some more?” This would be a rather original demand, but at least to me perfectly intelligible. For there may be people who make the mistake of not taking the evidence to be conclusive, and they may want me to be able to add some pieces, some additional witnesses, and so on. “This evidence must count as (can safely be treated as) conclusive. But may I, please, have some more?” This kind of question is not only intelligible, but to pose it may sometimes be a duty. There are no definite requirements of conclusiveness; they vary in relation to a number of factors (responsibilities, consequences of failure, past experience within a narrow field to which p belongs, past experience in a broader field, etc.).

The inadequacy of the everyday and the normality arguments for establishing what is conclusive evidence is well illustrated if one considers utterances such as “We are never justified in being absolutely certain” or “I do not know anything whatsoever” or “Nothing *can* be known.” We just have to reflect that there are, on the one hand, requirements for being justi-

fied in saying such things (try it out at some parties!) and, on the other, *admitted* failures; that is, those engaged may, after some time, retract and say “I *do* know something, after all.” Nor is the requirement for announcing “failure” universally higher than that for announcing “success”; that is, it is always equally possible that some failures may turn into successes.

I think we can safely say that sceptical locutions in everyday life are not intended to cover, systematically and in relation to current conceptual frameworks, an assertion “I do not know anything.” Compared with their epistemologically intended counterparts, such everyday locutions are offhand and unsystematic verbal gestures. And even if they are subjected to some form of regulation in the shape of justifiability requirements, these latter are of a fairly indeterminate and unconceptualized sort. Instead of everyday utterances, therefore, we shall take up sentences such as “I do not know anything” as they appear to be placed in philosophical debates, in the wide sense of “debate”—that is, in discussions that intend such sentences in a more systematic and conceptualized way than that in which they are used in everyday life.

Examples of Things We Know or Can Know

Epistemologists have provided us with many good examples of the kinds of things we know or can know. The method by which they are introduced goes more or less as follows. First, a certain situation is described, and then in that situation we are to suppose an utterance of the expression “I know” (or something equivalent). Then the author of the example, sometimes after adding in some convincing details, appeals to the reader, as much as to say, “Now you see, you doubting Thomases! Repent!” But of course, if the examples are to have their proper force in a debate, the adversary must not be thus pushed or terrorized into accepting them. One particular method of persuasion, astonishingly common all the way from Plato to Austin, amounts to little more than saying something like, “Suppose I see an elephant and I say ‘I see an elephant,’ could I be wrong? Would I ever have to retract?”

The procedure is intended to convince us that there are frequent and indisputable cases of our knowing that things are what they are, or as they are. Austin (1962: 115), for example, asserts that some statements “are *in fact* incorrigible” (Austin’s italics). They are “quite certainly, definitely, and un-

retractably true" (Austin 1962: 115). And, implicitly, he takes this to amount to a proof that there are p 's such that I know that p .

Thus, "If I watch for some time an animal a few feet in front of me, in a good light, if I prod it perhaps, sniff, and take note of the noises it makes, I may say 'That's a pig.'" This statement "will be 'incorrigible', nothing could be produced that would show that I had made a mistake" (Austin 1962: 114). Here Austin will have us assume, entirely on his own authority, that he *is* watching for some time, that it *is* an animal he is watching, that the light *is* good enough for the purposes at hand, and that he *is* sniffing the animal in question. By being made to postulate the truth of so many premises, we become confused as to how the conclusion could possibly be false. But of course we should recall that an implication comprising a conjunction of a swarm of antecedents and only one consequent is among the very weakest that can be made.

It may be conceded that Austin, under most circumstances in which this could have happened, would be perfectly justified in saying what he said in the example. But it is hard to accept that "nothing could be produced that would show that I had made a mistake." Very much depends on one's acquaintances, whether they have a penchant toward practical jokes or include an amateur magician. (If we are asked about pigs with a professional magician nearby, we are wise to bet only a small amount and keep our "knowing" under strict control.) In short, even a good amateur magician could bring us into a situation such as the one Austin describes and provoke us into saying, "I know this is a pig" and soon after make us *retract*.

There is something to be said in general about examples that contain plenty of detail about what has already been done in order to ensure that p actually *is* the case. The doubter or disbeliever is, as it were, politely but authoritatively requested to accept all the details as true of the situation in which "I know that p " was uttered. He has then nothing to complain about. But it is easy to forget that if the example were true to life, the doubter should have had the opportunity to inspect the details for himself; he would not have to rely on hearsay.

Consider in this respect the many reports of ghosts, for example, which *if* the apparently true details concerning the situations are accepted, are overwhelmingly convincing. The contemporary ghost-hunter, however, has professional rules that make him for the most part withhold judgment until he has the opportunity to report about the situation himself.

Consider another example: Someone remarks in casual conversation, “As a matter of fact I live in Oxford.” According to Austin, the speaker “knows it to be true (or, if he is lying, false)” (ibid., pp. 117–18). But many people are in trouble because they *want* to be able to say truthfully “I live in such and such town (city)” —because an authority of some sort can otherwise deprive them of certain privileges—but they are uncertain. The criteria are complicated, and their satisfaction is not always clear. Incidentally, Agrippa of Nettesheim, a famous, rather unruly and restless “sceptic” in the history of modern philosophy and author of *Of the Uncertainty and Vanity of the Arts and Sciences* (*De incertitudine et vanitate de scientiarum et artium*), could probably only rarely give an account of where he lived if he could have been said to live in any definite place at all. J. Wasiutyński (1963: 130) has assured us, however, that there “can no longer be any doubt that we live in a spiral nebula.” So, is no one to worry any more about where he or she lives?

The gist of the matter seems to be this: We take “I live in Oxford” to be beyond any doubt and requiring no evidence because we place ourselves in a particular situation in which we have no *reason* or *incentive* to doubt. But the claim that I live in Oxford is not made more certain by being expressed in a situation in which the reasons and incentives to doubt its truth are reduced. To say that it was, would be to misunderstand the relation between a statement and the conditions in which it is made. The conditions are not to be seen as part of the claim and therefore cannot be taken to determine whether or not it has the property of “being certain.” A claim made in one situation that excludes reasons and incentives to doubt its truth can be made in different conditions in which such reasons and incentives once more come into play.

Consider how in casual conversation the requirement of evidence for “I know that *p*” may be so small as to make the claim to know well-nigh gratuitous; whereas in criminal cases, for example, the requirement for evidence must be adduced. The difference here is largely one in the requirements of justification and as such is strictly related to questions of responsibility that may be near zero in casual conversation and “infinite” for an eyewitness in a murder case.

A final example:

If I carefully scrutinize some patch of colour in my visual field, take careful note of it, know English well, and pay scrupulous attention to just what I’m

saying, I may say "It seems to me now as if I were seeing something pink"; and nothing whatever could be produced as showing that I had made a mistake.
(Austin 1962: 114)

That Austin, after all his care and effort, his attention to exactly what he is saying, still uses the term *seems* suggests immediately something rather suspicious here, as does the use of *as if I were*. There is, in short, ample indication that something could well happen to make us assume he had made a mistake. Or so Austin's words suggest. If, on the other hand, the situation (as he judged it) warranted perfect certainty, then what he in fact uttered is grossly misleading. Surely it would have been more correct for him to avoid the hedging forms of expression and say, "I am seeing something pink."

I shall not try to introduce anything new into the difficult contemporary discussion on certainty and knowledge within the field of immediate perception. Austin has himself suggested many sources of error and therefore of possible retractions. The crux of the matter can be put in this way: A proof or strong argument in favor of *incorrigibility*, *unretractability*, and related *-ilities* must be more than an inductively based prediction. It should somehow derive from contemplation both of the nature of the incorrigible statement and of the subject covered by such a statement. This, however, is very hard to achieve. In fact, Austin himself points out how difficult it is to delimit certain *kinds* of statements as incorrigible by virtue of their subject matter (e.g., sense data) or other characteristics.

It seems, therefore, that incorrigibility claims are essentially based on convictions that in the particular case there could not be any source of error, both in the usual sense of "source of error *worth mentioning*" and in the sense of no source of error even of the more remote kinds that we neglect in daily life. This conviction is based on a trust that one's imagination works as it should at the time of making up one's mind about the corrigibility when one is "looking" for sources of error. So incorrigibility theses are testable by future events; they are vulnerable. Simply to add to one's statement "And what I have just said is incorrigible" does not close the matter.

Let us look at the discussion on incorrigibility dialectically. Strings of striking cognitive successes of incorrigibility claims may justifiably influence the standards for incorrigibility or unretractability claims, making them less rigorous. On the other hand, series of striking cognitive malfunctions may justifiably influence standards by making them appreciably rig-

orous. What is remarkable here is the “feedback mechanism”—the inherent norms, mores, and institutions affecting changes in certain directions.

Just how far can the changes of standard bring us? In fact, there seem to be no limits; they can be lowered or increased indefinitely in the sense that, having reached a certain height or depth, there will always be the possibility of an additional increase or decrease.

A criticism of contemporary debate on (epistemological) scepticism is that it does not seem to take into account the dynamics of standard fluctuations. But fluctuations affect the application of all so-called closure-expressions, such as “conclusive evidence,” “incorrigible statement,” “indubitable utterance,” and “definite establishment of truth.”

If the fluctuations are taken to result from the operation of some kind of factors—if, in terms of methodology, they are taken to be the dependent variable—what are the independent variables? One might be inclined to say that it is our experience that affects the standards and determines the fluctuations. This suggests a kind of empiricism à la John Stuart Mill in which whatever affects us, hence also our raising and lowering of standards, is something in our experience. However, such a way of putting it would be misleading because we are ourselves in some sense interposed between the happenings and the “resulting” fluctuations. There is a factor of making, supporting, and applying rules or mores that cannot be accounted for as long as we use a model of causation, of happenings *causing* fluctuations.

However, it is not our aim here to find out why or even how fluctuations operate. It suffices to notice that if the dynamics of fluctuations are not explicitly taken into account, one and the same statement may justifiably occasion opposite estimations as regards incorrigibility, unretractability, and unquestionableness. The situation in which the statement is supposed to occur is, in such cases, placed as a member of different series of situations. If it is taken as following on a long series of cognitive failures, a verdict of “corrigible” may be adequate, but if it is taken as following an opposite series, the adequate verdict could be “incorrigible.” As long as they are not explicitly related to the series, the verdicts are mutually inconsistent. But, of course, once that is done, they are compatible.

Recent discussions concerning examples of conclusive evidence and incorrigible statements support the simple prediction that the participants aiming at undermining the examples will always find (practical, particular) sources of error (if the examples are not circular), and that the participants

aiming at saving at least one example will be able to introduce modifications so as to eliminate those (particular, practical) sources. In fact, Sextus Empiricus's maxim "Not more than" seems eminently suited to the debate: the arguments for incorrigibility are not decisively stronger than the arguments against. A general conclusion unrelated to the dynamics of standard fluctuations, to the effect that there *are* cases of conclusive evidence, seems equally unconvincing as one to the effect that there are not.

Examples of Evidence Fusing with the Evidenced

"Why on earth should one think that such verification can't ever be conclusive?" It is Austin (1962: 118) asking the question, and his theme is statements in need of verification.¹⁰

If, for instance, you tell me there's a telephone in the next room, and (feeling mistrustful) I decide to verify this . . . I can take it to pieces a bit and find out, or actually use it for ringing somebody up—and perhaps get them to ring me up too, just to make sure. (Ibid.)

The object has then "stood up to imply enough tests to establish that it really is a telephone; and it isn't just that, for everyday or practical or ordinary purposes, enough is *as good as* a telephone; what meets all these tests just *is* a telephone, no doubt about it" (ibid., p. 119). Or, to extract the principle, something's membership in a class of things is for all *practical purposes* conclusively established if results are positive when the thing is subjected to a finite battery of tests.

But, alas, do we, when trying to verify concrete examples, test the identity of a thing as to what it is for everyday, practical, or ordinary purposes? If we sometimes have other (legitimate or illegitimate) purposes, and the thing then has to be classed differently, it may turn out that those other nonpractical or unordinary or uneveryday purposes are relevant.

Let us nevertheless leave that point and consider whether a case can be closed after a finite series of tests have been performed. In the case of the telephone, I have no concrete objection; positive results from the four tests would be, I am perfectly convinced, enough for me to exclaim, "Then this *is* a telephone!" because nothing but telephones, and only telephones, satisfy the four tests as far as I can see.

But what if the question of a telephone in the next room emerged during a police search? We are still only interested in situations in which maximum requirements are made. David Krueger, one of the greatest impostors of all time, and many lesser crooks have had telephones ("real" ones?) installed that buzz at convenient intervals during crucial conferences. The voices of very important people are heard over the phone, and some impressive business is cleared up; the voices, however, are from a gramophone or tape recorder. In the spying profession, innocent instruments such as telephones are completely rebuilt in order to make less innocent instruments. They still at least look like telephones. The instruments must perhaps satisfy all of Austin's four telephone requirements or, more correctly, must be such that a suspicious person will normally come to that conclusion after a thorough search. In short, the four requirements (in part seemingly, in part really) being satisfied, the thing may still be a lot of things other than or besides a telephone.

But enough of this. The real point of the example is to show that the idea of a finite battery of tests for identification purposes brings in no radically new factors. Especially when the tests have to be performed one at a time, as is the case in the telephone story, it is always possible for new sources of error to develop with time. Thus, our ultimate view will be unaffected by this story, but we do not suggest that the discussion over (concrete) examples is likely to come to an end.

Incorrigibility and Fallibility

An essential characteristic of human fallibility is the unpredictableness of what kind of mistake will be next. If it were true that human fallibility had (a) infallibly ascertainable limits that (b) were infallibly identifiable in certain particular cases, there would be human fallibility in one sphere and human infallibility in another.¹¹ But the awkwardness of the human predicament is due precisely to the unpredictable failures. However long the series of successes and however well the field of these successes can be delimited, this trait of general human fallibility does not change.

Let us for a moment grant that such is our human situation and consider some of the consequences. Does this situation make it universally justifiable, and in fact true, to add that "I may be wrong"? If so, it will always

be justified and true when I have already said "I know that p ," to add "but I may be wrong." But this disqualifies my utterance "I know that p ," and the only thing to do will be to give up ever using "I know" and to answer, being prodded, "I do not *know* whether p is true or false" and "I do not know and cannot know anything."

Here, then, is a possibility of deriving modern scepticism from a doctrine of general fallibility. But there is weakness both in that doctrine and in the derivation of scepticism from it. If we ask "Is the doctrine of general fallibility intended to apply to itself?," a positive answer at least opens the door to allegations that human beings might, after all, be pretty infallible, which again makes the doctrine useless for deriving "but I may be wrong" as a general addition to "I know that p ." A negative answer, on the other hand, provides us with an instance of a statement that is incorrigible and true; in that case "I may be wrong" is false if this statement is inserted for p . The derivation, therefore, collapses, at least in its completely general form.

More important are the considerations adduced by Austin.¹² Human fallibility involves fallibility in keeping promises and in many other important social transactions. Why does this not prevent us, and why *should* it not prevent us, from promising? Austin stresses that there must be special reasons for suspecting a break of promise in order to make it rational to avoid promising (because of fallibility). Similarly with knowing: Only where there is something special that points to an important source of error should fallibility prevent me from using "I know." General fallibility may be generally admitted, but not fallibility in any definite concrete case. That is, general fallibility is not permitted to constitute a justification for comprehensively avoiding "I know":

A: Tell the judge you know that it was he who did it.

B: What a responsibility to take! And do I *really* know? I am but a fallible human being.

A: That holds for all of us. Go on, tell him, you coward.¹³

In all concrete cases in which there involves a social responsibility to say "I know," general fallibility cannot be invoked. The same holds for all cases in which the sentence "I do *not* know" is conceptualized¹⁴ within a framework such that it implies awareness of how to know that one does not

know, awareness of how to get adequate evidence, and of what kind of evidence is lacking. Here, too, general fallibility cannot count as an adequate argument in favor of the exclusion of knowledge.

Nevertheless, a dialogue starting in the midst of everyday concerns may reach stages at which general fallibility becomes relevant in the argument. This we will discuss later.

Corrigibility as a Requirement of Scientific Knowledge

Although a justified utterance of "I know that p " is not incompatible with the general possibility of error, it is, as we noted, inconsistent with a concomitantly uttered "I may be wrong." What then is the core of a reasonable and workable incorrigibility requirement of knowledge? It can be stated in very few words.¹⁵

One cannot say "I know it, but I may be wrong"; "I know that p , but there is a chance that p is false"; "I know that p , but some time ago I knew that not- p "; and "I know that p , but next year maybe I'll know that not- p ." That is, if I have said "I know that p ," and later I am convinced of not- p , or feel uncertain about p 's truth, I shall have to retract. I shall have to admit "I *do not* and *did not ever* know p ."

The nature of the requirement of incorrigibility can be further clarified by discussing its relation to statements made about scientific knowledge. This knowledge is said to be essentially corrigible, liable to revision, approximate, never absolutely certain, more or less uncertain, never more than probable, the best we have but imperfect, always containing errors, often partially or totally modified, and sometimes swept away during scientific revolutions. Often these things are also predicated of "human knowledge" in general. But what then of our incorrigibility requirement?

Let us first notice that these characterizations nearly always refer to scientific knowledge *in general* or large bodies of doctrines, theories, hypotheses, and so on, taken together. Very rarely do they refer to single statements, or to single definite occurrences of "I know" uttered by a scientist.

If we glance at histories of science, or at historical accounts of theories, such as atomic views about matter, we find ample reference to the knowledge of the times—including doctrines that have subsequently been revised or completely abandoned. But we discover no (or practically no) kinds

of statements inconsistent with the “prohibited” locutions listed at the head of this section or, rather, with their equivalent third-person locutions. For instance, we do not find “Descartes knew that p , but already Newton knew that not- p ”; “He knew p , whereas we now know not- p ”; and so on.

We do find, on the other hand, that certain knowledge was corrected or improved on, but still “in the abstract”—the term *knowledge* being used, not the verb, and with no recourse to particular statements. There is thus no direct contradiction between the corrigibility view of knowledge especially common in reference to science, and the incorrigibility requirement for “I know that p .” Perhaps, in view of the tendency to use the noun *knowledge* in the former case and the verb *know* in the latter, it might be wise to talk about the “incorrigibility requirement of *knowing*,” and the “corrigible and fallible nature of scientific *knowledge*.”

Only persons can know. Yet concepts of knowledge can be, and have been, construed in such a way that knowledge is impersonal. Knowledge can be stored away in libraries, whereas knowing cannot. If knowledge can be transferred to books and put on tape, the existence of knowing readers and listeners can, of course, no longer be guaranteed by the existence of knowledge.

Thus, the distinction between knowing and knowledge can be of *some* help in avoiding certain pitfalls when contemplating fallibility and incorrigibility. The distinction is, however, not clear enough or simple enough in actual use to be of decisive value. If I say “I know that p ,” it is perfectly proper and legitimate to take this to imply that I have a piece of knowledge, namely that p . But this piece of knowledge must be true, that is, partake of the incorrigibility of truth. Again, if I tend to use “I know” repeatedly during conferences or lectures or refer to large groups of statements saying “All this I know,” there will be whole bodies of claimed knowledge for which the incorrigibility requirement is normally claimed to be satisfied. Thus knowledge claims may sometimes imply an incorrigibility claim even when they refer to a whole body of doctrines or statements. Appeal to usage, then, has only limited value.

Perhaps some of the uneasiness we sometimes experience when advocating the incorrigibility requirement for “I know” stems from a faint association with a preposterous infallibility claim for scientific (or other) knowledge: the claim that the body of science, an encyclopedia of scientific knowledge, is such that no error can or will be found and corrected.

Can the Incorrigibility Requirement Ever Be Satisfied?

Accepting the idea that knowing involves an incorrigibility requirement—which can be partly expressed by saying “I know, but I may be wrong” is an improper expression—the question then arises, If I require incorrigibility for the proper use of “I know” and yet refuse to apply as standards of incorrigibility standards of evidence that socially and normally justify my saying “I know,” what do I take incorrigibility to consist of? How does one conceive an incorrigible statement as distinct from statements, say, with few and remote sources of error?

So far as I can judge, there are no criteria by which we can make the distinction, at least none that would guarantee correct results.¹⁶ Remember that standards allowing any sources of error whatsoever would have to be rejected. But, then, if incorrigibility is (practically) inconceivable in every concrete case, we have to ask ourselves whether the requirement is *unwarranted*. Since it is impossible to satisfy, surely this requirement functions no differently from any other requirement that makes “I know” useless, that is, that prevents any correct use or occurrences of “I know” whatsoever. Could we not, for example, just as well require that *p*, in “I know that *p*,” be green, square, and smell of jasmine? The effect would be the same—or perhaps better insofar as we would not lead innocent people, with little ability to see sources of error, into misusing the phrase “I know.”

The relevance and strength of these reflections can be assessed more clearly if we first ask, in a preliminary way, where we are in the dialogue, what stage we have reached. It seems that we are not here considering something that implies criticism of uses of “I know” satisfying normal, social standards. We have already concluded that all such standards sometimes break down, or at least are liable to break down. We have in fact branched off from the usual discussions in concrete cases in which mistakes are supposed to be detected and standards modified, and have entered into a study and discussion of the implications of the view that the expression “I know, but I may be wrong” is improper. In fact, we are now taking a broad view that comprehends both the experience of past mistakes and the implications of the impropriety of the expression “I know, but I may be wrong.”

The transition to this viewpoint is effected in the light of the history of supposed mistakes. This is, of course, an undertaking and a purpose *dif-*

ferent from the usual, normal, or everyday. What we are interested in may be stated in various ways, one of them thus: errors, for instance, in geometry and in the historical development of the calculus of probability, have cropped up in a completely unpredicted way; judgments proclaiming that an error has been found have in some cases been retracted; in other cases such judgments continue to be upheld. For example, in this century, a large-scale reversal of judgments has affected Stoic logic. Because it was interpreted by Karl von Prantl and others in the light of Aristotle, we now say that Stoic logic was misjudged. Consider, too, the case of Aristotelian physics. Here there has been a shift (broadly speaking) from acceptance of it as true to a vehement rejection of it as false, and then a shift toward a kind of relativism, according to which if certain modern postulates and definitions are accepted, then there are many plain falsehoods, but if Aristotle's own are used, there are less.¹⁷ More often than oscillation from true to false and from false to true, painstaking studies reveal incomparabilities. Sentence T_0 is given various interpretations T_1, T_2, \dots . One generation accepts T_0 in the sense of T_1 ; another rejects T_0 in the sense of T_2 . How the first generation would react to T_2 and the second to T_1 is not known. The sources of incomparability are as unpredictable as those of error and affect the use of "I know that p " just as heavily.

It is my impression that in the course of intense studies of reversals of judgments interculturally and down the centuries, the application of the known/not-known distinction becomes unwise. That is, the use of this distinction in daily life does not itself furnish acceptable grounds for saying in such contexts "I know" in some cases and "I do not know" in others. Besides that, the usual incentives for saying such things disappear. Let us see how this might be.

Starting with everyday-life situations, one may in a perfectly natural way be led to discuss reversals of judgment in general, and from that to reversals of judgment in systematic research. What, at this stage, would be the reasons for applying the phrases "I know" and "I do not know"?

One could begin to apply some normal standards for "I know" and "I do not know" that appear in historical narrative or elsewhere. Any specialist on human failures must himself claim to satisfy such loose standards. But what if someone questions him as a fellow student at the same stage of the debate?

Well, first of all, we note that “I know” has a performative function conjoined with the cognitive, and this function is, as we have seen, inessential, irrelevant, or even disturbing in research communication. One researcher does not *vouch* for a (research) statement that the other then makes use of on the basis of trust. In fact, the situation could easily become ridiculous if the fellow student insisted on using “I know” or “I do not know.” In short, there is less incentive here for using “I know that p ” at the stage of discussion we are now considering.

Second, even if there were an incentive, how could the conceptual framework erected during the debate be pressed to yield a basis for the use of “I know”? Such a basis is destroyed by concentration on the truth requirement, by the resulting explication of the incorrigibility requirement, and by the detection of sources of error.

Perhaps it will be objected that this latter is a “mere” psychological account of inapplicability. But whatever the suggested demerits of this, the facts still provide the basis of a rational reconstruction. It is possible to construct rules for reasonable limits of the distinction’s application under variation of definiteness of intention, or to develop framework rules, the changes of framework being in turn correlated to stages of a dialogue.

We have raised the question “Can the incorrigibility requirement ever be satisfied?,” and the answer proposed is no, but with an essential qualification. The question belongs to the stage of a dialogue at which the standard requirements for justifiably denying “I may be wrong” are out of place, that is, the stage at which the rules regulating the propriety of the opposition of “I know” and “it cannot be wrong” are no longer applicable. In their place there is an unsatisfiable requirement that there should be no sources of error. This requirement is not unsatisfiable because it is unintelligible or nonsensical or because it can be derived from some kind of logical or other necessity. The basis for stating “It cannot be satisfied,” in my own case at least, is a study of proposed examples of incorrigible statements. How this basis is to be classified methodologically, I must leave for others to decide.

There is another qualification. There are good reasons—though not, I think, decisive ones—for demanding a description of particular sources of error in criticizing a proposed incorrigible statement. In this view, the claim for a statement’s incorrigibility holds so long as no particular source of error is given. However, in many cases and especially in relation to negative

examples ("I am *not* using the tail of a scorpion as a pen in writing this sentence," and the like) the sources will be ridiculously inadequate as *particular* sources of error. Perseverance in claiming corrigibility will then probably be found in some abstract considerations about sources of error; for instance, the chasm between evidence and the evidenced or the general fallibility in human thinking. I say probably, because here it is a difficult question of motivation. So, if it is agreed that *particular, special* sources of error are to be enumerated for corrigibility to be substantiated, my conclusion would have to be that there *are* incorrigible statements but that they are of very special kinds, occurring neither in everyday life nor in science, but constructed for the very purpose of establishing the thesis that there are incorrigible statements.

It will be pointed out, however, that if incorrigibility cannot be realized, a requirement of incorrigibility will amount to a demand for the impossible. But can we rationally demand something impossible? It seems not. Then if it is irrational to demand the impossible, to require incorrigibility is a case of irrationality. It might therefore be concluded that scepticism is irrational because conclusions such as "We cannot know anything" will have at least one premise that is irrational; and because we are aiming at being rational, the conclusions will have to be rejected.

What are we to make of this argument? Well, it seems clear that it would be of value only to an opponent who does *not* require incorrigibility, who is satisfied with what he *can* attain. For one could scarcely accuse the sceptic of irrationality unless one could show that there was a rational alternative in which incorrigibility was not required. Otherwise one would surely have to concede the sceptic his argument rather than criticize it.

However, it is not entirely accurate to say that the sceptic himself is demanding the impossible. All he states is that those who insist on using "I know that *p*," and who also require incorrigibility as a requirement of the known, demand the impossible. At the same time he would contend that a clear-cut explicit renunciation of incorrigibility leads to contradictions or to intolerable divergences from the established usage of "I know that *p*," an argument that we have tried to substantiate in the previous chapter.

Arthur Pap (1949) summarizes his own and George E. Moore's antisceptical position in a few words. The *real existence* of the external world is established roughly as follows: I hold up my right hand; I see it, you see it, hence my right hand really exists; hence the external world really exists. But the

Pyrrhonist would quite happily participate in a discussion, say, about whether this or that war veteran has a right hand or not. He is not a particularly diffident or mistrustful sort of person and might even believe and trust the testimony of others without seeing the man in question. But the words *real existence* is commonly associated with technical philosophic terminologies. It is here that the Pyrrhonist's suspension of judgment enters the picture. Given professional definitions of *real existence*, *hand*, *my*, *see*, *hence*, *world*, and so on, the Pyrrhonist is one who is likely to find himself unable to find one professional opinion more convincing than another. Pap adds:

The question which the skeptic ought to be able to answer is: *what would it be like* to know for certain that the objects we perceive really exist, in other words, what do you, skeptic, *mean* by the phrase "I know for certain that *x* really exists?" That you cannot be using the phrase the way common sense uses it is evident from the fact that your disagreement with common sense is not a *factual* one, i.e. one that could in principle be settled by making further observations. No matter how many corroborating tests we may adduce as proof for the real existence of our hands, the skeptic still is not convinced. (Pap 1949: 147–48)

In his answer, the sceptic would stress that he listens to dogmatists in order to understand the statement "I know for certain that *x* really exists." Surveying the various conceptualizations offered, however, there has not as yet been any *x* such that he, the sceptic, finds it certain and true that *x* really exists. But this does not preclude the sceptic from exhibiting trust and confidence in his or anyone else's having two hands, and it will be up to the dogmatist to prove that trust and confidence imply adherence to a conceptualization concerning the real existence of an external world.

Pap attempts, he says, "to show that the proposition, doubted by the Cartesian skeptic, 'there exists an external world[,] can be known with certainty by observing the way words are commonly used.'" His argument runs as follows:

Hence only one conclusion can be drawn: the skeptic must use the phrase "Knowing for certain that *x* really exists" in some very special sense, and as long as he does not explain what that sense is, we may as well assume that the statement "we can never know for certain that any physical objects really exist and are not mere dream images" is either false or else meaningless. (Ibid., p. 148)

If a man remains unconvinced after inspecting a long list of arguments in favor of the real existence of an external world *in the sense (or senses) of*

Cartesian dualism, to my mind he deserves to be congratulated. But to conclude that the sceptic must use the phrase “knowing for certain that *x* really exists” in some very special sense is wholly gratuitous. Indeed, in elaborating Cartesian concepts of real existence and the external world, one obtains very special senses, high-degree precisizations of everyday terms, many of them transintentional in relation even to most professional epistemologists. The sceptic’s position in respect of these is that he is faced with alternative directions of precisization and he sees no convincing argument why he should choose any one of them. As for the appeal to ordinary usage, that is beside the point; here the sceptic is entitled to insist on the unwarrantedness of applying any clear-cut conceptualized distinction between knowing and not-knowing.

It seems that Pap disregards the long and difficult path from believing somebody has a right hand, and has not lost it, to asserting propositions, positive or negative, concerning the real existence of something. Otherwise he would not be so optimistic as to try to show, by noting the way in which words are commonly used, that the Cartesian dogmatist (as an opponent of the Cartesian sceptic) is correct.

In short, it seems that the sceptic’s position is in no way affected by the unsatisfiability of the incorrigibility requirement. On the other hand, it must be remembered that when we talk in terms of incorrigibility, we are applying the known/not-known distinction at a stage in the dialogue at which the conclusion that incorrigibility is impossible no longer implies criticism of uses of “I know” satisfying normal, social standards. As far as these latter issues are concerned, this whole question of whether incorrigibility is possible or not does not arise. The relation here between ordinary uses of the distinction and uses in the extended stages of the dialogue can be aptly described in terms of Charlie Dunbar Broad’s (1925: 5) definition of a “silly theory”: “By a ‘silly’ theory I mean one which may be held at the time when one is talking or writing professionally, but which only an inmate of a lunatic asylum would think of carrying into daily life.” The incorrigibility formulations and formulations like “I do not know anything,” which can be accepted as expressing tenable conclusions provided certain qualifications are also accepted, have by these very qualifications an intended field of validity that explicitly *excludes* daily life. They cannot be carried over for the simple reason that if they were, they would not be the same conclusions.

The Incorrigibility of Truth

We must include here a few words on the role of the notion of truth in creating disturbing problems about knowing. The requirement that in order for me to know that p , p must be true, creates complications because the sense of "true" here is a very demanding one. In order that I should know that p , p must be the case. The notion of a true statement implied here is well defined by Aristotle (especially if *esti* is translated as "is the case"): true statements are statements that say about that which is the case that it is the case; false are those that say about that which is not the case that it is the case.

If it is the case that p , it cannot also be the case that not- p . The exclusiveness, the narrow path of truth, is also well taken care of when, following Aristotle, we link the notion of truth to his (so-called) principle of contradiction.

In ordinary, undisturbed discourse and thinking, there is a seemingly stable insight into what is the case. Even in posing questions and doubting (about something definite), there are always some stable insights implied, some indubitable and direct access to what is the case. By requiring " p must be the case," "what is the case" (*der Sachverhalt*) becomes unrelated to our standards of evidence, our purposes of discussion, our standards of incorrigibility. Yet the notion of "what is the case" is one that is very much alive.

Attempts to avoid the notion, exchanging it for the more, as it were, epistemically self-conscious notions of "what is verified" or "statement with maximum probability," and so on, easily break down. These epistemically self-conscious concepts of knowing break down in a way similar to concepts of knowing that try to do without a separate truth requirement: we generate the possibility of both p and not- p being true, or of p remaining true at one date and at a later date not- p being found to be true, and remaining true.¹⁸

Critical Inspection of Arguments in Favor of Incorrigibility as Unattainable

At this point there is a further possible objection to face. In asserting that incorrigibility requirements arise in view of experience of past mistakes, we assume we are justified in stating that mistakes have occurred. But what justification for this is there? What justifies us in stating that the evidence

for a mistake was of a high standard? And in making these statements, what kind of announcement do we make? If we claim to know all these things on which our critique of incorrigibility rests, surely there is an inconsistency somewhere? But if that is the case, are there not serious repercussions on the argumentation in the previous sections?

Let us first see what happens if the view that incorrigibility cannot be reached is expressed in the terminology of "knowing." Suppose the view goes as follows: "I know that p " is true only if it cannot be false that p , but I know that p may be false, that p is corrigible, that there is a source of error." But the truth requirement can be applied to the latter statement too. Hence we get, "It cannot be false that I know that p may be false" and "It cannot be false that I know that p is corrigible, that there is a source of error." Now it would hardly be reasonable of the proponent of this view to accept these second-order statements as incorrigible; to do so would be to falsify the view. Besides, there seems no reason to suppose that he alone should be immune to sources of error. At the stage of the dialogue at which the first-order statements were rejected, he will also reject those of the second order. That is, he will not consider their knowledge claim warranted—or better, he will abstain also in these cases from considering whether the known/not-known distinction is applicable.

What now are the repercussions on his argument concerning the first-order statements? Does the argument collapse leaving the field to the advocates of cases of incorrigibility?

It does not collapse. The knowledge claim could be retracted (as an inadvertence), and a new mode of announcement could be adopted, any other mode of cognitive *relevance*. The mode of announcement need not be explicitly expressed. One may inform people, in a preface, that in what follows, certain convictions, beliefs, or hypotheses are not put forward as assertions. Beliefs are good enough, and in forwarding a belief there is no implied incorrigibility claim. "I believe that p , but I may be wrong" is perfectly consistent. The same holds for "I am convinced that p , but p may be wrong." It is, as was already pointed out, not necessary to prefix every contribution to the case against the attainability of incorrigible statements with an "I believe that . . ." or some other announcement different from "I know that." It might be misconstrued. The point, of course, is not to inform people that I have this or that *belief*. Therefore a preface is better.

In short, the disbeliever in incorrigible statements is not handicapped in an argument because of his disbelief. He can have no wish to pretend, nor get any benefit cognitively by pretending, that his arguments are incorrigible, and he can without loss leave the “knowing” terminology and the conceptual framework alone.

There is another objection to be faced by those who dispute examples of incorrigible statements, namely that in citing sources of error, they are speciously removing themselves from the real situation and putting themselves, or the example, into an imaginary one. For instance, in countering Austin’s telephone example, we are simply inventing sources of error; as regards to the pig, it is simply known that Austin did not have amateur magicians as acquaintances. All we are entitled to say is something like: Very well, I concede that the world happens to be such that these statements are incorrigible. But there are many nonexistent yet conceivable worlds, some of them highly exciting. In one of them the “telephone” was not a telephone and the “pig” suddenly exploded like a balloon, leaving a new smell, that of helium. If it was a pig, it was certainly a very peculiar one.

The argument that sceptics must take the world as it is can be given the following form: Given human standards of credulity and the frequency and manner of our being deceived, it does not affect the issue of scepticism that other beings might have been more easily discouraged and so become sceptical. That they might be sceptical is of no more interest than say, for physical theory, the observation that if the earth had or developed a much greater mass than it in fact has, we should not be able to walk.

This kind of attack is serious, but not altogether convincing. When the disbeliever of incorrigible statements points to a source of rather astonishing mistakes, the erratic behavior of practical jokers or amateur magicians, it is his conviction that it is just *our* world that contains the source—and James Jeans and others *may* be right in their suspicion that there are few similar worlds in all the Milky Way. The more details that a believer in an example of an incorrigible statement hands out, the more concrete the disbeliever is able to make his indications of sources of error. They may be very far-fetched, and the believer will, I presume, always be able to modify his example so as to take care of a suggested source of error $n + 1$. But the sceptic is likely to conceive of a source $n + 2$. No conclusion of the debate is foreseeable, and the field remains divided between the contesting parties.

It can also be pointed out that any insistence that the world must be taken "as it is" itself implies a partial scepticism. The rejection of both Academic and total Pyrrhonian scepticism in British philosophy is a case in point. Its scepticism consists in its disbelief or doubt in styles of life, ways of experience, and views of the self and the universe, *Weltanschauungen*, that distinctly color even everyday life. Our daily life is supposed to be something we all have in common, and in it words get their distinct meanings. It is therefore supposed to furnish a common inescapable, nontranscendable frame of reference. This in spite of poets', prophets', and philosophers' testimony to the contrary. Indeed, this testimony is taken not only to be highly suspicious but also misleading: whatever has been said to the contrary, all men live in exactly the same world, the world of common sense.¹⁹

In German philosophy, in Kantian and phenomenological trends, Pyrrhonian and Academic scepticism is also rejected, but the counterarguments are characteristically different from those of Austin and Ayer and others. Instead of an appeal to institutions of everyday life, there is an appeal to absolutely basic insights and principles that make doubt impossible *überhaupt* (in general). The principle of identity, of contradiction, the axioms of mathematics, and similar highly uneveryday themes are embarked on.

Edmund Husserl's famous refutation of scepticism (in his terminology) is worth considering even if it is directed against negative dogmatism (Academic scepticism in Sextus's terminology) rather than against the Pyrrhonist.²⁰ A theory as a piece of knowledge is, according to Husserl (1913), a kind of proposition that claims truth, is certain of its truth, and is justified in claiming truth. (One is reminded of the sets of three requirements for asserting "I know that *p*.") Theories such as "There is no truth," or "There is no knowledge" deny something they must accept in order to claim what they do. They are inconsistent, and the inconsistency can be proven.

From the point of view of the Pyrrhonist, both the negative dogmatism of "There is no knowledge" and the refutation by Husserl are open to the charge of rashness. Arguments about the necessary conditions for the possibility of a theory *überhaupt* are typical of philosophical arguments within formidable conceptual systematizations. The modern Pyrrhonist would, I think, point to the nonevident conditions for the possibility of constructing theories about necessary conditions. The development of theoretical phenomenology has exposed the intricate maze of questions surrounding

concepts of evidence functioning to establish what Husserl calls “the evident conditions of the possibility of a theory *überhaupt*.”

To those who believe in a universal everyday life or in certain basic insights, the testimony of Sextus Empiricus and others about mature sceptics with their complete suspension of judgment has to be rejected. The testimony must be *false*; the sceptic is suffering from self-deception.

Our Penultimate Conclusion on Modern Scepticism

We have examined arguments against the reasonableness of saying “I know nothing” and against the assertion that there are no incorrigible statements. It appears, however, that there are circumstances making it reasonable to use expressions like “I know nothing” and “No statement is incorrigible.” The realization that even in mathematics and logic, as well as in sense experience, statements are sometimes withdrawn and their negations asserted, sometimes even both assertion and negation withdrawn and the issue left undecided, gives the expressions a legitimate function in conveying an appreciation of the history of human error and of the reversal of truth claims. But such expressions do not function cognitively, that is, they do not serve to express conclusions for which a general validity is claimed. In fact, both for these expressions and the more radical ones of the Pyrrhonist, it is hard to find any satisfactory role in epistemology or in philosophical systems in general, if only because they are apt to be self-defeating when expressed in the precise way proper to their use in philosophical discussions.

Our conclusions are therefore extremely qualified compared with those that would have been expected to emerge if our discussion had been firmly anchored in a set of precise definitions. They can be expressed as follows: (1) “I know nothing” can be a reasonable assertion in certain circumstances, but it has to be understood in the context of the circumstances that make it reasonable. (2) There are no incorrigible statements, although it is only pertinent to attach significance to this in certain situations.

What needs to be stressed is the very special character of an endorsement of “I know nothing” or of the inapplicability of the distinction between known and not-known. The important point is this: The endorsement does not have consequences for the socially established standards of evidence, but reflects the socially accepted, indefinite fluctuability of standards. It re-

lates to special categories of dialogues in which there is created a conceptual framework of a special kind. In relation to such dialogues, and only in relation to them, the endorsement of the extremist formulation is tenable.

However, the so-called sceptical position in current discussion is mostly interpreted precisely as having consequences for the usual, that is, average standards. The so-called sceptic is a critic of those standards and denies the justifiability of *any* knowledge claim. Thus Arner (1959: 87), who has the sceptic conclude "that we never properly claim knowledge." The distinction between verification and justification (stressed by Anfinn Stigen [1961, e.g., p. 266]) can be of some help here. I justify a claim for definite purposes, facing definite persons in definite situations. In doing so I may sometimes be rash, sometimes overcautious, the weight of evidence being the same in all cases. The mores covering the transactions take cognizance of series of failures, but they also take cognizance of successes.

It would be irresponsible to spread the rumor indiscriminately, that is, to any listener in any situation, that no statements are incorrigible, that strictly speaking we know nothing, or know no empirical statement to be true.²¹ Or, on the basis of considerations in the foregoing, that standards of evidence need a general uplifting, or, that one ought to stop talking in terms of knowing or knowledge because the hazards are too great. These are highly startling and "significant" pronouncements that are certainly misleading and without foundation in the foregoing conclusions.

There remain, however, results of interest to any serious student not only of the social psychology, sociology, and other branches of the nonformal (future?) sciences of knowledge, but also to anyone interested in contemplating human efforts to reach new knowledge and to improve the quality of the knowledge they have. Some such results and hypotheses may—at the risk of repeating what has already been said—be stated as follows:

1. There is within a community no definite stable *general* standard of evidence that must be fulfilled in order to justifiably say "I know that *p*," where *p* is any statement that can be grammatically inserted.
2. There is not even a definite stable *particular* standard for any particular *p*, although there are approximations to stability. There are, therefore, no "usual" standards that most people most of the time find satisfactory in their normal working, in contrast to "special"

standards for special purposes. The fluctuations of standards are not exceptional happenings, but part of their normal working.

3. Two of the many factors influencing the level of standards for knowing are:
 - a. The stage and direction of the dialogue in which a particular statement occurs.
 - b. The responsibilities and risks incurred if it should happen (should later be agreed on) that p is false.

On the whole, as evidence is more closely inspected, as more failures of high relevance to p are reported in early stages of the dialogue, or as participants in the dialogue about p grow more divided in their opinions about p , the standards for knowing will also increase.

Further, the graver the risks and responsibilities, the higher the standards necessary to avoid being subjected to social retaliations in the case of failure. Sets of rules or theoretical models can be constructed more or less fitting the occasional explicit rules already verbalized in the community and the actual behavior of the individuals considered competent.

4. However the rules may be formed in detail, there should be one from which a prohibition of the expression "I know, but I may be wrong" can be derived. That is, it is clearly the part of the function of "I know that p " to vouch for or guarantee that p is true and not false. Therefore clear indication of a source of error may exclude the use of "I know that p ."
5. If the competent user of language and well-adjusted individual perceives a source of error at the moment of using, or at the moment of forming words in which he might possibly include an "I know," he will not withhold "I know" unless this source is of an important or probable kind—that is, only if it seems more or less likely that p will actually turn out to be false. Otherwise it is not worth mentioning. The situation for the users of "I know" is different from that of the neutral (or more or less neutral) bystander who (1) studies the community and (2) studies sources of error *independently* of the particular situation or interaction in which the utterer of "I know" happens to be placed.

The bystander will normally find many sources of error that do not occur to the user of "I know." Some of these will be such that the

user, on being made aware of them, will ignore them, taking them to be of a completely negligible order of magnitude, or of a kind that all users are aware of but implicitly agree not to consider. ("There may be a war," "The sun may not rise tomorrow," "Our historical textbook may be completely wrong," and so on.) Some will be embarrassing to the user of "I know," not because the standards of the community are such that it should have occurred to him as a source of error, and should have made him abstain from using "I know," but because he will normally take any source occurring to him as ipso facto a sufficient reason to refrain from using "I know." The bystander disturbs the delicate adjustment.

6. Nearly all cases in which an "I know that p " is well placed are such that one might very well have *not* known that p . This means that platitudes are generally not introduced by "I know that," but that, on the contrary, very many statements that to the bystander involve clear sources of error *are* thus introduced. It is therefore out of the question for him to take the actual use of "I know" as conforming or even intending to conform to a rule of incorrigibility as understood and operated by a neutral bystander.
7. If he makes the sources and frequencies of error a subject of special study, the epistemologist *continues* dialogues (of a kind rarely observed in the community) with such a tenacity and stress on explicit conceptual frameworks that he must be considered a more or less neutral bystander rather than a member of the community in these matters.

At such stages of the dialogue on a given p , or on knowing in general, it is reasonable to hold that *no statement is incorrigible*, and that the known/not-known distinction decreases toward zero in applicability with increasing stress on defining knowing within a precise conceptual framework.

8. Among fellow epistemologists, the results of the studies may well be expressed by "I do not know anything" or "Knowledge cannot be reached," but only as part of a report containing many reservations and qualifications and stating some assumptions or postulates governing the study. Announced to the epistemologically innocent user of "I know," and particularly in situations in which he is just making use of that phrase, the epistemological formulations are grossly

misleading, inevitably being interpreted in ways that are not intended.

Thus, it seems to me that the study of "I know" is not a profitless or pointless study, and that interest in it is not reduced because the results are of no or little application to the so-called everyday life of the users of "I know." After all, the everyday life is not the life we live *every* day, and the limitation to the ordinary is itself extraordinary.

My conclusion on modern scepticism can be put as follows: If by scepticism or "epistemological scepticism" is meant a doctrine expressible by "There can be nothing known," "No statement can be known to be true," or "No empirical statement can be true" without essential and severe qualifications and reservations, then scepticism is untenable. If, however, the reservations and qualifications suggested in the foregoing are made, then such scepticism is tenable. But its most adequate formulation, as I have tried to show, is in terms of the inapplicability of the known/not-known distinction in relation to any suitable explicit conceptual framework.

Our Ultimate Conclusion on Modern Scepticism

The arguments I have offered both in this and the previous chapters are designed to give support to the Pyrrhonian sceptic. The discussion as a whole is an attempt, on the part of a sympathetic metasceptic, to defend the Pyrrhonist against various undeserved objections. Some of these objections can clearly be met. In the case of others, it may be less obvious that a satisfactory answer has been provided. In some cases, perhaps even in most, it would be wrong to say the counterarguments against those who dispute the possibility, or plausibility, or sincerity, of scepticism were decisive. But, of course, the radical sceptic, too, would fail to find them decisive. Perhaps our own conclusions here should be genuinely sceptical. After all, the conclusion that there are no incorrigible statements might seem to be based on such doubtful case studies as to cast doubt on its tenability. It might be appropriate to wonder whether the requirements of evidence are so low that it might also not be possible to deny a thesis on the nonexistence of incorrigible statements.

Yet another counterargument might detract from the acceptability of our conclusions. If our study of candidates for incorrigibility is based on our

own experience of the correction of mistakes, we should note that every case of correction of a mistake is based on certain assumptions. If for p we insert " q is a mistake" in our formulas, we get statements such as "I know that q is a mistake," and the incorrigibility requirement that at one stage in the dialogue was found to be unsatisfiable is once more applicable. We should not say "I know that q is a mistake, but q may be true." If the incorrigibility requirement forces us to admit that no claim that a mistake has been known to occur can be accepted, it must be admitted that there is no *known* case of somebody having been mistaken in saying "I know that p ." More correctly, "I know that N. N. was mistaken when saying 'I know that p '" is always to be rejected as a claim to know; the incorrigibility claim involved cannot be satisfied. The same also holds for "I know I may be mistaken," "I know the incorrigibility requirement cannot be satisfied," and, of course, "I know knowledge can never be reached."

I must confess that there are arguments against my conclusions, and that my own arguments are not such as to compel acceptance, either in the reader or in their author, and certainly not in the mature sceptic. On the other hand, to be content with conclusions based on arguments so *obviously* incapable of winning the sceptic's acceptance would hardly be a convincing demonstration of the metasceptic's sympathy for his subject matter.

Notes

Chapter I: Pyrrho's Scepticism According to Sextus Empiricus

1. See Sextus Empiricus, *Outlines of Pyrrhonism*, bk. 1: secs. 12 and 26. (In all references to this work, hereafter referred to as *Outlines*, Arabic numbers refer to sections unless explicitly noted as specifying chapters.) The German translation by Eugen Pappenheim (1877: 26) of bk. 1: 12 is very good on this point: "Hochbegabte Menschen nämlich kamen, beirrt durch die Ungleichmässigkeit in den Dingen und unentschieden, welchen von ihnen sie sich mehr fügen sollten, dahin, zu suchen, was wahr sei bei den Dingen und was falsch, um in Folge der Entscheidung hierüber unbeirrt zu sein."
2. In this connection, the second to the last sentence of *Outlines* (bk. 1: 12) is important. Robert G. Bury (1933: 9) translates, "The main basic principle of the Sceptic system is that of opposing to every proposition an equal proposition. . . ." But here we should first of all reject the term *system*. Second, the term *basic principle* somewhat unhappily suggests a rule or proposition claimed to be valid. Third, an impression of deliberate policy is conveyed by talking of a principle of doing such and such. Pappenheim (1877: 26) has avoided most of these unsceptical suggestions, translating *systaseos arché* as *des Verharrens Anfang*: "Des skeptischen Verharrens Anfang (Grundprincip) aber ist hauptsächlich, dass jeder Rede eine gleiche Rede gegenübersteht."
3. [Editor's note: In discussing this revision of *Scepticism* with Naess, he was emphatic about asserting that it is no longer his view that the mature sceptic, by definition of being a mature sceptic, has peace of mind.]
4. For study of the use of *apangello* and related words, see especially chapters 23, 25, and 27 of *Outlines*, bk. 1.
5. Ten occurrences of *foné* are found in chapters 7, 8, 19, and 28 of *Outlines* (bk. 1: 14, 15, 187, 188, 191, 207).
6. If *all* utterances, then "perception" must be taken in a rather wide sense.
7. The dogmatist also "puts forward" but "with affirmation" (*Outlines*, bk. 1: 197).
8. Pappenheim (1877: 71) translates very well: "Eine Redensart, welche unseren Zustand kundthut."

9. For some important occurrences of *adoxastos*, see *Outlines* (bk. 1: 15, 23, 24; bk. 2: 13). And of *adiazforos* (bk. 1: 195, 207).
10. Nevertheless, Sextus sometimes uses the term *to opine* when referring to his own (or to sceptical) utterances (see *Outlines*, bk. 1: 4, 17). In bk. 1: 19, the reflexive “they seem to me” is used.
11. Whereas considerations of style may have perverted Hume’s argumentation in a dogmatic direction (or did he perhaps wish to convert the reader to a system?), the dictates of intellectual sincerity can sometimes pervert the style of the sceptic. In the writings of Ingemund Gullvåg, a contemporary sceptic, repeated expressions of uncertainty and perplexity leave the reader wandering in a structureless plain of if’s, presumably’s, and perhaps’s (cf. his “Scepticism and Absurdity,” 1964). One wonders how many important manuscripts by sceptics have remained unpublished because they ultimately led nowhere. Hume’s writings are eminently readable in part because of the provoking dogmatism of their style and the highly interesting general conclusions that crop up in every chapter.
12. *Outlines*, bk. 1: 7, *Fainesthai bémin*.
13. For example, *ta aisthéta* (*Outlines*, bk. 1: 9), *fantasia* and *pathos* (bk. 1: 13).
14. Hicks wrote in 1910, but as recently as 1964 there have been writers who think that scepticism essentially concerns itself with the belief in an external or material world. The *Dictionnaire de la philosophie* (Didier 1964) says some quite astonishing things: “Hegel a distingué, d’une manière très lumineuse: 1° le *scepticisme antique* (Pyrrhon, Aénésidème), qui consiste à douter de la réalité du monde extérieur et à croire néanmoins en la réalité d’un monde spirituel, et l’existence de Dieu; 2° le *scepticisme moderne* (positivisme, scientisme), qui consiste à ne croire que ses sens, à affirmer la seule réalité du monde matériel et à douter de Dieu. Le Philosophe Berkeley rentrerait dans la première catégorie, Auguste Comte dans la seconde.”
15. Chisholm (1941) writes: “In this context he seems to suggest that metaphysical statements might be true, even though not known to be true, but it is doubtful that he intended this, in view of his doctrine that indicative signs have no reference. He opposed such statements, not merely because he regarded them as nonsense, but also because he believed them to engender futile controversy which seriously interferes with that quietude or ataraxy which is the sceptic’s ultimate goal. The objection is primarily a pragmatic one” (p. 375).
16. “C’est dans le raisonnement que le scepticisme montre toute sa faiblesse. Il est clair, en effet, que du désaccord des opinions et des systèmes on ne pourra conclure légitimement à l’impossibilité, pour l’esprit humain, d’atteindre la vérité qu’à une condition, c’est que ce désaccord ne puisse s’expliquer que s’il n’y a pas de vérité ou si elle nous est inaccessible.”

Chapter II: The Psychological Possibility of Scepticism

1. Russell (1948: 191) distinguishes sceptical solipsism, the view that “there is not known to be anything beyond data” from dogmatic solipsism, the view that “there is nothing beyond data,” although according to Sextus, both views should be distinguished from scepticism. Russell’s tendency to equate scepticism with the view that knowledge of something or other does not exist or is impossible leads him to quite astonishing assertions. Thus, in a discussion of Pyrrho, he states that sceptics “of course, deny that they assert the impossibility of knowledge dogmatically but their denials are not very convincing” (1945: 234). Russell does not say why he finds the denials unconvincing. But surely, if all Sextus’s efforts to use ways of announcement compatible with his rejection of negative dogmatism, and all his efforts to show that he could not possibly take the sceptical phrases (“Nothing is known,” etc.) as true, are to be registered by the critical historian as mere “camouflage,” the most surprising results might be expected from other philosophical sources. Maybe, in his heart, Plotinus was a follower of Democritus?
2. In *Our Knowledge of the External World as a Field of Scientific Method in Philosophy* (1914), and later, in various epistemological and ontological works, Russell has tried to develop philosophically satisfactory solutions to problems that many have considered to be pseudo-problems.
3. In some circumstances, of course, it might be more appropriately thought of as tantamount to acceptance of some other proposition, for example, “This is how a confident man walks into a room.” But the view would be that some proposition or other is implied, and, perhaps, that enough information about the circumstances would determine which proposition.
4. The terminology here is that of Chisholm in, for example, *Theory of Knowledge* (1966). See especially chapter 1.
5. “Choose to go out or not to go out” might seem at first sight to be a genuine option, but it is unclear that in subsequently going out or not going out, one has necessarily chosen to do so. What if one forgets the option, or goes immediately to sleep, or is swiftly carried out before having decided? Even if one cannot (and perhaps one can) avoid the choice, it may elude one. James, however, does not give this as an example of a genuine option. Instead he gives the following example: “Either accept this truth or go without it.” But, according to our discussion above, even this would be a genuine option only when nonacceptance of the truth could be shown to exclude all ways of participating in it.
6. On the other hand, the terms do not connote something invariable. Pyrrhonism, as described by Sextus, is, after all, only one kind of sceptical philosophy according to the terminologies using *sceptical* in more or less broad senses. To

take an example, the existence of Carneades and sceptics adhering to his probabilism does not directly support the hypothesis that there also existed Pyrrhonists.

Chapter III: Scepticism and Positive Mental Health

1. Unless otherwise noted, this and all subsequent quotations in this section are from Jahoda (1958: 33–65). The book is a report written for the Joint Commission on Mental Illness and Health. Its purpose, according to the author, is “to clarify a variety of efforts to give meaning to this vague notion” of mental health. The qualification “positive” indicates that one is looking for something more than mere absence of illness.
2. By “sceptical personalities” I mean personalities with a more or less pronounced sceptical bent of mind. I am not thinking only of persons adhering to the radical scepticism of Sextus.
3. Jaspers (1946: 112 ff.) describes a case with obvious relevance for studies of states of *deep and general doubt*. It is plain from Jaspers’s classification of the sceptical frame of mind in his *Psychologie der Weltanschauungen* (1954) that his own psychopathological experience colors his perception of philosophical scepticism. Scepticism is classed as a form of nihilism and is clearly taken to be a form of deep and general doubt that is incompatible with mental health.

Chapter IV: Conceptual Complementarity of Evidence and Truth Requirements

1. Apparently Plato could only explain how such knowledge is generated by recourse to theories or myths about remembering: knowledge is not reached, but found introspectively in a way in which reaching for it is not called for.
Aristotle in no way weakens the “Platonic” requirement that knowledge should comprise an absolute relationship to truth: Quite the contrary, he explicitly and repeatedly affirms that what is known is true, and that such knowledge (*epistémé*) is incorrigible. We only know things about which we *cannot* be mistaken. If truth is claimed, a kind of incorrigibility claim must follow (cf. *Nicomachean Ethics* 1956: 15–36). To say “I know it but *it* may be false” is strange, at best. I may admit that as a human being I am always fallible, but if I say “I know that *p*,” I must claim that *p*’s truth is incorrigible, or that I cannot even make sense of attributing corrigibility or incorrigibility to truth. A belief, a working hypothesis, is corrigible, but if *p* is accepted only as a good working hypothesis, I would not say “I know that *p*.”
2. Other representative subfamilies of such truth-requiring concepts of knowledge can be introduced by variation of interpretation of the “=” in a formula $A=B$, in which *A* is defined as “He knows that *p*” and *B* is defined as “He is

sure that p , he has adequate grounds for being sure, and p is true." In this way the following kinds of sentences (among others) are constructed:

1. A means the same as B .
2. The criterion of A is B .
3. The necessary and sufficient condition of A is B .

As an example of an individual member of the family, we may take the following: "He knows that it rains" means the same as "He is sure that p , he has adequate grounds for being sure, and p is true." The expression "means the same as" can be made more precise in various directions, for instance, by introducing references to particular people and situations: " A means for P_1 in S_1 the same as B for P_2 in S_2 "—or by introducing criteria of synonymy of various degrees of strength. In what follows, it is largely (but certainly not totally) irrelevant how the schema $A=B$ is interpreted. At least three possible functions should be distinguished: (1) use of the requirements for decisions concerning the *social* or *ethical justification* for calling, or of having called, a proposition knowledge. "You said you knew the gun was unloaded; were you entitled to do so, did you have adequate grounds?"; (2) use of the requirements as a *criterion* or *reminder* when deciding whether p is known or not; (3) use in defining what is meant by " p is known." The set of three requirements may function more or less well in these ways. But these functions must be carefully distinguished. It is dangerously misleading if, for example, one applies the requirements in ways (2) and (3) to our own momentary decisions.

3. In many cases the question is closely related to such questions as "Is N. N. aware of the fact that . . ." or "Does N. N. know it or is he ignorant of it?" The questionnaire is not intended to take care of this usage.
4. Efforts to make this formulation of Ayer's more precise soon evoke a question: Does Ayer intend by "is the case" (in the phrase "knowing that something is the case") something different from "be true" (in "to know to be true")? If not, the first part of his formulation can be reformulated: "Thus necessary and sufficient conditions for knowing that something is the case are first that it is the case. . . ." The nonepistemological, purely ontological weight of "to be the case" makes the expression better suited to bringing out the difference between the first and the third requirement. The expression "to be true" has too many uses with epistemological shades. It very often resembles "to be (to have been) verified."
5. It is interesting to note that positive answers to the questions "Is he sure that p ?" and "Is p true?" may, in a very natural way, be introduced by the phrase "I am perfectly convinced that . . ." but not so the synthesis "Do you *know* that p ?"
6. Some will object that the truth requirement is apt to enter anyway. How do I *know* that a specified standard is fulfilled? If this requires such and such to be the case, truth must be reached in the matter. There is no compelling reason,

however, why standard evidence should not replace truth here as well, if it can do such a thing at all. There remains the objection that the replacement would lead to an infinite regress. We must have adequate evidence that there is adequate evidence. . . . The objection is not a serious one, however, as long as it is not shown that the regress is vicious or fatal.

7. It is crucial that (1a), (1b), (1c) are *not* taken to introduce general concepts of the terms *knows*, *knew*, and *mistaken*. In order to make use of (1a), (1b), (1c), there must by definition be references to an N, N., a *t*, and a *p*, and these references must be part of the conceptualizations.
8. This would be better considering the frequent occurrence of an extremely high degree of evidence in relation to standards nevertheless combined with disbelief. Scientists with strong intuitions have been led to important discoveries through mistrusting evidence that has more than fulfilled the requirements of the scientific community—and they have sometimes stuck to a theory that has prestige in spite of counterevidence that seems completely compelling to later generations.
9. In my article “Can knowledge be reached?” (1961 [in SWAN VIII]), I did not sufficiently stress the difference between viewing the distinction between knowing and not knowing as inapplicable or inappropriate in certain situations and denying that knowledge (in certain senses) can be reached. As a consequence of this, I considered the negative answer to the question in the title automatically to *comprise* a kind of scepticism. It is more accurate to say that it may be used to support sceptical conclusions, but only if other premises are added.
10. “To say that I know . . . is not so much to report my state of mind as to vouch for the truth of whatever it may be” (Ayer 1956: 17).
11. The concept is introduced in my *Interpretation and Preciseness* (1953 [SWAN II]).

Chapter V: Dialectics of Modern Epistemological Scepticism

1. We should recall that some of the phrases (*fonai*) that Sextus calls sceptical do not, when uttered by sceptics, express any doctrine or view whatsoever.
2. Cf. especially Russell in *Human Knowledge* (1948) and Ayer in *The Problem of Knowledge* (1956).
3. In the following, the phrase “I know” is fairly consistently adhered to instead of the long series of phrases “I know,” “He knows,” “I knew,” “He knew,” “knowledge,” “It is known,” etc. Having made up one’s mind about the functions and use of “I know,” the conclusions on the other phrases are in the main predetermined—but only in the main. If one wishes to talk about all the expressions of the series, there turns out to be very little to say. However, there are so many differences to be noted that a painstaking exposition concerned with all these phrases would be rather confusing.

4. The comparison of relative severity of requirements across different situations or topics is extremely difficult. It would seem that the attempt to make such a comparison must lead to the construction of conceptual frameworks of methodology. However interesting in themselves, these throw little light on everyday (or even scientific) evaluation and comparison of standards. The whole idea of definite standards is, of course, to some extent an artificial creation; there is little basis in the everyday use of “I know” for that fiction—however useful it is in the debate on scepticism.
5. In this example, an individual modifies his requirements. In other cases, the group or community does. The use of “I know he does not cheat” is perhaps less restricted in Middletown because “it is now years since those shocking revelations—well, you remember.”
6. This sounds as if I were to say to myself, “From now on, do not use ‘I know’ in important matters.” The adjustments and modifications in usage are, of course, normally made without articulation. Freud would call them *prebewusst* (pre-conscious); there are no barriers to their being made conscious (as with the unconscious); they simply do not occur to one.
7. It is not entirely clear to me why general fallibility in the sense of “acute sense of general fallibility” should not rationally (“psychiatrically rationally”) prevent somebody using “I know” or adequately expressing his attitude by “I know nothing.” It all depends on the way of announcing “Alas! we are all of us always fallible” and “I know nothing.” If the announcement amounts to a knowledge claim, a position Sextus calls “Academic” (cf. chapter 1, pp. 4 and 11–12) is taken. There are some very strong objections to that position, some of them formulated by Sextus himself. If something less than a knowledge claim is made, and the way of announcement is consistent with one of Sextus’s “sceptical phrases,” I see no reason to reject “I know nothing.” According to some definitions of philosophy, it would not belong to philosophy, but that should not detract from the propriety of the expression.
8. One can learn by denotation or by connotation. “Perpetuum mobile,” “eternal bliss,” and many other expressions are learned by their connotations.
9. The same lack of clear distinction between social and cognitive factors goes back to the father of British commonsense philosophy, Thomas Reid (1941: vii, viii), who insists that if anything is self-evident, then “to desire more evidence is absurd,” and that this is a valid argument against sceptics. It is perhaps absurd to say “This is self-evident, let me look for more evidence,” but not “this fulfills standard requirements justifying the pronouncement ‘this is self-evident,’ but let us look for some additional evidence beyond that which is required.”
10. Of course, in what I have been saying in the foregoing, there is no doubting about the *justification* of saying “I verified it” in very many situations. What

has been discussed among verification specialists is verifiability in highly technical (in part purely logical) senses under maximum-requirement conditions.

11. One is reminded of the possibility of constructing infallible statements ("This is pink") and fallible ones ("This is magenta"), or infallible ("This seems to me pink") and fallible ones ("This is pink"). Do we really mean to say that here is an infallible difference?
12. Austin (1961: 66) says that "being aware that you may be mistaken doesn't mean merely being aware that you are a fallible human being: it means that you have some concrete reason to suppose that you may be mistaken in this case. Just as 'but I may fail' does not mean merely 'but I am a weak human being' (in which case it would be no more exciting than adding 'D. V.'): it means that there is some concrete reason for me to suppose that I shall break my word. It is naturally *always* possible ('humanly' possible) that I may be mistaken or may break my word, but that by itself is no bar against using the expressions 'I know' and 'I promise' as we do in fact use them." Very well, but from using "I know" in a socially convenient way, it does not follow that anything is known. (Cf. our use of the terms *demon*, *devil*, *superhuman*, *eternal fame*, *never to be forgotten*, and so on.)
13. Suppose N. N. says, "Alas, I am always liable to be mistaken in whatever I say, but I know London is sometimes rather foggy," would we not interpret this "but" to refer to an exception from the liability? That is, in his use of the term *know*, N. N. excludes the possibility of being mistaken.
14. It seems that Wittgenstein would deny the necessity of a conceptual framework here and hold that all "proper" use of the expression "not know" implies awareness of how to know.
15. Ayer (1956: 25) puts it elegantly: "[I]t is inconsistent to say 'I know but I may be wrong.' But the reason why this is inconsistent is that saying 'I know' offers a guarantee which saying 'I may be wrong' withdraws." It is inconsistent, in a social sense, to give a guarantee and immediately withdraw it. Making a gift and withdrawing it are two actions related to each other in a particular way. But what is guaranteed in asserting "I know that *p*"? That it is so, that *p* is true. The wrongness referred to in the qualification "but I may be wrong" is then wrongness about *p*'s being true, that is, the falseness of *p*—that it is *not* as I say it is. This is the crux of the matter, not the inconsistency of two behaviors as such. However important it is not to neglect the performative aspect of "I know," it is the cognitive inconsistency that counts in the debate on scepticism.
16. Stock answers would be to say that if the statement is true by convention, logically true, necessarily true, analytic, or a priori, then it is incorrigible. But the conventions referred to are mostly obscure and the relation between a convention and the statements said to be true by that convention are compli-

cated and far from “self-evident.” (Do statements that are true *solely* by convention express knowledge?) Similar things hold for the criteria that a statement is necessarily true, analytic, or a priori.

17. Cf. Kuhn, *The Structure of Scientific Revolutions* (1962). The basic sceptical and relativist framework of this book has been clearly pointed to by Paul K. Feyerabend (1965: 250).
18. I may say “ p was true, but is not true any longer.” Here the truth has not been corrected. It is still true that p was true. At time t , A says, “There is a rainbow toward the East”; at time $t + 1$, B says, “You are wrong, there is no rainbow,” at which A answers, “I was right. What I said was true but is no longer true.” That is, if I repeated it now, the statement would be false. If “It is true that p ” is retracted, not the truth but the belief is said to have been wrong.
19. It would be interesting to hear the personal experiences of university philosophers on this issue. All too little has been said about the personal background from which, in part, different points of view may be *understood*. Some sort of personal understanding is perhaps a necessary condition when judging fundamental positions in philosophy. At the opening of my own commonsense and trivialism period, I remember the shock I received looking at about 150 philosophers advancing toward the food placed on a big table at a congressional banquet. There were no characteristic differences in behavior, looks, talk, or speed corresponding to disagreements in their books. Later in life, I believe I have seen so much disconformism behind conformism and behind strikingly similar ways of behaving and talking in daily life that I am convinced that styles of life and general attitudes color everything—including the kind of statement I am making now.
20. The following remarks refer to Husserl (1913: sec. 32).
21. We have not discussed various subclasses of statements (empirical, logical, mathematical, analytic, necessary, and so on) because the arguments are in the main unaffected by the differences.

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Books or articles appearing in the *Selected Works of Arne Naess* are identified by (SWAN XX) at the end of the entry, where XX refers to the pertinent volume number.

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Which World Is the Real One?

The Selected Works of Arne Naess

Harold Glasser, Series Editor
Alan Drengson, Associate Editor

I

Interpretation and Preciseness
A Contribution to the Theory of Communication

II

Scepticism
Wonder and Joy of a Wandering Seeker

III

Which World Is the Real One?
Inquiry into Comprehensive Systems, Cultures, and Philosophies

IV

The Pluralist and Possibilist Aspect of the Scientific Enterprise
Rich Descriptions, Abundant Choices, and Open Futures

V

Gandhi and Group Conflict
Explorations of Nonviolent Resistance, *Satyāgraha*

VI

Freedom, Emotion, and Self-Subsistence
The Structure of a Central Part of Spinoza's *Ethics*

VII

Communication and Argument
Elements of Applied Semantics

VIII

Common Sense, Knowledge, and Truth
Open Inquiry in a Pluralistic World
Selected Papers

IX

Reason, Democracy, and Science
Understanding Among Conflicting Worldviews
Selected Papers

X

Deep Ecology of Wisdom
Explorations in Unities of Nature and Cultures
Selected Papers



The Selected Works of Arne Naess

Which World Is the Real One?

Inquiry into Comprehensive Systems,
Cultures, and Philosophies

VOLUME III

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Author's Introduction to the Series

At ninety-two it is a great honor to be still alive and to witness the publication of my selected works in English. Few philosophers have their work published in a series, fewer still receive this honor before they die. When I was originally approached with the idea of publishing my complete works, I was overwhelmed and overjoyed, but added that not all my books and articles were important enough to merit such an honor. Selected works? Yes, and I am extremely grateful for this initiative and the final result, which presents a representative selection of my work from the earliest to the most recent. [*The Selected Works of Arne Naess* are hereafter referred to as SWAN.]

My interest in philosophy began with Spinoza's *Ethics*, which as a seventeen-year-old I was fortunate to read in Latin. I appreciated Spinoza's grand vision and trusted him implicitly as a person. I accepted that human beings could, and should, have a general outlook with the grandeur of Spinoza's, but I recognized that our individual views on this grand scale will not be identical. Through the years I have realized that there is a splendid variety of interpretations of Spinoza (SWAN VI and IX). His texts are exceptionally rich. As the years have gone on, I have focused on how he leads us to realize we can increase our freedom and sense of connection with the world through strengthening and intensifying our *positive* emotions. For example, loving and caring for our place and others leads to an expansive sense of being part of a much larger world. Emphasizing hatred and anger, on the other hand, makes us feel smaller and isolated from the world. Spinoza, as I interpret him, would express this by saying that "We are as large as our love." Increasing our freedom as human beings leads us toward life in communities colored by friendship, sharing joy and sorrow.

Before I left gymnasium [the end of secondary education] the headmaster asked me, "What do you intend to be?" My immediate answer was "A philosopher." In fact, I had already conceived of myself as one. I viewed the

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writings of many contemporary philosophers that I was familiar with, however, as vague and airy and certainly not as inspiring as Spinoza.

My doctoral thesis in philosophy of science was an effort to remind us that in science the content of a theory is not independent of research behavior—the activities of observing, confirming, disconfirming, and so on, and that these are set within a deep context of place, history, and culture. Later, as a postdoctoral researcher at the University of California at Berkeley, I studied the behavior of experimental psychologists doing animal research.

In 1934 and 1935 I studied in Vienna and while there became a member of the famous Schlick seminar, the main discussion group of the Vienna Circle. Their quest for clarity and cordial cooperation in pursuit of knowledge led me to appreciate that “What do I mean?” is an open question. I concluded that we never intend to express anything extremely definite, even in mathematics or symbolic logic. I saw the importance of using empirical methods to find out how we actually use certain expressions and sentences. I developed and applied a wide variety of such methods, which became part of the core for the empirical semantics that runs through my work. I continued to do this type of research into the 1990s, my last project being one in which I questioned experts and policy makers about their ideas of values intrinsic to the natural world (in SWAN X).

In one of my earlier studies, I reviewed about 700 articles from philosophers concerning their use of the word *truth*. For the most part, I found these unconvincing and soon started on empirical studies of the use of *truth* among ordinary nonprofessional people and schoolchildren (in SWAN VIII). Many philosophers seemed to assume that ordinary people hold very naive views about these deep matters. I found through research that, on the contrary, the views articulated by these “ordinary” people were every bit as sophisticated as those held by professional philosophers. This reinforced my conviction that, generally, we greatly underestimate ourselves. Much academic philosophy was narrowly focused and abstract. Philosophers who elicited interest in wide-ranging issues of practical and global importance, such as nonviolence and social justice, have in my lifetime said things that were considered creative, but often too far out. In spite of consistent proclamations that science neither would nor could take over all the problems discussed by philosophers, I tried to argue in ways that reminded readers of science done as open inquiry, and I tried to emphasize that it is occasionally

necessary to perform empirical research to illuminate or support a philosophical viewpoint.

My empirical and historical research led me to realize that there are no certainties and that there is a great diversity in our spontaneous experience as well as endless ways to describe and appreciate the complexities and values of the world. Thus, I realized that I am one of those lifetime seekers that the ancient Greeks called a *zetetic* (see SWAN II and VIII). From my research on scepticism and the foundations of science and logic, it became clear to me that pluralism (every event has many descriptions and possible outcomes), possibilism (anything can happen), and a healthy scepticism (always seeking the truth but never claiming it) make up the most consistent approach to respecting the perspectives and experiences of others, human and nonhuman.

From my empirical studies of semantics, and from my knowledge of several languages, I came to appreciate the complexity of communication. Being committed to Gandhian nonviolent communication, I saw the importance of avoiding dogmatism and fanaticism. One of the most important discoveries coming from this research, leading to the publication of my major book, *Interpretation and Preciseness* (SWAN I), was the insight that we cannot avoid values in any field of endeavor or research. There are no value-free inquiries or theories. Even if we refuse to express our values, this is itself an expression and choice of values. We must, therefore, be clear about our value choices and try to make them explicit. The choices we make, as Spinoza pointed out, shape the quality of our lives, and values emphasizing positive emotions or feelings are expansive and lead to our growth. We must become ever more aware of our choices and the values involved. Even pure logic assumes certain norms. Empirical research can shed light on these matters. My colleagues in philosophy often found my empirical work perplexing. I, in turn, grew to underrate the necessity of visiting great centers of philosophy, as I preferred to be close to or in the mountains.

When I visited the United States, it was mostly to climb in the mountains or walk and camp in the desert. On one fortunate visit, I dropped in at the graduate students' discussion room at Harvard. Speaking with students who were writing their doctoral theses in philosophy, I understood that my knowledge of contemporary philosophy, and of recent important contributions in its various fields, was narrowly limited to special themes of lively personal interest. Even in later years, the tendency to take personal inclina-

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tion very seriously colored my contribution to the philosophical literature. As can be seen, though, from the titles in these *Selected Works*, my strongly felt interests span a rich variety of fields, philosophical traditions, and movements.

Since childhood I have experienced an intense joy in being together with animals and plants and in contemplating the immense evolutionary development of life on earth over millions of years. From an early age I also developed an intense love for mountains and for being in them. Much of my creative philosophical work was done at Tvergastein, my mountain hut in Norway (see SWAN X). My devotion to outdoor life is in the Norwegian tradition called *friluftsliv* (literally, free-air-life). In many respects, I approached philosophical and cross-cultural studies as if I were a field ecologist or naturalist. It was against this background that my work from the 1960s onward focused with close attention on cultural diversity, biodiversity, sustainability, and the deep ecology movement.

My work since the Second World War has been increasingly within movements such as those furthering social justice, peace, and ecological responsibility. During the war, I engaged in anti-Nazi activism, and from that time also in promilitant Gandhianism, a nonviolence that is not pacifist in the usual sense but insists that if it is a bloody fight for justice against injustice, we seek "the center of the conflict" and, if necessary, cooperate with people who use arms. During the Cold War, I participated in the "third side," against both communism and extreme anticommunism, for example, as the scientific leader of a UNESCO project bringing Marxist and anti-Marxist politicians and political science researchers together in an unbiased discussion of the essence of democracy and freedom. Some of the relevant publications are included in SWAN IX.

The broad spectrum of books and articles included in the *Selected Works* represents, in many ways, a chronicle of my passions and influences. The *Selected Works* record, albeit in an inevitably fragmentary way, one possible expression of these. My dream and hope is that some readers will be inspired by their sheer variety, and that young philosophers will be encouraged to let strong personal motivations steer their studies.

Working habits vary. Some people write an article and go on to the next without looking back on the old one; others come back from time to time, radically revising and changing the old one. The latter is my way of working. Lecturing in many places about these subjects, I have found it

natural to revise the old manuscripts until sometimes very little is left of the original. Therefore, I have always viewed my writing as preliminary; a year, five years, ten years after publication of the first editions I have itched to revise, *thoroughly* revise them. When my first book was printed in 1936, I went to watch the hulking presses printing out one page at a time. I was terrified, thinking of mistakes or some awkward sentences being duplicated again and again.

When I was offered the opportunity to have a selected-works series published, I immediately thought I would like to review all my work and ask how, from today's perspective, I might answer the difficult questions I had earlier attempted to probe. Such a task would have been a particularly difficult proposition, because although many of my books and articles contain new ideas, the ideas are often not developed as well as I might have hoped. But alas, I am saved—at my age there is not time for me to accomplish such a comprehensive reevaluation of my work; I do not even have the capacity to do it now in any case.

Who could contemplate undertaking a publishing project of such ambitious proportions? Douglas Tompkins, mountaineer, entrepreneur, protector of wilderness in Chile and Argentina, and creator of the Foundation for Deep Ecology, is such a person. "Miracle Doug," as I call him, likes the idea that the deep ecology slogans and the deep ecology approach were introduced by a philosopher. I am grateful to him for his firm conviction, inspiration, and great generosity. My gratitude, however, extends well beyond my thanks to Doug, to others who have supported and championed this project.

Quincey Imhoff, when executive director of the Foundation for Deep Ecology, supported SWAN with generous grants and other contributions. SWAN has also benefited from faithful assistance and cooperation in the preparation and editing of the manuscripts. The late Professor Ingemund Gullvåg prepared the initial translation of *Which World Is the Real One?* (SWAN III). Professor Alastair Hannay translated the first edition of *Communication and Argument* (SWAN VII) and offered invaluable suggestions for improving the readability of the first editions of *Scepticism* (SWAN II), *The Pluralist and Possibilist Aspect of the Scientific Enterprise* (SWAN IV), and *Gandhi and Group Conflict* (SWAN V).

Most of all, however, I am grateful to Harold Glasser, the series editor, and his assistant, Kim Zetter, who oversaw all aspects of the project from design to production. Glasser's unique combination of intellectual tenacity,

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attention to detail, mastery of my work, and cooperative spirit made him a natural to take on the monumental task of selecting and editing my works. Glasser not only labored to improve the English and clarity of each manuscript, but his keen ability to ferret out countless technical and pedagogical errors has resulted in substantial new editions of volumes II–VII that are both far more comprehensible and accessible than the originals. I thank him for his valiant work on this project, both during his stay in Norway as a visiting Fulbright professor, where we collaborated on a strategy for revising the previously existing material, and in the subsequent years it has taken to complete the project.

From the beginning of the SWAN Project in 1994, Alan Drengson has encouraged and helped to move this work forward in numerous ways. Especially in the last crucial stages of completing volumes I, VIII, IX, and X, his help and editorial oversight have been invaluable. Thanks for his devotion, good humor, and positive enthusiasm. Thanks to both Drengson and Tim Quick for their extensive bibliographic research. Thanks to Bill Devall for his support and encouragement and especially his help on the completion of volume X, *Deep Ecology of Wisdom*. Thanks to Anne Collins for her outstanding work as the copyeditor of the SWAN volumes. Thanks to George Sessions for his support and encouragement.

Last, but certainly not least, immeasurable thanks go to my wife, Kit-Fai Naess, who has worked beside me throughout the years to provide invaluable assistance, encouragement, and inspiration.

Arne Naess

2004

Author's Preface to This Edition

An urgent issue today is whether economic globalization with a strong world market will make deep cultural differences impossible. As of now, there are still cultures that significantly differ from one another, with diverse views about nature and the world. At least to some degree, these differences can be articulated. We then get systems that are comparable in a rough and superficial way. Comparisons presuppose some common fundamental but very general ideas and procedures.

There are many worldviews, but is there only one *real* world? This question is taken up in this SWAN III volume. Twenty-five years of gradually less creative thinking have not changed my opinion in these matters. However, recent developments in philosophy have accentuated the problems in unpredictable ways. There is doubt among some anthropologists about the very concept of culture and especially about deep differences. Furthermore, doubt exists about concepts of foundations, which also touches science. "Science without foundation!" (Feyerabend). Is this merely an overreaction to extreme dogmatism—for example, the arrogant announcement that one has found the only conceivable set of ultimate premises that are absolutely valid? Such arrogance is rare, and markedly different from the single exposition of one's ultimate premises with an implicit claim that they are valid. I expect that existential philosophy of life and cosmos will flourish in the twenty-first century. No worldview will survive as THE real one.

Karl Jaspers's (1919) book *The Psychology of World Views* surveys and classifies personal worldviews. It is unique for its comprehensive and philosophical sophistication. It is sad how little influence it has had on recent philosophy-of-life reflections. Classification along his Dimension No. 2 has three subclasses. In rough English translations of the German text,

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they are: nature-mechanical worldviews, nature-historical worldviews, and nature-mythical worldviews.

It is astounding how much the richness of these worldviews has been underestimated and also how resilient they are to criticism. For example, materialistic systems are regularly said to be unable in principle to include theology and a supreme God. Yet, such systems were propagated in ancient Greece. For the atomists, God consisted of very smooth atoms communicating with us by means of extremely delicate waves emanating from those atoms. The crude waves of human communication, as well as the waves from sheer noise, usually make the waves from God inaudible. Therefore, meditation and listening in deep silence are recommended by proponents of these worldviews.

In short, and schematically, the natural, rich diversity of worldviews goes unrecognized because of mutual distrust: If a philosopher A offers a set of basic premises P_A , a second philosopher B says that from P_A something follows that everybody considers unacceptable. However, B would say that this something does *not* follow from P_B , and would add that from P_B such and such follows, which is clearly acceptable. There is a reluctance to give other creative philosophers "plenty of rope," an expression I borrow from William James but one that is also well known to mountaineers. There is a tendency to choose interpretations of the texts of others that place their philosophy in a bad light, rather than ones favoring friendly cooperation in the unending pursuit of truth.

This book (SWAN III) makes use of my empirical semantics techniques, which offer sets of basic premises expressed in vague, ambiguous sentences as points of departure, formulations that are essential at the initial stages of an inquiry. The aim is to open the mind for encounters—never complete within a world of irreducibly different worldviews. In the second edition of this book, I invited readers to combine philosophical inquiry with cultural anthropology. The aim is to help prevent the reduction of the rich cultural and subcultural diversity that is essential for the further development of humanity. The long-term radical development of *Homo sapiens* may be cultural rather than biological. This requires protection of cultural mutations against the domination of a single family of similar industrial or green societies.

Strong economic forces are pushing for a world market that allows the free flow of capital, goods, services, and labor. It is difficult to see how

deeply different cultures, some of which may be economically weak and vulnerable, can survive this onslaught. Provocative subcultures will survive, but in subcultures the continuity through generations is endangered. There is, for example, the need for proper school systems adapted to specific subcultures.

The philosophical systems of Aquinas, Spinoza, and Hegel are, in limited ways, meant to cover everything. In Indian and Chinese philosophical traditions, analogous systems have been created. In the [twentieth] century, linguistic philosophy has prevailed, especially in English-speaking countries, but grand systematizations may well see the light again. The two main arguments against the future value of *total views* are weak. The first, that they are too dogmatic, seems to be caused by the belief that the more general the view, the more likely it is that it will not be changed. However, Spinoza changed his views all through his life, and the same holds, as far as we can tell, for others. The second argument says that critical thinkers, such as Kant, have definitively shown the impossibility of reaching the goal of comprehensive metaphysical systems. However, such "proofs" are only hypothetical and have highly controversial premises.

In this book I accept the cognitive and practical value of systems but hold that the more like a total view they get, the less comparable they are, and the smaller are the possibilities for falsifying or undermining their fundamental tenets through argumentation. The change from acceptance to rejection must contain cognitive steps. In the terminology of some post-modernists, systems need "deconstruction." Others point out that all deconstructions, and their premises, may be deconstructed. The latter seems obvious to me. Worldviews of the most comprehensive kind have a great future as important cultural assets.

How can we live without taking seriously our feeling of what it means and entails to be a human being? How can we ignore the seemingly deep differences between people both in how they actually rank values and in how they act? We may ignore our more or less spontaneous, vast generalizations about life, and deeply different life conditions in different countries, or even in our own environment. We may consciously repress our vast generalizations, talking about a better life here rather than there, about the worst kind of life and the best. If asked, we may even deny that we have any

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view about life and the world. Some may laugh at the arrogance of people who profess a life- and worldview (*Lebens-und Weltanschauung*). These almost untranslatable terms were created in a country that has excelled in visionary generalizations, but where now the professional philosophers mostly reject the value and even the meaning of contributing to systematizations of life- and worldviews as all-embracing views.

I find untenable the prevalent rejection of life- and worldviews on the scale of Aquinas and Spinoza in the West, and of some great Chinese philosophers in the East. Since the 1960s I have consistently talked and written about a renaissance of great systems, that is, verbalizations of how one feels and thinks about existence, life, and the world in general. After all, some children by the age of four ask questions of a sublime and general character. In my opinion, no one has shown, through logic, philosophy of language, or any other kinds of arguments, that such grand systematic efforts are meaningless and fruitless. Should we leave these great tasks to the four-year-olds? We should all be able to have such fun!

Curiously enough, some critics see it as an imperfection that there is no consensus in questions of *Lebens-und Weltanschauung*. As long as there are deeply different cultures, and deeply different subcultures, we have reason to hope that the general outlook on life and the world will offer a great richness of fruitful differences, on par with the vast potentialities of human and natural creativity. Our spontaneous experiences and individual and cultural differences are a source of diversity that reveals the complex and deep richness of the *real* world.

Arne Naess

2004

Introduction

There is today a strange belief that science will slowly but surely solve all questions that have the possibility of a solution. With a scientific world-view as our framework, we will gradually elucidate human ends and activity, at least in all theoretically essential and practically important features. Because it is fruitless to try to anticipate the results of science, it is often argued — although unjustifiably — that it would be similarly fruitless to try to create more comprehensive syntheses than those permitted by scientific research. Such efforts may have their human uses, but they are not likely to be of any use in furthering science.

It is often said that extrapolation is unscientific and reveals a want of mental discipline. A wait-and-see attitude is more proper. Future generations will acquire a general view that we are not yet able to discern. Until then, the scientific attitude, the only intellectually honest one, dictates silence.

What happens, however, if our assumption that science will be able to generate a comprehensive world picture is an illusion (based on false conceptions about science)? What if we will have waited in vain? What if it is a delusion to believe that such a picture can emerge as a conclusion after innumerable single investigations?

Doubt with regard to the assumption that the more we perform scientific research the closer we get to a total view of the world, is not new. This doubt, however, is supported in very different ways. One standpoint argues that what purports to be a scientific world picture is an extrapolation in, scientifically speaking, unknown or insufficiently clarified dimensions. This criticism is based on a premise regarding the infinite multiplicity of types of phenomena in the world. If growing scientific knowledge is compared to the enlarged illuminated area that results from increasing the luminosity of our lamps, then the premise suggests that an ever larger area of

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darkness is also exposed. As more scientific problems are “solved,” so grows our capacity to formulate new unsolved problems.

A second standpoint stresses that science, in its basic features, is predetermined by its methods, axioms, postulates, and rules. These basic features cannot be the product of scientific research; they are presuppositions, not results, of research. The corollary is that the results of science cannot be integrated into any specific scientific world picture, but they may, perhaps, be integrated into philosophical world pictures, insofar as scientific presuppositions are subjects for philosophical research.

We are not on “the long road of approximation” (Kierkegaard) but in a historical process that causes the fragments to vary in character, to be perceived in new ways within a totality that itself varies and is not at all scientifically justified or justifiable in itself. Changes in methods, axioms, postulates, and rules are frequently, or perhaps always, at least in part motivated by results of research, but they cannot be said to be scientifically *justified*. Furthermore, the basis for making such modifications seldom seems to be self-evident, even to active researchers. Even if this were the case, we would still have the problem of *explaining* changes that were viewed as self-evident by individual scientists.

Let us return to the question of the possibility of a synthesis that is either a combination of the scientific and philosophical or is “purely” philosophical. In relation to this problem, as with others, one must distinguish between scepticism (Zeteticism, Pyrrhonism) and negativism (“Academic scepticism” in Greek terminology). The negativist denies human beings the *possibility* of working out all-embracing syntheses that can stand up to critical scrutiny. The sceptic — more precisely the Pyrrhonic zetetic — *seeks* (*zeteo*) to solve the questions that must be solved if the task is not to be declared impossible. The seeker thinks that what has been achieved so far does not measure up as an expression of plain, verified truth. There is no compelling reason to accept some relevant conclusions as more true or probable than others. A combination of two attitudes is suggested then: a seeking, sceptical attitude toward all total views, and a positive attitude toward wonder, not only as a point of departure, but also as the endpoint of philosophizing. (Beyond what I do understand, there seems, fortunately, to be much that I do not understand and shall never understand of philosophy and human cultures.)

What follows here is motivated by this kind of seeking, sceptical attitude toward what already exists in the way of syntheses. The investigations in this book are sustained by delight in the multiplicity of ideas for syntheses that are already available to us in the twentieth century. It is a pleasure to contemplate and, to a very modest degree, to reexperience generously worked out, widely differing philosophies, basic attitudes, and views of the world and man. This is the pleasure of a wanderer, or rather a vagabond. The vagabond, in this sense, does not constantly seek to compile and integrate experiences.

Following these rough guidelines, we will treat my preceding sentence, which suggested that the results of science cannot be integrated into any single world picture, as a working hypothesis, not as a thesis I claim to be true. This hypothesis is based on historical and other studies that, besides being fragmentary, rest on methods and postulates that are not self-evident and fixed but admit to variations. It would be dishonest, however, to pretend that while writing this "thesis" and elaborating its consequences I do not have *trust* in it or an implicit confidence in its truth. The comfort that the dogmatist finds in conviction and hope, the sceptic finds in trust and wonder.

There is a belief that it is possible to create compendia of the history of philosophy that would clarify the exact interrelationships between individual philosophies with regard to their specific contents. At this point I see no possibility for such a survey and feel content to stand before something that unfathomable, something that no one can classify adequately.

Faced with someone who sees a want of engagement in scepticism, I would have to draw his attention to the fact that adopting a sceptical attitude makes it easier to gain insight into more than one view of life. Engagement without insight into the unfamiliar occurs on a false basis. If one is all the time certain in one's heart where, or in what direction, Truth is to be found, insight into the unfamiliar becomes impossible. If one takes a certain presupposition as one's point of departure, one cannot gain an intimate understanding of something that has an opposite presupposition. To do so, one must uncover one's own presupposition, be willing to abandon it, identify oneself with the opposite presupposition, and see "everything" from this other point of view while at the same time maintaining a connection with the old presupposition. This involves at least a temporary relativization or suspension of

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both presuppositions. As long as one is convinced that one of them is more probable or true than the other, such a relativization is not possible. One cannot, as in the specific sciences, say, "If one presupposition is accepted, then so and so follows; if the other one is accepted, however, then such and such follows." Philosophies are too profound for such calculations; one has no immutable reference point from which to launch the if-then sentence.

Some may say that this delight in the multiplicity of alternatives must have some limits. How can one, for example, take pleasure in fascism, Nazism, Stalinism, or other extreme views of the world and man? The problem, however, is only ostensible: At the beginning of the twentieth century, attempts were indeed made to lay a philosophical foundation for fascism, but no great philosophical movement addressed such issues. No outline of a fascist philosophical system exists that is worth mentioning or that can be subjected to systematic analysis.

Scepticism, as a method of seeking, presupposes that one at least has ideas of what one is seeking. If one has the pretensions of a researcher, this implies that at the very least the ideas are sufficiently conceptualized to provide goal-standards (at least hypothetical ones) for what one seeks. A kind of standard of truth and genuineness must be retained, even if more precise formulations are held in suspense. In this sense, scepticism has at least one limit, but it is not a fixed limit. It is itself subject to wonder and search when we succeed in bringing it to awareness.

It is commonly believed that system construction must be accompanied by a blind faith in the system's truth and a strong belief in the falsity of other systems. The system creators whose ways of working are known to us, however, were constantly revising their standpoints, and one can trace their movement through many outlines and attempts. What they were sure of was a kind of intuitive, inarticulatable insight that their systems sought to express adequately. In relation to the articulated parts, these philosophers were regularly nondogmatic. Nevertheless, unfortunately, the locution *system* was and remains associated with *dogmatism*.

In what follows we shall distinguish sharply between a proposition's *degree of generality* and the *claim of certainty* with which it is asserted. One can pretend that a very specific statement (for example, about the blue anemone) is certain, and that an immensely general statement is highly uncertain (for example, about all the plants in the world). It is unreasonable to

attribute far-reaching pretensions of certainty to a philosopher simply by virtue of his expressing himself in very general terms and claiming, for example, that some of his statements are *a priori*. The *a priori* character does not relate to pretensions of certainty, but to relevant ways of verification. The great apriorist Nicolai Hartmann is particularly clear on this point.

Nevertheless, with regard to the intellectual legitimization of attempts to formulate a system, it is decisively important to consider one's standpoint on the question of whether implicit assumptions incorporating system character can be avoided at all. Perhaps the antisystematist is merely a person who does not want to, or dare to, bring his own attitudes to awareness and subject them to systematic articulation. Such an attitude cannot count on sympathy in philosophical debate.

Philosophers with deep respect or veneration for the sciences have noted how many systematizers have trudged carelessly or uncritically when considering a scientific topic (for example, Hegel in relation to Newton). These philosophers have also noted that some systematizers, out of excessive respect, exalted scientific hypotheses to all-embracing theories (for example, Spencer on Darwin). Owing to such unfortunate phenomena, there is a lamentable tendency to reject the study of systems.

The word *synthesis* is less central philosophically than *system*, but it generates more associations in the direction of this book's thrust. According to these associations, a synthesis does not have to mean that the various constituents are tightly knit and that the whole is closed. It is enough for the parts to be seen together; it is adequate that these parts or aspects of man, the universe, values and facts, or poetry and science are instructively related to each other. Inadequate synthesis would then be a matter of deficiencies if one asked the critical questions "How does this philosopher make room for . . . ?" "What is the standpoint of this philosopher to . . . ?"

One weakness of the term *synthesis* is that it deprives us of thinking of systems as originating in ways other than by bringing together things that were not united before. If one way of viewing the world and oneself is central for the system maker, the system may be said to be created by analysis of the manifestations of this way within various areas of cognition-logic, methodology, epistemology, psychology, and so on. Alternatively, the system may be created through differentiation of an unstructured whole, just as a newborn gradually learns to respond to different phenomena.

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Let us, however, return to the question of whether we can acquire an adequate overview of systems. It may be the ambitious dream of a historian of ideas to show how each philosopher's thoughts can be explained in terms of influences from other philosophers, from the milieu, from his time, and from everything else except the philosopher himself. But is a philosopher a kind of empty barrel?

Great philosophies are created by highly distinct personalities, and even the most abstract parts of their philosophical systems are expressions of living human beings and ways of seeing, feeling, and thinking. It would be a miracle if we could reexperience the world through the eyes of Plato and then Aristotle, Thomas Aquinas, Descartes, and Spinoza. As one concentrates on a particular philosopher, it simply becomes more difficult to bring out the differences between his view and others from any standpoint other than his own. Indeed, the difference seems to be that the others are wrong or do not see the whole truth. Plato did not see the world as Democritus saw it. Aristotle seemed unable to familiarize himself with the ways the pre-Socratics saw it. How could *we* possibly acquire an overview and see through the whole series? Do we have appreciably better qualifications, greater intellectual power, and more empathy than Plato and Aristotle?

We cannot expect to understand a system's sentences—even the most seemingly colorless and neutral ones—exactly as their author did. We can perhaps, to some extent, approach an authentic understanding and thereby look into worlds with essentially different structure, emotive atmosphere, and intellectual appeal from the one we habitually frequent—if, indeed, we frequent any definite world at all.

The road to an approximately authentic understanding presupposes, I think, that one turns away, *at least temporarily*, from all so-called surveys of philosophy and its history. By reading such works, one obtains a technique of acquisition and orientation that is limited to the surfaces of those systems. In learning to gloss over these, however, one remains cut off from a nuanced and deep understanding.

In Scandinavia, especially in Denmark and Norway, for more than half a century students learned a kind of survey of the history of philosophy that manifested itself, for example, in the selection of philosophers considered worth mentioning, in choosing how many “pages” were devoted to each philosopher, and in characterizing the relevant “-ism” terms. These terms are regularly taken as an indication of which characteristics are espe-

cially important among the innumerable possible ones. If it were possible to communicate in a few words and in a short time the most essential aspects of a philosophy, then even Spinoza would be blamed for having used several hundred pages to communicate what was most essential for him. In surveys of philosophy one might be able to include the most important *words* and *sentences*, but not the most important *concepts* and *views*.

In my own history of philosophy (Naess 1980), I have not wandered far from the beaten path except for a wide-reaching elimination of the vulgarizing “-isms.” The main reason for my conservatism is that I have not been able to find convincing arguments for choosing between the range of new alternatives.

In addition, it may seem difficult to oppose and renounce the grand overview because so many authors seem to master our cultural history so sovereignly. They are able to place and classify philosophers and schools of thought so that everything seems surprisingly intelligible. What is needed, however, is more abhorrence and less admiration for such things. Only when one can, without embarrassment, declare to himself and others that he has no adequate overview whatsoever, and that with few exceptions he is perplexed about what the various philosophies even involve, is there room for pleasure in the quite small approaches to deeper understanding. Grand surveys of philosophy and the history of ideas may then be tolerated simply as learned authors’ reports of whims and thoughts that they have gleaned while reading old texts and contemplating old works of art.

Indeed, lecture series or compendia of philosophy covering long periods of time are banished in some seats of learning. In others, they still have an important place. A researcher who accepts a position as a philosophy teacher at such an institution must face up to the fact that he may be required to lecture, within a limited number of hours, on a series of gigantic philosophical systems created by thinkers whose intelligence and perspicacity he is at the same time expected to bow before in awe. Furthermore, he must extract the “essentials” of the specifically philosophical contributions of the philosophies, indicate how one philosophy influences the other, and, more generally, how each fits — or does not fit — into all-encompassing, widely differing cultural epochs. For some of us who have participated in this, there is not only something unworthy but also something fascinating in such an arrangement. The manufacturing of philosophy accounts in pill form has perhaps sprung from excellent motives — the cultivation of

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our “cultural heritage,” respect and veneration for profundity and genius in generations other than our own, and even recognition that the road to truth cannot be established straightaway without inspecting different roads—but cruelty and noble motives often go together.

A survey is no hindrance if it is phrased so as to make the reader understand that he is moving on the surface and will become familiar with key words and important formulations, but not much more. Unfortunately, such representations are rare, and few readers appreciate such limitations. Most readers want a comprehensive survey of what is most essential that covers the most heterogeneous philosophies.

What, then, is so reprehensible about such a comprehensive “overview”? It is the fact that it reduces something immeasurable to something perspicuous, easy to grasp, and uniform. It favors pigeonholing, ordering, and control at the expense of imagination and empathy. What is unreasonable about the idea of an adequate overview covering all of philosophy is the implicit conception that heterogeneous, deep, and consequential thoughts can be demarcated and grasped in one and the same mind or in one and the same conceptual framework and that they can then be presented to any moderately gifted individual who makes a slight effort to grasp them. It is simply assumed that individuals can “approach” a deeper and more exact understanding of all philosophies without generating discontinuities, crises, blind leaps, or new difficulties. Kant, however, had great difficulty understanding Hume, and Berkeley found Spinoza rather unintelligible. Kant and Berkeley required, as do many of us today, a certain depth of understanding; they were not satisfied with a mere surface-level grasp. Therefore, they failed to create grand, total overviews.

Travel agencies, for example, often give advice about how to see Europe in one, two, or three weeks. One can actually see all the most famous places and works of art; nevertheless, we would say that this is a rather superficial introduction. It is hardly different in the worlds of thought. Philosophies are not suitable for sightseeing in the course of a few weeks. If one attempts to introduce someone into these worlds in this manner, one commits an injustice to the thinkers as well as to those who seek an understanding of them.

A philosophical system covers “everything,” and therefore the ego itself and the relationship between the individual and tradition. To understand two different systems, then, one must be able to perceive oneself and

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the world in two different ways. This requires significant training and much time and effort. It involves creating discontinuities in one's own person; one becomes another, and then yet another, and then returns "to oneself." It is doubtful that one returns to "oneself" as one was originally. This alone makes the possibility of obtaining an overview of *both* systems dubious. Who is the person who has the overview?

After these strong words against the pretensions of adequately surveying philosophical systems, some admissions are in order, particularly pedagogical ones. In the first encounter with the history of philosophy, it may be useful to sample widely differing philosophical terminologies in order to become stimulated to find something from which one can profit personally. Such superficial encounters can often be sufficient to give one an inkling of what kind of philosophy or which philosophers one will be able to study with great profit. Attempts to suggest main thoughts and tendencies in different philosophies in quite simple words and without any pretension of adequacy are an important pedagogical task. Understanding comes gradually, and at every stage there are specific ways of formulating one and the same topic.

I

Descriptions of Maximally Comprehensive Perspectives

Context and Overview: Some Questions

Plato claims that only a life subjected to examination and evaluation is worthy of human beings. Hence, he presupposes that we can examine, inspect, and evaluate life; that we can, as parts, grasp the whole. Starting with our childhood, we can successively enlarge our object of investigation until it covers our whole (unfinished) life as it unfolds in a world that must, in the main, be assumed to be known.

The philosopher sees things in connection with each other and seeks to articulate what he sees in the form of statements that something is thus and not otherwise. Let us, for a moment, dwell on the words and concepts in “The philosopher sees things in connection.” What does this sentence mean? Do the things comprise *everything* minus the philosopher and viewer? How comprehensive is “everything,” and what does the subtraction involve? When we begin to analyze such a sentence, problems emerge that are bad omens for the possibility of realizing comprehensive systems as well as the potential for renouncing systematic theories. The word *things* may make us focus attention on “things” in a narrow sense; for example, rigid bodies, but what we need is an exceedingly broad concept, something that also comprises awareness of things, something that comprises both π and justice, the philosopher himself and his insights and illusions.

This brings us to the next point, that the philosopher does not see much when he sees the way two things are related. Here we need the concept ‘see’, which philosophers have sought in vain to agree on. The context in which everything is seen must comprise the absence of many kinds of close connections; otherwise, one excludes the radical pluralist, as William James sometimes characterized himself, from the circle of philosophers.

One may have insight into the absence of something, and in something being an illusion. (What *is* it then and where in “the world” does it belong?)

A brief consideration of these words and concepts is likely to remind us of the superficial level on which our investigation must, in certain senses, remain. This is inevitable if we are to avoid immediate bias: as soon as we start precisization and articulation of a conceptual framework, it is difficult to stop before we find ourselves standing inside the frame of presuppositions of a quite specific kind of philosophy. Therefore, we have precluded even a superficial consideration of other philosophies. It is perhaps similar to the view of a landscape. If we lose ourselves in beholding one tarn, not only do we lose contact with the other tarns, but also the original object—“small dark tarn in light landscape”—disappears. We move even farther away from the original object if we step down into the very tarn that we were viewing from a distance!

“World picture” is a metaphor that suggests all things may be seen in connection. World pictures are mental images of the whole world and, as such, there may be many of them and they may be compared at leisure. It sounds suspiciously simple. Can we really acquire an album of world pictures?

In Karl Jaspers’s *Psychologie der Weltanschauungen* (Psychology of world-views, 1919), he says that by a “world picture” he means “that which is objectual for man.” Jaspers’s term *objectual* (*gegenständlich*) is useful inasmuch as it alludes to an object concept that is much wider than the ordinary thing concept. However, it emerges from his own characterization of world pictures that this descriptive definition is inadequate. What Jaspers writes about are the kinds of syntheses that human beings form of the objectual (for example, a mythological cosmology and anthropology). Jaspers’s formulation would cover his actual use of the expression more adequately if he defined “world picture” as “picture of what is objectual for man,” or if he defined “the world picture of a human being” as “the picture of what is objectual for this human being.” The relativity—or rather the relationality—emerges clearly in the last formulation. The following problems arise: How can a human being perceive that others also have world pictures and that these may differ from his own? Furthermore, if a human being can perceive the world pictures of others, do these then become parts of his own? (If the world pictures of others are something objectual for me, they must, according to Jaspers’s modified definition, be parts of my world picture.)

In spite of his, in a certain respect, very wide concept of world pictures, Jaspers does not permit worldviews to be determined by the world pictures alone. He characterizes worldview as a combination or synthesis of world picture, attitude, and spiritual type (*Geistestypus*).¹ Thus, there is something outside a conception that nevertheless determines or characterizes it. There is, at least in part, a psychological element, a specifiable attitude, that produces the particular view of the objectual. Even if the presence of this element raises purely practical problems for the understanding and mutual comparison of views—something we shall touch on later—this in itself does not create the logical problems we shall discuss. Those arise from the very attempt to formulate total views—to treat everything as objectual.

A connection that one finds or postulates may be loose or tight; the specific parts of a context may be more or less autonomous and independent from one another. The connection that one speaks about may be thought to apply to a number of things that are of interest at the moment (for example, prices or demand and supply as regards the paintings of Edvard Munch, and so on). Then one does not pretend to have said anything at all about other things and their connection or lack of it. On the other hand, the context spoken about may conceivably cover the whole life of a human being, or life in general. We encounter difficulties when we try to say something about everything, as well as when we try to say something about a thing that has been abstracted from its context.

Before a life can be subjected to evaluation, it has to be examined as a whole. Does that whole, however, involve self-evaluation? At least not the same whole, one might say.

Evaluation of life is different from life itself. We are concerned about two different wholes. There is nothing paradoxical here. All our everyday reasoning and action give the impression of wholeness, even if they are not manifested as an explicit and consistent total world picture. Such an underlying unity must be presupposed if actions and arguments are not to seem senseless and pointless. This connection with other mutually confirming arguments, beliefs, and attitudes is present even if a person is entirely ignorant of the underlying unity and is perhaps unable to verbalize the complicated network of interdependent parts. This merely means that everyday human activity reflects a frame; hence, it may be regarded as a whole.

There is, however, something strange and paradoxical about the non-mundane philosophical activity of constructing systems that distinguishes

it from the more or less structured activities in everyday reasoning and action. Philosophical systems seem to be attempts to consider and articulate everything (activities, life, and the evaluation process itself). How can the unity that is basic to the activity of a system builder be considered, and made explicit, within the world picture that is the result of his own activity? Like any activity, especially any reasoning activity, system building requires certain rules. The system creator assumes a framework. Certain presuppositions are accepted, even if only implicitly. It is difficult to imagine how the system, which is indeed the product of these rules, incorporates independent presuppositions.

If we are interested in the study of foundations rather than superstructures, and the presuppositions rather than the conclusions of a dogmatic system, then our interest will involuntarily turn away from the explicit toward the implicit. If, for example, we examine an argument we have employed to justify that we know something and find that it does not satisfy the demands we had posed for justifying knowledge, then we discover how lenient our implicit criteria of knowledge were. When we become aware of our oversight, we may then investigate the stricter requirements that made us condemn our earlier argumentation as careless. Perhaps these requirements are also not strict enough? Perhaps they are too strict? Perhaps our initial procedure was not so bad after all? It is not impossible that a transition to a wider or larger frame may make us sceptical and uncertain regarding our previous sceptical worries concerning our original irresponsibility.

This shift of our attention from frame *A* to another frame *B* that also contains *A*, takes place within a new implicit frame where one may imagine making both frames *A* and *B* explicit. As we retreat in the chains of presuppositions, we immediately and unavoidably move from one frame to the next in a regressive series. Attention may then turn to frames of reference and thereby to the foundation of our whole way of posing the problem. The following questions arise: Why not get rid of the frame as well as the problems? Do we really have, and do we employ, an implicit frame of reference? How did this idea get involved in our considerations? What presuppositions did we use at this point?

Such reflection on frames of reference exemplifies how we can clarify our own presuppositions when we try to become clear about those of others. This thinking is itself a legitimate subject, and since this way of thinking is only an assumption about the nature of our methodological convictions,

then perhaps we can do without it, even if we cannot do without the methodology. It does not seem likely, however, that abandoning this terminology will lead us any closer to answering the questions that necessitated it.

We seem to be caught in a trap, unable to free ourselves and unable to start anew. We can examine all the beliefs we had until the moment of examination, but we never reach the critical examination itself. It is tempting here to quote Kierkegaard's wonderful rhetorical reflection in his main philosophical work *Concluding Unscientific Postscript* (1941) about "the System," but here are the essentials: The System begins with the immediate, and is therefore without any presuppositions whatsoever. The beginning is therefore an absolute beginning. But does it start immediately with the immediate? Kierkegaard finds that impossible. So what does the System really start with? Kierkegaard suggests that it starts with the reflection, existential reflection. And then he asks: How could one end the reflection and start with the System?

We mentioned how the all-encompassing character we try to give a frame is made explicit by a collection of basic beliefs or assumptions. The inescapable retreat into frames of higher order and the succeeding infinite regressions suggest that it is impossible in principle to formulate all basic principles of this all-encompassing kind. The whole eludes what we grasp and formulate in discursive thought. Just as we cannot inflate a balloon from the inside, we cannot examine a set of methodological beliefs without employing another set of beliefs. In order to examine one belief, we must do something analogous to blowing new air in from the outside, meaning that we must introduce or employ principles other than the ones we examine at the moment. In the regressive process of examining frames of reference, something eternally eludes us. (Of course this applies to the latter claim as well because it can also be subjected to critical examination. Parts of the frame of reference can be clarified, and implicit assumptions can be made explicit.)

The notion of fundamentality is itself a relational concept. We use our existing fundamental views to probe the set of views that are implicit in our first-order investigation, to build on the explicit elements of our first-order views to examine second-order views, and so on—*B* is basic or fundamental in relation to *A*, and *C* is fundamental in relation to *B*. If fundamentality is a relational concept, however, then the quest for a natural point of departure is no more realistic than a journey to reach the horizon.

The explicit total views that we find in the history of philosophy are fraught with paradoxes. Either a view is explicit but fragmentary, or it is total but implicit. If one considers the usage of the word *view*, one may arrive at a similar conclusion. A view is something from a place, and this place is not part of what is seen. One cannot have a total view in the sense of a view that comprises the view as well as the point from which it is seen. Perhaps it is only after philosophers and others attempt to work out gigantic systems that we ask, What makes system builders with "totalitarian" aspirations believe in the possibility of reaching their goals? It is only after considering what seem to be inescapable paradoxes or contradictions that we can begin to talk about conceiving the sort of preconscious views or inclinations that we must have before making philosophical investigations.

At this stage we must ask three main questions: (1) What kind of totality should we ascribe to a total system that cannot be completely explicit? (2) How can we explain the great philosophers' belief in the possibility of total views? (3) What are the consequences of the unavoidable incompleteness of any explicit total view for the representation, survey, and comparison of philosophical systems?

In what follows we focus on the last of these three questions. First, however, let us consider briefly how we might answer the first two. A possible response to the first question has been suggested. Perhaps we should assume, as a counterpart to the explicit system elaborated by a philosopher, the existence of a basic preconscious view or orientation—a kind of matrix that is the basis of all attempts at investigating and explicating the concepts and categories in any implicit frame of reference. Since such a "view" could not itself be made explicit, it would not be a part of what could be investigated in this manner. This conception of the "totality" of a view would at least prevent the type of leakage that seems to result in any system in which an explicit methodology presupposes still another methodology, which then has to be made explicit, and so on. For if the explicit view arises from a preconscious matrix, then what it excludes is not something that it ought to include. This might tempt us to say that whatever can be clarified by means of concepts and categories in a certain way is an expression of the preconscious view that cannot itself be clarified in this manner. It then becomes questionable, however, whether we should call it a preconscious view at all. The word *view* suggests something that can be maintained and examined, which again implies the possibility of making it explicit. If the pre-

conscious is not regarded as a view, it would perhaps have to be considered as the cause of the explicit system, as its psychological foundation. We shall not pursue these questions further.

What about the second question regarding how belief in the possibility of explicit total views has arisen? This question may be phrased more precisely: How can it be explained, from the point of view of psychology or social science, that man has begun to speak or come to believe that he has spoken, or can speak intelligibly, about his total view, his logic, ontology, epistemology, and value system? How have we arrived at the possibility of contemplating our own total view as one among other views that can be made explicit?

Perhaps the belief has developed this way: We believe we notice that a particular human being always thinks and believes in a certain way (of the many possible ways we can imagine). The set of possibilities is implicitly fixed by our own—the observer's—frame of reference. To express as clearly as possible what distinguishes our view from that of others, we seek not only to make their views explicit, but also to make our own explicit. We tackle the latter as if it were identical to the former—something that already lies within our own frame. We are not fitted into our own frame, however, and this makes the second task completely different from the first. To perform it seems to me to be just as difficult as eating not just a part of oneself, but the whole. The analogy that makes one believe in the possibility of such an explication is fortuitous. A view that comprises other views does so by fastening the different views that it comprises to something else, the adjusting view. However, this latter view in its turn can be explained only by being adjusted in the same way, by being fastened to another something else.

Since the considerations about another person's views must be conceptualized within our own frame, our own views will also be in that position. Because we are primarily interested in contrasts, the part is mistaken for the whole (i.e., one's own fundamental conceptual framework tends to be overlooked). The same applies to another possible way in which this belief can arise, namely by looking back at earlier parts of one's life and attempting to investigate them. As Arthur Koestler reminisces, "My Party education had equipped my mind with such elaborate shock-absorbing buffers and elastic defenses that everything seen and heard became automatically transformed to fit the preconceived pattern" (Koestler 1949: 60).² One need

only take a small step away from the belief in an all-encompassing knowledge of one's own mind as it was at an earlier stage of development to imagine that one has at that moment a definite all-embracing view, which can be made verbally explicit as one view among others.

Within psychology, as in other sciences of humans, there have been conceptions of all-embracing views, and it has apparently been taken for granted that they were to be used in research in order to produce satisfactory neutral classifications of individuals or groups. Let me quote from one who has a strong belief in all-encompassing outlooks:

Our revised, more dynamic and concrete conception of an ideology may now be defined as the complete system of cognitive assumptions and affective identifications which manifest themselves in, or underlie, the thought, speech, aims, interests, ideals, ethical standards, actions—in short the behaviour—of an individual human being. (Walsby 1947: 145)

Walsby's belief in an underlying ideology has striking similarities to the belief in a God who manifests himself in everything that happens in the world. There seems to be a strong Hegelian tendency in ideology research, influenced by Karl Mannheim's comprehensive concepts. In his *Ideology and Utopia*, Mannheim writes, "Here we refer to the ideology of an age or a concrete historico-social group, e.g. of a class, when we are concerned with the characteristics and composition of the total structure of the mind of this epoch or of this group" (1936: 49 f.).

The ideology of a person that Walsby might wish to observe would have to be described and classified in relation to a frame so all-encompassing that it completely comprised the observed person's frame. The ideology of an age or a particular historical social group, such as Mannheim observed it, must at any point be surpassed by—be contained in—the ideology of Mannheim's group or age. The all-encompassing structure of Professor Mannheim's own mind must, in the same way as a divine intellect, provide a frame of reference and a conceptual structure of the most all-inclusive or value-neutral type. Concepts such as these are mighty factors in the construction of the images of fascists, communists, and others who cannot be reached via ordinary lines of communication because their basic views differ from those of the observer. One cannot discuss with them, but they understand the language of power.

Walsby presupposes, on the one hand, that the people he observes are developed comprehensively (so that their ideology can be derived) and, on the other hand, that their views can be understood by a nonspecialist. What is the outcome if by mishap one catches a logician in the net, for example, Professor Benson Mates? Before rejecting him as nonrepresentative, one would have to consider what he says about philosophical opinions—indeed, they are parts of the ideologies. Mates (1968) asks if, within a system that is approximately all-embracing, problems can arise that threaten its foundations. Unsolved empirical problems cannot threaten the foundations since they have an open character. If one constantly finds the opposite of what one expects (empirically), there is room for any conceivable change in the empirical hypotheses. Logical problems, it seems, may cause fundamental crises.

According to the logician and epistemologist Mates, we may consider such a crisis to be present today. This suggests that we should look for new foundations—a new set of basic logical concepts. Mates takes the logical antinomies as his point of departure. By “antinomy” he means an argument that has a logically false conclusion, yet seems to be valid, and has only analytical truths as premises. Examples are the liar³ and Russell’s antinomy.⁴ Mates does not expect that any intuitively evident solution will ever be found. He believes that, after 2,400 years during which philosophers have examined the liar, the proposed “cures” are still worse than the “illness.” The main difficulty is not a lack of powerful arguments; on the contrary, it is the existence of excellent, logically watertight arguments both pro and contra.

Mates suspects that a series of philosophical problems—free will, the existence of an external world, and so on—are such that the more precisely they are posed, the closer we come to ending up with contradictions that are equally well justified. The more precisely the problems are posed, the more apparent their antinomical character becomes. The conclusion is that if we put them together in suitable groups, some of our most basic concepts are radically defective.

It is difficult to say what we can do in such a situation. Mates thinks that the only cure is to “forget it all,” in the spirit of Hume. Imre Herman, Lucien Lévy-Bruhl, Mannheim, Walsby, and many others have no doubt that the fundamental convictions and attitudes of others—for example,

their logic — can always be described and compared.⁵ In cases in which the all-embracing description is directed at so-called primitives, the observer is seldom exposed to a reversal of roles; the primitive merely ignores the scientist's total view. If, however, the social scientist is confronted with critical or enraged adherents of systems that are not verbally primitive, then he will become conscious of some of his own assumptions, and he will speak about, or believe that he can speak about, his own basic frame of reference. In the course of rationalizing his implicit assumption that he can discover and satisfactorily describe the total views of others, he can be led to believe that he also has a total view that is capable of being verbalized. That which he more or less uncritically ascribes to others, he now feels compelled to ascribe to himself. He insists that he has a total view and is willing to verbalize it, using terms such as *the world, man, freedom, progress*, and so on.

It is tempting, even if the method annuls itself, to apply Heidegger's system when one seeks to characterize the attitude of philosophers and scientists who unhesitatingly and unreservedly give all-embracing descriptions of ideologies, total views, and philosophies, primitive or not primitive. Heidegger writes about the way of being when "das Man" has the upper hand, and one is generally comfortable with it in its very degeneracy (*Verfallenheit*):

Prattle (*Gerede*) and ambiguity, the having-seen-all and having-understood-all — builds up the assumption that the openness of *Dasein* accessible in this way can guarantee *Dasein* that all the possibilities of its being are secured, genuine and unfolded. The self-assurance and resoluteness of *das Man* spreads a growing frugality as regards actual "personally" appropriated understanding.
(Heidegger 1927: 177)

Formulation of Systems

System Concepts

The term *system* will be used somewhat loosely to describe sets of propositions, rules, or postulates that are in part explicit and in part implicit and that purport to cover a large and essential part of all possible kinds of objects of thought and imagination. The system may be said to be total when it is intended to cover all such parts. Something can be represented by a system, although not everything that can be said about this "something" (the

object) is said. Plato's theory of forms covers all forms without saying something about each one. Newton's theory of matter covers all material particles without saying anything about a single one of them.

The representation of "system" is intended to ensure that what is traditionally characterized as a system in the history of philosophy can be subsumed under this designation relatively unproblematically. Of a person, group, or nation, we sometimes say that she or it has a general or total orientation toward existence. We contrast such general orientations with attitudes toward a limited phenomenon or a limited aspect of reality. We speak of views of existence as a whole as total views or total systems. For professional reasons, the word *system* is preferred over view.

Since Aristotle, we recognize attempts to verbalize general orientations in the form of long series of formulations that seek to cover all subjects. These general orientations, however, do not appear in the form of detailed knowledge: one seeks to express only the vital or essential aspects of categories of thought. One has not restricted access to new material, but one has predetermined its categories. If something occurs that defies the divisions, the system bursts. If, however, the system is "sufficiently total," such a thing cannot occur. We will return to this point later.

Philosophical vocabulary includes words that, besides having other uses, characterize general orientations: rationalism, pragmatism, materialism, Spinozism, Hegelianism, and others. These catchwords are useful for our deliberations as long as we remember that they are adapted to the *history of ideas*, not the *systematization of ideas*.

Some systems are explicitly presented as systems (Aristotle, Hobbes, Spinoza); others are not. Among the latter, there may be explicit rejections of philosophical system construction (Sextus, Hume, Kant, Wittgenstein). It would lead to unfruitful distinctions to exclude total orientations to existence, verbalized but "ant systematic" thoughts about all essential subjects, just because they also contain direct antisystematic statements. If system *A* answers 1,000 questions and system *B* consists of arguments connected to each of these questions with the conclusion "pseudo-question," *B* has indeed a very negative character, but this is not sufficient reason to deny that it is a system.

It is another matter with Sextus Empiricus and Pyrrhonism as he represents it. Pyrrhonism contains no assertions at all. The possibility of knowledge is not denied. The Academic sceptics, on the other hand, deny the possi-

bility of knowledge. Therefore, they are involved in arguments committing them to acceptance of propositions, and we may speak about an Academic-sceptical system (provided the possibility of complete consistency is accepted). As a questioner, in relation to others and himself, the Pyrrhonic philosopher participates in discussion, but not as one who asserts anything. There is a Pyrrhonic philosopher but no Pyrrhonic system.⁶

From Socrates' time to our own century's Rudolf Carnap, Ludwig Wittgenstein, and G. E. Moore, antisystematic, but nevertheless approximately total, verbalized orientations have existed. In what follows, the primary focus will be on the more or less explicit system constructions.

To understand philosophical systems, we must keep the pretension of totality (*Ganzheitsanspruch*) clearly in mind. The philosopher's struggle to gain an entirely sublime perspective, his striving to avoid the fact that something hides behind his back and may thereby remain invisible, must necessarily put its stamp on much of his work. Thus, abstract ontological reflections may acquire a weight that must seem incomprehensible to anyone except the totality-seeker himself. Bertrand Russell's monograph about Leibniz (1937) illuminates the importance of seeking out precisely the decisive problems of the system creator.

In the preceding, the word *orientation* is often preferred to *belief*. As I construe it, an orientation also comprises one's attitudes toward what one believes. Furthermore, an orientation can encompass beliefs. Hence, the word *orientation* in the preceding is meant to express a more general concept than 'belief', and to emphasize something active that involves both subject and object.

In his doctrine of philosophical systems, Everett W. Hall (1960: 1) assumes a rather wide concept. By "philosophical system" he means something like a "set of categories." This is hardly a satisfactory point of departure. He uses "category" in a sense close to the *Oxford English Dictionary's* second, extremely wide definition: "a class, or division, in any general scheme of classification" (Hall 1960: 3). Actually, he has no use for such an extremely wide concept. As an example of a system, he mentions the Scholastics' "transcendentals": *ens* (entity), *unum* (unity), *res* (thing), *aliquid* (something), *verum* (truth), *bonum* (good). In our terminology, we would say that these are categorial basic concepts within a class of possible systems. If we connect the concepts and define them in various ways, they

may become limited parts of the conceptual structure in systems defined as on page 10. Differences might arise, for example, by using different definitions of *true*, as these are developed within Scholasticism.

Categories in Hall's sense are not propositions; their adequate verbal expressions are terms, not statements. According to Hall, a system does not need to be expressed as a set of contentions. Such a rule is appropriate if one inserts an "only": an adequate articulation of a philosophical system cannot consist *only* of contentions or propositions. On the other hand, our concept 'philosophical system' is impossible to identify only by using categories. Certain Scholastic systems can, as a first approximation, be articulated by constructing a set of descriptive and normative sentences in which the expressions *ens*, *unum*, *res*, *aliquid*, *verum*, and *bonum* occupy a characteristic position. A group or family of such systems can be characterized by the fact that they are based upon, or make a central use of, these categories. No single system, however, can be exhaustively characterized by them.

For reasons that easily follow from the preceding, we shall therefore not seek to characterize a system definitionally by a set of concepts, a conceptual structure, or a conceptual frame of reference. The requirement that systems must not contain contradictions leads to interesting research problems concerning disciplines that are said by experts within the discipline to involve conflicting presuppositions. Let us consider an example. Many lawyers and philosophers of law have struggled with problems of free will.

One can indeed say that in our conception of law and our evaluations of it, we operate with mutually incompatible presuppositions. In our demand that the law must be just, we presuppose a certain freedom of will. In our demand that the law be efficient we assume that the law, as a causal factor, can create motives and thereby act as a determinant of human conduct. (Castberg 1966: 65)

Experts on "law and our evaluations of it" acquiesce in what they at least suppose is a contradiction, as long as the contradiction cannot be formulated within their field, that is, within the subject "law and our evaluations of it." When the contradiction lies in the *presuppositions* of what is said within the field, scruples of professional ethics do not arise. It is left to others to alleviate the contradiction, for example, to philosophers who specialize in free-will problems. The elimination of the contradiction, if it exists, may be sought by confronting the philosophy of law with a series of philo-

sophical theories and precizations of sentences such as “*x* acts freely,” “*x* is determined by *y*,” and so on. Only those syntheses that yield a consistent set of presuppositions can be integrated in a total system.

According to the indicated use of the term *system*, a system is total if it covers all partial systems. It is here taken for granted that a kind of basic distribution of subjects is possible. If this were not the case, one could not place systems in relation to each other so that they appeared to be possible parts of a whole. A circularity cannot be avoided here: a total system can be constituted only by integrating partial systems, but constitution of a partial system qua partial system can occur only in relation to the idea of a total system. To avoid the specific difficulties involved in the notion of an absolute system, it is appropriate to focus on the movement from less total to more total, hence on the phase of integration in which a system is expanded successively. When the integration reaches a level at which the system begins, in a relatively vague manner, to cover the traditional disciplines of philosophy, most problems of interest to those other than specialists in logic also emerge clearly. Specifically, logical problems do arise (for example, in connection with self-reference: that the system must be applicable to itself), but these problems arise only with attempts at maximal precision and depth of intention.

Following our proposed use of the word *system*, a system is on the level of connotation, not denotation (reference)—the B-level in the Semiotic Triangle. It is something that is expressed by a specific set of texts. This implies that different sets of texts can express the same system. It further implies that in the absence of direct knowledge of the B-level, the claim that a text expresses a system is a hypothesis of a very special type. Recognizing the need to choose a mode of conception and definition within a total system brings us to ask, Can we find a neutral semantics?

We have already begun to choose a semantic metasystem for the study of the semantic characteristics of systems by virtue of characterizing systems as things that exist on the level of connotation (the B-level) and not on the level of formulation (the A-level). At this point, we have probably already lost our neutrality. Some systems may contain semantic part-systems incompatible with our own, and this may endanger our description of the system.

On the basis of the selected use of the term *system*, it is easy to give a preliminary indication of what must be counted as different systems. As soon as the depth of intention increases and we transcend the level of every-

day parlance (the " T_0 -level"), it turns out to be very difficult to define criteria to describe the difference.

If two sets of propositions, rules, or postulates are presented and at least one proposition, rule, or postulate is different, then the sets are different. If the two sets constitute systems, then the systems are different.

In practice, texts and authorship play a decisive role in our identification of systems. If two different philosophers write two different texts, and we believe the texts express systems, we take it as a matter of course that the systems are different. We tend to treat systems as if they belong on the level of signs or texts (the A-level). To consistently identify philosophical systems with texts, however, leads to even greater difficulties than if we try to restrict them to the B-level. I mention this because a problem is encountered immediately when one attempts to compare sets of propositions, rules, or postulates. By putting "and" or "full stop" between the various propositions, rules, and postulates, we can significantly vary the number of sentences used to characterize a system (on the A-level). The use of "sets of propositions" depends, strictly speaking, on the dubious presupposition that one can in fact indicate all the necessary propositions with a specific number of sentences. If one includes implicit propositions in the set, the situation becomes even more difficult. Implicit propositions are those representatives of the system that may never have been articulated but that would likely be revealed when the system is reviewed in an exhaustive way and contrasted to other systems. Here the indication of particular numbers of propositions must seem highly dubious. For comfort, however, one may limit oneself to defining sets by relations relative to other sets (inclusion, exclusion, emptiness, and so on), and thereby avoid having to specify the number of elements in a set.

Jaspers's Typology of World Pictures

It is not within the scope of this work to give a classification of systems. Nevertheless, Jaspers's attempt to create a typology of world pictures on psychological grounds is so instructive for our questions that we find it important to consider some aspects of it.⁷

Under the term *world picture*, Jaspers includes far more than the products of professional philosophy. The latter, however, are special formulations of something that has a universal basis. Presumably, if one aims at a

typology, the study of the most refined philosophical culture products ought to take the “more general” for its point of departure. It is well worth the trouble to highlight Jaspers’s typology.

First Main Class: Total Pictures of the Sensible in Space

Jaspers’s types according to Division Fundament No. 1 are:

1. *Immediate World Pictures.* With regard to a person’s immediate world picture at a given time, Jaspers seems to include whatever this person experiences insofar as it is experienced as something outside the person herself, in space. Jaspers gives examples: colors, tones, smells, horizons, and so on. By the word *world*, he presumably suggests a kind of synthesis or combination of these constituents.
2. *Pictures of a Limited Cosmos “Behind” the Immediate.* There are also conceptions of a “world” behind that which is immediately experienced. Such a world cannot be perceived but is perceivable in principle. Every human being creates fragments of a geographic-cosmic world picture. The first systematic pictures are of a limited and finite cosmos.
3. *Spatio-Temporal Infinity.* Giordano Bruno gave us pictures of infinitely many worlds in an infinite space. Jaspers’s acceptance of the infinity of the spatial world did not yet, in 1919, take seriously Einstein’s distinction between infinity and unlimitedness. Today we know, perhaps, that the world is finite but unlimited.

Already by this division, Jaspers has ventured a solution to immense problems. The three types of total pictures indicated here by Jaspers are, strictly speaking, not kinds of actually occurring world pictures, but sides or aspects of something, *x*, that comprises 1 and 2, or 1 and 3, or 1, 2, and 3.

Today, Jaspers’s doctrine about what is immediate will presumably seem peculiar and rather special. We do not live in the present moment but in something that has a considerable fullness in time. The moment is formed by abstraction. Furthermore, tones and colors do not have the special relationship to the immediately given that psychology in Jaspers’s period assumed. We do not live in a shop where tones, smells, degrees of hard-

ness, and so on are to be found on shelves. Here Gestalt psychology and phenomenological trends have provided correcting insights. Furthermore, *inventorying* the immediately given is not the same as *describing* the immediately given. Here Jaspers encounters the problem of the relationship between living in a certain world and precisely conceptualizing that world. What can reflection and consciousness achieve here? Are we not inhabitants of this immediate world? Furthermore, can the world in which we live be abstracted from our evaluations and attitudes? Jaspers answers no. Under the title "Evaluations" (*Wertungen*), he touches on the plurality of values, noting that value hierarchies vary from individual to individual. If I find Vigeland Park beautiful, that which I find beautiful does not move from Oslo to Paris when I myself move. The beauty is in Vigeland Park, not in me. It belongs to my world picture, to that which I conceive as being outside myself. The same notion applies to a host of other evaluations. Turning to attitudes, consider how an object's utility contributes to our view of the object.

Attempts to draw a distinction between internal and external will have deep repercussions on philosophical problems. These repercussions will manifest themselves in different ways, which depend on what basic philosophical positions are assumed. These efforts will, of course, also vary according to the results of scientific research (psychology, sociology, and so on), which different individuals seek to exploit or ignore.

These critical remarks may make it appear as though my aim is to replace Jaspers's classification, but that is not my intention. The remarks are meant to illustrate difficulties that immediately arise with such distinctions. As soon as they are expressed somewhat precisely, such distinctions can no longer be viewed as philosophically and scientifically neutral.

Attempts to delineate immediate from more reflected worlds, and sense-worlds from conceptions of more or less hidden or merely imagined worlds (for example, worlds in which there are no secondary and tertiary sensory qualities), are nevertheless important for understanding how the search for total systems arises. They remind us that "everything hangs together." The various proposed worlds appear, at least in part, as ethereal or arbitrary abstractions—but abstractions from what? There is a great temptation to answer: from the only concrete and real world. But which one is that? From what world are the different conceptions we have been considering drawn? It is precisely the different conceptions of that world that we

have sought to consider. It is this treatment that first (heuristically) motivated the introduction of abstractions.

Let us now move on to Jaspers's types according to Division Fundamental No. 2. According to Jaspers, three types of world pictures develop by differentiation from the immediate world picture, which contains the seed of all later ones. They are:

1. Nature-mechanical world pictures.
2. Nature-historical world pictures.
3. Nature-mythical world pictures.

The nature-mechanical pictures are nonsensuous; they are construed by abstraction and mathematical analysis. Everything qualitative and intuitive is forced out of the world. Nature becomes something that can be computed and controlled.

The nature-historical world pictures retain the secondary sensory qualities and seek to provide a natural division of the sensuous variety of nature. Jaspers says that loving absorption in individual phenomena (insects, crystals, mountain formations, and so on), a morphological appreciation of everything that is created, is characteristic of this world picture.

The nature-mythical world pictures contrast with the other two (and developed from them) by making room for what the others consider to be "mere" experience, symbolism, mental life, or fiction. The atmosphere of scenery is in the scenery, not in the soul. The atmosphere has object character. "Objectively, phenomenologically formulated, man finds in this world infinitely many connections and analogies, which we know from the whole of history, all the way from the Babylonian doctrine of the connection between the movements of the stars and human destinies: everything in nature stands in an intimate kinship: human beings, stars, animals, plants, organs, minerals, metals" (Jaspers 1919: 161).

The three types of world pictures have become "absolutized" into *philosophical* world pictures: mechanistic (for example, Democritus), naturalistic (for example, Ernst Haeckel), and nature-mystical (for example, romanticism, theosophy, and Gustav Fechner).

Implicitly, Jaspers assumes that philosophy seeks more comprehensive and unified pictures. A demand for a total picture is operative, and it is

strongly supported by a requirement for consistency. In an (unphilosophical) individual, however, the three types of pictures can coexist, each alternating with the other in domination.

In line with Wilhelm Dilthey's view, Jaspers exemplifies the "absolutization" that is common in attempts to form total systems (in my terminology) by referring to Goethe's passionate struggle against Newton's mechanical theory of colors.

Goethe held the mechanical explanation of nature as something absolute, as philosophy of nature, while it was merely an abstracting emphasis of a connection with the purpose of grasping and mastering nature from this point of view. Likewise, Goethe absolutized his own nature-historical procedure and had to fail insofar as he did not recognize the essence of the other procedure.
(Jaspers 1919: 142)

Jaspers's view of system differences between Goethe and Newton is itself part of a larger web, part of Jaspers's view of history, methodology, mechanical explanation of nature, and nature-historical procedure. From other "meta-systematic" points of view, such system differences may contrast profoundly. An example is the difference seen from the point of view of Hjalmar Hegge (1967). According to Hegge, Goethe's methodology is based on an epistemologically fundamental position in which man has the capacity of *exact sensuous fantasy* ("eine exakte sinnliche Phantasie"), a specific cognitive ability. Goethe's "sensuous fantasy" refers to a form of cognition that must first be developed through the researcher's work with the material of experience. It is not part of his makeup (except as latent talent), but rather is comparable to an *organ* (an expression used by Goethe himself) that is developed, or perhaps, trained, by systematic use. Here, Goethe's view of science is radically opposed to the generally predominant conception in epistemology today.

Jaspers suggests that a (nearly) total system must contain all three pictures—a very daring position from the standpoint of the doctrine of systems.

The fight about the world pictures always begins only with the absolutization, that is, when a world picture will apply to everything and for everybody: nature is not only a dead mechanism, not only life, not only mythical world. It is everything, but only for the one who looks positively and the one who exclusively looks at the moment, not for the one who denies, the one who takes from each standpoint merely the no in relation to the others. (Jaspers 1919: 143)

This seems very convincing—a speech in favor of the open mind that makes room for everything. The positively oriented person in Jaspers's terminology, however, is not yet a philosopher, and maybe he is right in abstaining from such attempts. For it is precisely the attempts to integrate the three types that lead one to interesting difficulties. The absolutization that Jaspers mentions can be reflected or superficial. In the deeply based absolutizations, room is sought for all three pictures, but within the frame of a single total view. Consider the atomic theory of antiquity which contains the doctrine of mental phenomena as especially smooth atoms. In this theory, atoms cannot be identified as corporeal or material. An atomic world with only smooth atoms contains only souls and gods! Even if all atoms have extension, this does not entail corporeality. The atomic theory provides examples of "absolutizations" of the nature-mechanical world pictures wherein *everything* sought is encompassed. Analogous examples can be given for the other two types.

All models, however, are restricted in their accuracy. No model or method can justifiably claim to be regarded as fundamental or all-inclusive. This relativity is itself relative. It can be denied without inconsistency; the nonrelative standpoint is not, however, thereby reestablished and stabilized. This was my position in *Erkenntnis und wissenschaftliches Verhalten* (Science as behavior):

We believe that all models and methods must have a limited range of application and that they are most effective within this range. We have strongly emphasized the relativity of models in connection with subjective models. We can now emphasize that in our opinion behavioral models are just as relative as the subjective ones.

Our asserted relativity is also relative. When someone objects that the argumentation is either too unclear to be disproved at all, or leads to an infinite regress, then we answer: Indeed, if we both assume fixed models where (1) infinite regress is definable and (2) a prohibition against *regressus ad infinitum* is derivable—or if we both accept the same criteria of clarity. Perhaps we finally agree and accept the same models and criteria, but perhaps we don't agree!

If we disregard a model's relative unsuitability as a means to solve specific tasks at hand in an appropriate way, then any suitable model can justifiably claim universal applicability. In this sense any method, any attitude that serves to master a situation, can give occasion to an all-inclusive absolutistic system. It is merely a question of applying sufficient time and fanaticism to it.

(Naess 1936: 247–48)

Hence, we need a concept of absolutization that is evaluatively neutral — a concept that covers, in principle, a completely defensible generalization or extension of propositions or postulates with respect to a range of applications. From intended validity within a partial area, or from an aspect, one generalizes to unrestricted validity.

*Second Main Class: Total Pictures with
Point of Departure in the Mental and Cultural*

Jaspers depicts the following constituents:

1. The individual's own immediate world.
2. The world of the other and the stranger.
3. The infinite world of the intelligible.

Jaspers illustrates the individual's immediate world by referring to the fact that we construe meanings and motives as something perceptible and objectual: we understand or misunderstand people, actions, and works of art. (We note in passing that misunderstanding is by no means the least interesting and revealing form of understanding.)

We also seek to immerse ourselves in the worlds of others, in other cultures. We consciously seek to see things from points of view that are foreign to us. Our world can expand to comprise, *inter alia*, spiritual, logical, aesthetic, religious, and political spheres. The potential for expanding our perspective is limitless, but it necessitates becoming aware of our own perspective's relativity.

In the world picture of the infinite comprehension there are unlimited possibilities for experiences and cultural contents for man. One sees the limitation and relativity in one's own mental and cultural existence, insofar as this has objective form.
(Jaspers 1919: 153)

So much material and so much comparison can render our own existence problematic and deprive us of our instinctive will. The enormity of such a project may lead to internal submission and to satisfaction with contemplation and inauthenticity. If the historical and psychological world picture is absolutized on the basis of such tendencies, one speaks of *historicism* and *psychologism*. The person who becomes addicted to such absolutization may be

characterized thus: Instead of arguing, he understands and explains. Instead of choosing, assenting, or opposing, he passively acquiesces in everything effective because it is effective. He refrains from any evaluation (Jaspers 1919: 158). These words are well worth considering when one tackles an investigation of world pictures *en gros*. Absolutizing historicism and psychologism is then close at hand. In Jaspers's own case, his typological endeavor did not lead to any philosophical self-renunciation. We note that Jaspers's typological endeavor was not performed with a purely descriptive aim either: Jaspers selects and discards, recommends and deprecates, gives praise and blame. He does not pretend (or should not pretend) to be evaluationally neutral. In that respect, the title *Psychologie der Weltanschauungen* is misleading, as Jaspers suggested in the preface to a later edition of his work.

Third Main Class: The Metaphysical Total Pictures

Types divided according to *content* are:

1. Mythological-demonic world pictures.
2. Philosophical world pictures.

These types can be further divided into:

- a. Absolutizations of some concrete world pictures.
- b. Rationalistic and pan-logistic world pictures.
- c. Negative theologies.
- d. Mythical/speculative world pictures.

In addition, philosophical world pictures can be subdivided according to a particular philosopher's ways of thinking:

1. Beholding types.
2. Substance-directed way of thinking.
3. Formal way of thinking.
4. Ordering/receptive way of thinking.

According to Jaspers, metaphysical world pictures are those that "concern the *whole* and the *absolute*" (1919: 161). The metaphysician responds to

the question "What is the actual, ultimate reality?" Plato answers by referring to the realm of ideas: the sensible world is unreal, or not of the highest degree of reality.

In the mythological-demonic world pictures, people *live*, "one does not ask intellectually." These people provide intuitive background through myths about the world, about the origin and destruction of gods, and about obscure forces that form our destinies. In contrast to the mythological-demonic world pictures, the philosophical ones, according to Jaspers, are characterized not by depending on authority, but by appealing to the individual's own original experience. The picture is sought empirically and is justified intellectually.

Absolutization of mechanism, naturalism, psychologism, and historicism gives rise to philosophical world pictures that can be divided into two types: those that take the object for their point of departure (materialism) and those that take the subject for their point of departure (spiritualism). In the rationalistic and pan-logistic world pictures, outlines of the connections between our views are absolutized. Instead of saying that everything is either stuff or something else perceptible, one says that it is logos, number, being, becoming, and so on. Jaspers himself construes a metatheory, perhaps on a Kantian basis, with "forms of connections of our views." Regarding the opinions about the absolute itself, Jaspers believes that Hegel has provided a clear survey:

The absolute is Being (Parmenides), is Nothing (Buddha), is becoming (Heraclitus), is quantity (Pythagoras), is object (Leibniz, monad), is substance (Spinoza), is subject (Kant, Fichte). For Hegel himself the absolute qua spirit is all of this, each exists merely as moment. (Jaspers 1919: 174–75)

This quotation shows, rather dramatically, what the result may be when one thinks it is possible to stand above the systems and describe them objectively and clearly, both as they themselves were intended and within one's own supersystematic frame. Everything is perspicuously ordered by a small set of words that the metasystematician presumes the reader will understand in the same way he does.

There is, however, scant reason to believe that such a supersystematic conceptual framework can be ferreted out and made precise. Such surveys of all intended absolutes can be comprehended only as long as one is permitted to float on the purely everyday associations of the words *be*, *not*, *become*,

number, object, and so on. As far as I understand, however, such a level of comprehension is inadequate. This becomes apparent when one immerses oneself in the writings of any particular philosopher: the more deeply one penetrates, the more one manages to see *everything* from this one system — provided the philosopher actually intended a view that concerns everything. Other philosophical attempts will then either not be perceived or will find a place within the frame in which one immerses oneself. Hence, an opportunity for fully coordinating the different systems does not seem to be present; not if one seeks to move from word to word-meaning, sense, and concept. As long as one keeps to the very wording in “Spinoza means that the substance is the absolute, not being, nothing, world, and so on,” one can obtain a kind of superficial understanding. If, however, one asks, What does the word *substantia* stand for in Spinoza’s thought?, the intelligibility of the schema diminishes until it becomes more or less meaningless.

In relation to Jaspers’s exposition at this point, I shall, in conclusion, only pose the question “What basis does Jaspers’s account rest on?” Does a statement such as “Spinoza takes the substance, not the object to be the absolute, Leibniz the object, but not the substance” make sense if one assumes plausible interpretations of their writings? Can such a comparison be undertaken at any level other than a superficial one?

The third kind of philosophical system, according to Jaspers, is the “negative theology.”

The thinker constantly experiences that, when thinking, he turns that which he thinks into object, thereby delimiting it, and that by virtue of this he no longer retains the totality: the object of his thought has become finite. These inescapable characteristics of the rational constantly become conscious, and so the totality of the world picture ends with merely *negations* and *paradoxes* being asserted about the totality. Thereby one withholds from knowledge that which is reserved for experience, appropriation, and faith, that which is inexpressible, unknowable, but which becomes symptom of forces in man and gives the direction to his interminable endeavor. By its form this world picture is only pure reference without content (Cusanus’s *docta ignorantia*), in ancient theology as in Kantian philosophy, however different these worldviews can otherwise be.

(Jaspers 1919: 175)

The point of departure for the negative theology, as Jaspers describes it, seems very special. This is because one must already operate with specific

kinds of concepts and postulates before one can justifiably assert that certain characteristics of the rational are unavoidable and that the only possibility left for statements about the whole consists of negations and paradoxes. This is not meant as a criticism of Jaspers's division, but as a reminder that all the conceptual demarcations ('rational', 'the whole' or 'totality', 'subject', 'object', 'paradox', and so on) and methodological postulates or axioms must be presupposed *before* one can, on a rational basis, accept or reject such a division. Jaspers presupposes acceptance of an enormous conceptual apparatus.

The fourth type of philosophical system, the mythical-speculative world pictures, is viewed by Jaspers as closely related to the negative theologies. (Jaspers uses only nine additional lines to discuss them.)

Finally, Jaspers subdivides the world pictures according to the ways of thinking (*philosophische Denkungsarten*) that are manifested in the pictures. The beholding type is the most original and uniform. The substance-directed is the most creative with respect to conceptual richness. It seeks, above all, logical justification and coherence. The formal becomes empty because the subject and "the personal-philosophical existence" become unimportant. "From a thoughtful, worldwide mastery of the achieved and acquired arises a scientific school activity that has a strange ambiguity: modest and omniscient" (Jaspers 1919: 187).

With this brief rendering, the reference to Jaspers's rich and instructive work must be concluded. It is regrettable that the line that Jaspers has taken up with so much energy and insight has not been continued. Typology of worldviews is a central concern for systematicians as well as for historians of philosophy, and, presumably, it cannot be pushed to the periphery of psychology and social science for long, as is now the case.

For the central problems of this book, Jaspers's work has significance as support for the following points of view:

1. Even though philosophical systems have many peculiarities in aim and execution, they can be adequately understood only against a broader background, namely worldviews. Fundamental ideas and points of view, and also ultimate aims—systematic and personal—have had and presumably will continue to have their origin in worldviews. As an example, consider Spinoza's struggle for clarity

in the matter of worldview, which brought him into purely philosophical lines of thinking. His *Short Treatise* on God, man, and man's happiness still contains much material that prophetically addresses the mythical and religious aspects of life. Using a purely philosophical treatment, and emphasizing logical consistency, Spinoza transformed the ideas of the *Short Treatise* in the *Ethics*. The *Ethics* is a monumental work of a purely philosophical character, but it is presumably incomprehensible unless it is investigated in its genesis from the point of view of history, history of ideas, biography, and logical systems.

2. Worldviews are immensely varied and exhibit so many differences that one must be exceedingly careful with generalizations. The latter may be regarded most fruitfully as camouflaged research programs and working hypotheses, without sharp demarcation.
3. Any schema, *fundamentum divisionis*, that aims at a relatively precise survey and ordering of philosophical systems must be neutral in relation to the contents of the systems that are to be classified.
4. Lack of neutrality does not reduce the value of the divisions as long as it is not concealed.
5. The more a division formulation is vague, imprecise, or phrased in general terms, the less obvious is any lack of neutrality.

The Goal Is to Exhibit the Difficulty and the Possibilities of Constructing Systems

In the following, I shall try to clarify a selection of difficulties involved in formulating systems. We shall first concentrate on giving examples of "verbalized total views" or "outlooks" and call them, in short, systems. Again and again, attempts at clarification miscarry owing to lack of examples, lack of something that illustrates "the kind of animal" that we are talking about. It will soon become clear that this deficiency is not easily alleviated. On this point, too, we must capitulate to some extent.

The thought of drawing examples from textbooks on the history of philosophy immediately suggests itself. These books offer a basis for condensed representations of the systems of Aristotle, Descartes, Spinoza, Hegel, and others. Starting with pretensions of objectivity, however, one

becomes easily overwhelmed with uncertainty trying to interpret the original intent of systematicians. Furthermore, one cannot avoid the fact that even highly condensed representations become rather complicated. Therefore, I must, at this stage of my account, give up the pretension of fully understanding the total view of any other particular human being. Absolute accuracy of historical details must not be pretended. It is enough for my purpose to sketch systems.

The Function of Philosophical Debate

Adherents of a particular system tend to believe that there could exist a representation of the system that is complete in the sense that it provides the key to the system's answers to all essential questions. This may mean nothing more than that the system can provide answers to essential questions posed from within itself. The outsider will ask for a road map to the terminology of the system, to the very question formulations as they are interpreted by the system's defenders. Without guidance, however, the danger of misunderstanding is imminent.

Misunderstandings may be unraveled by philosophical debate. It turns out in practice, however, that it is very difficult to engender highly objective and professional debate on a highly objective and professional debate. Since a philosophical system is not only comprehensive but also seeks to go to the bottom of things, the individual who is strongly engaged with a system will also look through that system's spectacles and listen through its ears. This difficulty may become acute when the talk is of other systems, other principles, and other possibilities. No question formulation can remain truly neutral. Nevertheless, contributions to the discussion have presumably often had a clarifying effect at certain points. A contemporary example is Bertrand Russell's discussion with Frederick Copleston about the formulation "Everything must have a cause" (1957). Russell's system contains a principle of causation that only requires each thing or event to have a cause, whereas Copleston's principle also requires a cause for everything considered collectively (God). A system difference was clarified at this point. Another example is found in Heidegger's (1949) *Letter on Humanism*, in which he succeeds, to some extent, in clarifying a profound difference between his and Sartre's views of man.

When one attempts to judge the degree of clarity with which a system difference is delimited, one's hypotheses about the definiteness of intention and the level of discrimination within the systems will play key roles. Less potential for discrimination implies less potential for clarification. One may have a high level of discrimination but a low level of definiteness of intention, which also yields a low probability for clarification. A characteristic common to the great systematicians, however, is their formidable depth of intention, be it explicit or assumed.

If one philosopher is strongly influenced by another, the representation of his system can often be uniquely coordinated, in terms of subject matter or chapter by chapter, with the other representation. Spinoza's enumeration and characterization of "feelings" in the *Ethics* corresponds to Descartes's enumeration and characterization in *The Passions of the Soul* (1649). Their doctrines, however, are not identical. Differences appear between Spinoza's concepts of 'passiones' and 'affectus' and Descartes's 'passiones', and there are further differences in definitions and characterizations of the single "feelings." I put "feelings" in quotes because it is a purely conventional translation. Besides, the English word *feeling* cannot possibly stand for Descartes's and Spinoza's concepts.

If one seeks an exact, point-by-point comparison between the two philosophers' doctrines about particular passions, one must try to supplement their accounts with two new chapters that answer the questions "What would a Cartesian description of the kinds of phenomena that Spinoza calls *affectus* and *passiones* look like?" and "What would a Spinozistic account of the kinds of phenomena that Descartes calls *passiones* look like?"

Generally, let us assume that philosopher *A* has a doctrine about a subject that he designates *K* in the form of a series of theses that all contain the term *K*. Let us further assume that philosopher *B* also has such a theory. The system comparer is then primarily interested in four accounts. First, we must consider *A*'s doctrine about what *A* refers to by the term *K*; let us call this *A*'s doctrine about K_A . We also have *B*'s doctrine about what *B* designates by the term *K*, hence *B*'s doctrine about K_B . Next, we must consider *A*'s doctrine about K_B and *B*'s doctrine about K_A . If *A* and *B* have explicit doctrines about these subjects, the case is in principle relatively straightforward. If they do not, a comparison of *A*'s and *B*'s systems, topic by topic, will require the construction of two accounts in *A*'s and *B*'s "spirit." Only

as an improbable special case, in which A and B refer to precisely the same K , hence where $K_A = K_B$, or even ' K_A ' = ' K_B ', can the system comparer be content with only two accounts. Normally he gets to analyze four:

A 's doctrine about the topic K_A

A 's doctrine about the topic K_B

B 's doctrine about the topic K_A

B 's doctrine about the topic K_B

The person who seeks to compare two systems without accepting either of them as an adequate basis for the comparison must then himself supplement even the most thorough and original representations. A debate between living representatives of the two systems seems to be the richest source of supplementation. Such a debate, however, can seldom be realized.⁸

It is easy to provide examples of how such a profound comparison can become impossible after only a few steps of precization. For example, what exactly is the relationship between statements about "substance" in the works of Spinoza and Leibniz? If the inquirer is demanding precision, he must envisage answers involving systematic reconstructions of both Leibniz's and Spinoza's systems.

Special complications arise in connection with certain topics, such as semantics. If K_A and K_B are semantics with diverging rules for interpreting philosophical system formulations, a question arises as to whether the semantics used by the system comparer in his comparison of A 's and B 's texts is neutral in relation to K_A and K_B . The question of whether the two semantics differ from each other leads at once to difficulties when one demands an exact answer. For, if one reads K_A first and applies it to the representation of K_B , one will probably interpret K_B differently than if one had begun with K_B straightaway. In other words, insofar as K_A is a general semantics, it covers B 's text as well. From the point of view of K_A , the conclusion is perhaps that B 's text expresses a semantics K_B that contradicts K_A at points a, b, c , but not d, e, f . Correspondingly, from the point of view of K_B , the conclusion by application to A 's text is perhaps that the semantics K_A contradicts K_B , but only at points d, e, f , not a, b, c .

Spinoza has, in part, very peculiar conceptions in semantics. If one employs another semantics, one has perhaps already broken with Spinoza's sys-

tem, but how is one to avoid this situation in setting up a comparison with Leibniz? Leibniz may legitimately demand to be judged on the basis of his own semantics, not Spinoza's.

These difficulties appear when one attempts to give an account of what two systematicians separately assert about something. Of course, new difficulties arise if one goes on to ask about the truth or validity of what the two systematicians claim.

Rules for Extremely Simplified System Presentations

Given our present goal of searching for the simplest and most surveyable system constructions, we shall imagine that the presentations of two systems can be made using a pyramidal form. At the top, as the first tier, one places:

1. All propositions that are not entirely derived or justified by other propositions. These may be divided into two sets: those from which consequences are drawn and those from which no consequences are drawn. The latter possess a peculiar trait of being isolated. We will disregard them in subsequent discussions.
2. All rules that are not derived from other rules but are perhaps justified, for example, in that they are necessary for achieving a particular goal. (The concept 'rules' is construed to include imperatives.)

One class of rules may be omitted, namely rules of usage in the form of definitions. For example, Kant's *Critique of Pure Reason* (1781) contains many sentences that cannot be described as parts of the system itself. It may become essential, however, in the course of discussing various systems, to clarify the precise relationship between such sentences, and in such cases, we shall retain the sentences. Consequently, we must distinguish between statements that are part of some representation of the system and statements that are part of *any* representation of the system.

It is rather common for systematicians to introduce new terms without adequate explanation and to use old terms in new ways without the necessary clarifications. Hence, if one finds the formulation p in one system and non- p in another, the two formulations may not necessarily express different propositions; similarly, if one finds p in both systems, the two occurrences do not necessarily express any agreement.

*Simplification Can Generate Misunderstandings: Stepwise
Construction of More Adequate Representations*

When a systematician seeks a condensed rendering of a system, it is natural for him to select basic sentences and basic rules. It is then left to the reader's imagination and experience to infer what consequences the systematician has worked out. In this chapter, our systems will follow this procedure, although it is obvious that this practice is insufficient. Since ancient times, the Epicurean and Stoic systems have been characterized by certain basic sentences. Unfortunately, many people have drawn erroneous conclusions from the fundamental premises, conclusions other than those drawn by the philosophers themselves. In spite of very different points of departure, the Stoics and the Epicureans ended up with very similar conclusions. Different foundations do not exclude essential similarities in the houses built on them.

If one seeks an immediate and fully adequate comparison of two systems, the systems will become completely unsurveyable. It must be possible, however, to carry out a comparison of systems in phases or stages.⁹ For the metasystematician, then, it is tempting to work primarily with systems that are constructed pyramidally or seem amenable to pyramidal reconstruction. Such projects are far more complex if, however, one cannot indicate basic premises and basic rules. The metasystematician must then seek a kind of listing of "terminal" sentences, which justify but are not themselves justified and which appear as isolated constituents of the system.

It is tempting to attribute a pyramidal character to systems that are unclear and not consistent enough to warrant such an interpretation. The eminent philosopher Ernst Cassirer may have succumbed to this temptation. Consider Cassirer's introductory remarks in his study of Nicholas Cusanus's philosophy:

Any study that seeks to view the philosophy of the Renaissance as a *systematic* unity must take as its point of departure the doctrines of Nicholas Cusanus . . . Cusanus is the only thinker of the period to look at all of the fundamental problems of his time from the point of view of *one* principle through which he masters them all . . . Indeed, all his thought is simply the unfolding and extension of that fundamental first principle developed in his first philosophical work, *De Docta Ignorantia*.
(Cassirer 1963: 7)

If Cassirer is correct, then Cusanus achieved the most cherished dream of many philosophical systematicians. Moreover, if Cusanus did succeed in

this, then presumably others have too—hence the research program of representing philosophies as wholes of the kind that Cassirer thinks Cusanus has realized. The great question, however, is whether Cassirer has not to some extent been deluded into confusing dream with reality. Has he actually—on the basis of the few existing Cusanus texts—accomplished a reconstruction of their contents in pointed, pyramidal form with a well-defined principle on top? Without disparagement to Cassirer, I must emphasize that he only gives suggestions to this. Implementation would require an approach with quite another depth and breadth.

If we are to begin addressing the most far-reaching problems of this work, we must, for the time being, (1) disregard the task of giving an example of a system that is especially tenable or probable. Should a simplification be necessary in order to consider strikingly erroneous statements, we must undertake that simplification without scruples. It is to systems that we shall find our way, not to a particular subclass, 'true systems'. Furthermore, we must (2) give up formulating examples as precisely as possible. Something that might require one hundred words to express precisely can often be expressed adequately in ten words. That is what Cassirer does in his presentation of Cusanus.

Other complications also arise. When we attempt precise formulations, we become dependent on a conceptual framework, a conceptual metasystem, that is then essential to describe the system at hand. As a result, certain difficulties will arise at an inconveniently early stage in our deliberations. Of particular importance is the difficulty associated with representing a system without presupposing another system as a frame, a wider system within which the former is presented. Although the metasystem might go beyond the system only at certain points, it, nevertheless, presupposes the possibility of seeing the system from the outside, a possibility that can hardly be present for the systematician himself.

It is disappointing to limit our presentation of examples to synoptic minima, but the two alternatives generate even less satisfactory examples. The first leads to examples that are unmanageably complex for our purposes (for example, A. H. Winsnes's otherwise satisfactory presentation of Jacques Maritain's philosophy). The second leads to meta-researchers who fail to give examples of whole systems (for example, Everett Hall).

Tore Nordenstam (1968b) has subjected what he calls "the deductive

ideal in ethics” to a criticism in principle.¹⁰ His study is relevant to our discussion because ethics *is* an essential part of total systems and because we shall focus our deliberations on the pyramidal simplifications of systems.

Nordenstam thinks that the deductive ideal is clearly formulated by Richard Brandt in the following words:

Ideally a normative “theory” consists of a set of *general* principles analogous to the axioms of a geometrical system. That is, ideally it comprises a set of *correct* or valid *general* principles, as *brief and simple* as possible compatibly with *completeness* in the sense that these principles, when conjoined with true nonethical statements, would logically imply every ethical statement that is correct or valid. Such an ideal for a system must be our guide. (Brandt 1959: 295)

Nordenstam’s criticism seems completely justified, and it is good that Brandt in his valuable works has not let himself be led by the ideal he has set up!

Nordenstam suggests (1968b: 30) that Johan Galtung and I (1955) either had such an ideal for ethical systems or held that an ideal form of ethics must be deductive. This assertion does not hold water. In our opinion, it is a valuable undertaking to construct models for ethics or ethical systems, but this is not the goal of *descriptive* ethics. Models must necessarily be simple insofar as they are construed as descriptions. Nordenstam refers to a reconstruction of Gandhi’s political ethics in which it is explicitly stated that we are considering a model, not a description. In terms of the model, “derivations” are performed. By precization, these derivations acquire the character of deductions. In authentic systems, derivations are performed, but these can hardly be considered “deductions.” Deduction cannot be an adequate paradigm for derivation within a system. Furthermore, one must distinguish between an ethical system and ethically relevant behavior, both verbal and nonverbal.

Nordenstam correctly emphasizes the “openness” of ethics: “A satisfactory ethical system e.g. must be flexible enough to be amenable to application in new and unforeseen kinds of situations, and in order to be flexible the system must to some extent be open” (1968b: 30). Mathematical terms, on the other hand, have no flexibility (as parts of axiomatic systems). This distinction is well established. When Spinoza defines and derives deductively, this process occurs with concepts such as ‘definition’, ‘proof’, and

'derivation', which are *not* drawn from mathematics. To make reasonable sense of the statement quoted from Brandt, one must presume that he employs classical philosophical terminology. Even if Nordenstam's criticism scarcely hits as hard as he intends, it is nevertheless commendable to emphasize that one is led astray by thinking that concrete ethical decisions can be deducible from more or less general ethical norms and descriptions of situations. This ideal cannot be presumed in ethics any more than it can be presumed in jurisprudence.

Thus, descriptive ethical research cannot be a search for basic ethical norms from which the rest of an ethics can be derived.

The open structure of ethics plays a role that has important methodological consequences for descriptive as well as normative ethics. Search for fundamental norms can no longer be regarded as the principal task of the descriptive analyst; his task will be to describe the whole system of moral norms, general and special, for the general norms cannot be fully understood in isolation from the special features which contribute to fixing their meanings.

(Nordenstam 1968b: 34)

This is quite clear as far as the ethics of nonviolence is concerned. A long series of more or less special norms is required to begin outlining what nonviolence involves. It is of no use to combine all ethically relevant concerns into a single basic norm "Be nonviolent!" One must proceed from rather special norms to particular individual decisions.

Knowledge of general principles in a person's ethics does not render it superfluous to gather knowledge of his special moral decisions, just as acquaintance with the statutes of the lawbooks does not remove the need to study particular cases. Casuistry is an essential part of ethics.

(Ibid., p. 34)

Synoptic Philosophical Systems

Statement Sets That Satisfy Minimal Requirements for a System

What we, at least for the present, are searching for may be specified as follows: sets of brief, vague and ambiguous, true or false, but not completely unintelligible or vapid formulations that can act to demarcate

roughly and provisionally the basic features of a selection of systems. Such presentations cannot afford to be elaborate. We seek something that will just barely qualify as a representation of a whole system. The expression *synoptic systems* is perhaps good enough if one keeps in mind that while requirements of precision are minimal, requirements of explicitness are not.

To be philosophically all-embracing a philosophical system needs to express a position on each of the five main topics of philosophy as they are usually represented. In general, to be all-embracing, a system must: say something general and fundamental about what is (ontology), say something general about knowledge (epistemology), say something about conditions for cognitive meaningfulness (semantics), have a consistent logic, and outline an ethics. Sometimes we can manage with less, but it is not an insuperable task to get at least one formulation from each of the five fields.

Everett Hall (1960: 3) contends that the categories of systems tend toward a triadic pattern corresponding to the classification schema of the philosophical disciplines: ontology ("investigating being"), epistemology ("studying knowledge or, more broadly, any reference"), and axiology ("concerned with values"). He therefore thinks that in order to ascertain differences in the categories of systems, one ought to search for principal concepts that are used or that are presupposed when there is discussion of existence, reference, and value. Hall's schema may, at times, provide a fruitful basis for comparison, but it will quite likely prove to be unmanageable when a philosophy's ontology is axiological (Heinrich Rickert?) or its axiology is ontological (Spinoza?). Similarly, it is difficult to reduce logic to ontology, epistemology, or axiology.

With regard to logic as a main topic in systems, a controversial question among foundation researchers in logic is the extent to which different formal logics may exist or be constructed. W. V. O. Quine (1953), however, claims to find an example of very different formal logics or logical ontologies in connection with the extensional/intensional distinction:

Intensional and extensional ontologies are like oil and water. Admission of attributes and propositions, along with free use of quantification and other basic idioms, rules out individuals and classes. Both sorts of entities can be accommodated in the same logic only with the help of restrictions, such as Church's, which serve to keep them from mixing; and this is very nearly a matter of two separate logics with a universe for each. (Quine 1953: 157)

The conceptual framework of a system determines which questions can and cannot be considered, but can the metasystematician know this in advance? In the last chapter of this book, I address problems of this kind; for now I shall only add a brief comment.

The following question posed by Thomas Aquinas could also arise and be posed within other Christian medieval systems, but it could hardly be posed in any modern non-Christian system: will human beings need food after the resurrection? Great difficulties arise in connection with the introduction of 'resurrection'. If they are not addressed, the question cannot even be posed. W. M. Kneale and Martha Kneale condense Thomas Aquinas's argument as follows:

[I]t would be a denial of the good ordering of God to suppose that eating will take place after the resurrection. For a man who eats without losing anything by corruption will grow to an immoderate size (*perveniet ad immoderatam quantitatem*). Indeed, if after the resurrection a man continues to eat through the whole of his unending life, we must suppose that he will grow to infinity (*oportet dicere quod corpus hominis resurgentis in infinitum augeatur*). But that, he says, is impossible. (Kneale 1962: 647)

Since Aquinas's reasoning is remote for most of us, it is comparatively easy to list presuppositions or assumptions that must be taken for granted in order for the question to arise. Here it will suffice to mention two: (1) human beings are in a certain sense resurrected after death, and (2) human beings have a body after death. Only when these presuppositions are accepted can we attempt to put forward and assess a solution. In contrast, when we are very intimate with a particular way of thinking, it is often more difficult to identify such presuppositions and assumptions. In such cases, the system appears to stand on its own two feet, accessible and comprehensible to all.

The intended totality of a total system must not be exaggerated. In the first place, philosophical systems cannot describe everything; they should recognize their inherent incompleteness. It is the "types" of things, the objectual, that systems aim at, rather than particular individual things. Even in the case of types, it is clear that not all of them are intended to be separately characterized or characterizable.

A reflection of Jaspers's and many others is worth mentioning in this connection:

A simple logical consideration shows us that nevertheless the whole and the absolute *cannot be object* for us, because as subject we would then have to stand opposite those as an object, while we as subject are then not included in the object, hence the whole is then not the whole. (Jaspers 1919: 161)

When a philosopher at a certain time or by an act of thought seeks to contemplate his total system and thereby also reconsider what the statements of the system are about (refer to, designate, denote), he cannot actually perceive the act of thinking. The dated, concrete act of thought is part of a process. His next “act of thinking” may turn the preceding one “reflectively” into an object. He objectifies what went on. If he thinks that what went on was worthy of particular philosophical attention, his system will include a characterization or theory of such acts of thought. By “such,” we mean a kind of thought, not a singular event. The system is “open” in the sense that new acts will fit in; they do not rupture the frame. The whole is not “the whole” in a certain absolute sense, but rather how one might reflect the intention of a thinker that aimed to encompass everything singular.

One is reminded of the myth that Hegel refers to about the embittered god who creates but thereafter swallows the world. In clear moments, great system builders have hardly conceived of the system idea as crudely as this. They do not seek to halt the course of events simply to prevent something, to which they did not already lay claim to, from happening. Hence, Jaspers’s misgiving does not seem to apply to *philosophical* systems in which totality is viewed as intention.

Full Explicitness Is Required of Synoptic Systems

In a certain respect, our synoptic systems are not at all minimal. We make more than minimal demands with regard to our requirements for explicitness. This is a consequence of, *inter alia*, the goal of establishing maximal comparability between synoptic systems. Furthermore, we require that the text be intelligible and valid without reference to biographical material, such as statements about the author’s emotional life. Pessimism in principle is not verified by reference to an author’s remarks about being terribly pessimistic and depressed. A doctrine of anxiety as a basic human attitude in principle is not verified by the author’s anxiety at one moment and then falsified by his lack of anxiety at a later moment. Systems are consid-

ered to be “detached” from the personality of the system builder. The builder may have some special basic attitudes, but they must be verbally expressed with a claim of intersubjective validity in order to be accepted as examples. Statements such as “The spiritual is higher than the corporeal” and “Any evaluation is subjective” may be accepted, but not “To *me*, the spiritual seems higher than the corporeal.”

The more deep-seated a standpoint, the more difficult it becomes to maintain its separability from the personality said to take on this standpoint. Emmanuel Mounier’s personalism reflects his way of experiencing and associating with his fellow human beings. Personalism as a system standpoint, however, is nonpersonal. Its truth conditions do not refer to the person Mounier. As soon as the detachability requirement is rescinded, however, the system loses its intersubjective, cognitive character. The task of comparison then disappears as a *philosophical* task, falling instead to biographers, psychologists, sociologists, biologists, and physicians. The nonpersonal character of the systems does not mean that the study of systems can be undertaken without any study of persons; quite the contrary. The person who wants to understand and compare must, as directly as possible, expose himself to different personalities and identify himself with their life situations. (In Indian philosophical tradition, the direct personal confrontation appears, in principle, to be presupposed. A verbalization — or an eloquent silence — must be interpreted in terms of the total teacher-disciple relationship.)

A person may change his mind about female ministers, pacifism, and much else, but can he change ways of understanding, total views, and conceptions of himself and the outside world? The question is open. Some will, from certain premises, answer yes; others, no. It seems clear to me, however, that one cannot “have a (nearly total) system” or “have a total view” the way one can “have an opinion.” One ‘is’ more than one ‘has’, in the case of deep-seated attitudes. We agree, presumably, that one person cannot be several persons, but can one person compare several systems? Which more profound ways of understanding can this person have? The question is a burning one if, like Guttorm Fløystad (1967: 2), one has the working hypothesis “that a person’s identity is closely connected with a person’s understanding, that a person in an essential sense is or is constituted by his understanding.”

When we say that Spinoza’s, Leibniz’s, and Kant’s philosophies give expression to three different personalities’ total views of existence, it is not a semantic relation that is intended but a psychological or existential one.

The system may be more or less apt and may be a more or less pedagogically and didactically adequate expression of a fundamental view or vision. Dilthey and his school sought to understand systems in terms of noncognitive expressive relations. Carnap emphasized them and declared the cognitive claims to be misapplied: the metaphysicians are bad musicians, meaning that they would have, had they been more musically gifted, expressed themselves in music. In this work, however, we take seriously the system builders' cognitive, interpersonal claims to validity. We have no grounds, in principle, for rejecting those claims. We must also acknowledge that a fundamental view or a basic vision can in principle be given expression by different cultural products — poems, philosophical systems, music, drama, and sculpture. Indeed, some individuals are creative in the worlds of several media (Nietzsche, for example).

Three Examples of Synoptic Total Systems

Formulation Set F1

1. Everything that exists can be experienced.
2. The ultimate source of all knowledge is experience.
3. Two statements that directly contradict each other cannot both be true.
4. All cognitively meaningful statements are statements about something that can be experienced.
5. Choose the strategy of action that gives the greatest possible happiness to the greatest possible number.

The first two sentences, ontological and epistemological, are meant to exemplify possible sentences in an “empirical” system. The third sentence, which exemplifies a logic, is merely a formulation of the principle of contradiction. The fourth sentence provides an empirical criterion of meaning (of the form that is associated with logical empiricism). The fifth sentence, a formulation of the highest norm for deciding actions, introduces a utilitarian ethics.

The catchword *empiricism* fits the formulation set F1. The following set, F2, is intended to exemplify “materialistic” systems. The list is not intended to fulfill the minimal requirements in every detail.

DESCRIPTIONS OF MAXIMALLY COMPREHENSIVE PERSPECTIVES

Formulation Set F2

1. Of nothing, nothing becomes.
2. Everything that exists is corporeal.
3. Of the bodies, some are composites of bodies; the rest are that which all the other composites result from. These, the "atoms," are indivisible and unchangeable.
4. Atoms have no properties apart from size, shape, and weight.
5. The soul is a body that is distributed in the whole human body.
6. The human body covers the soul and leads sensations to it.
7. Any pleasure is a good thing; any pain is an evil.
8. Spiritual pleasure is better than bodily pleasure.
9. The virtues can be reduced to wisdom.
10. Wisdom teaches that enduring pleasure can be obtained only by nobleness and justice.
11. The basis of all cognition is sense experience.
12. Sense experience is true in itself.
13. The immediate objects of cognition are sense pictures, not things in themselves.
14. Criterion of truth: confirmation through sense experience.
15. Two statements that directly contradict each other cannot both be true.
16. All cognitively meaningful statements are statements about something that can be experienced.

The set F2 is a freely condensed rendering of Friedrich Lange's (1866) classical representation of Epicurus's system. The first four sentences may, roughly classified, be said to provide an ontology; sentences 5 and 6 outline a philosophical psychology; 7 through 10 outline an ethics and art of living; and 11 through 14 represent an epistemology. Logic and semantics (sentences 15 and 16) have been added by borrowing from the formulation set F1.

F2 is not an entirely satisfactory example of a minimal system because some of the sentences can hardly be construed as fundamental, for example, numbers 8 and 9.

Formulation Set F3

1. Dialectics

- 1.1. That which exists, nature, is a continuous, uniform whole, where things are organically interconnected.
- 1.2. Nature is in a state of incessant movement and change.
- 1.3. Qualitative changes do not occur by degrees, but quickly and suddenly; not accidentally, but according to law.

2. Materialism

- 2.1. Nature is material; the varied phenomena are different forms of matter in motion.
- 2.2. Matter has objective reality outside the human consciousness and independent from it.
- 2.3. Our knowledge of the laws of nature is reliable, and there are no unknowable things.
- 2.4. Things in nature have internal oppositions.

3. Addendum

- 3.1. Matter can move only in space and time. Thought is a product of matter, specifically the brain. A thought without a brain is absurd.
- 3.2. The function of consciousness is to give reflections ("copies") of that which exists (including the regularities).
- 3.3. There is no god or world spirit or idea; the material, observable world is the only one that exists.

Parts 1 and 2 provide a simplified version of a summary of dialectical materialism given by Joseph Stalin (1940). Stalin provides examples of "internal oppositions" (also, in Hegelian fashion, called "internal contradictions"); thus, opposites are "+" and "-", differential and integral, action and reaction." I have added part 3 myself to provide a more comprehensive picture.

Simple Comparisons: Conflicting Systems and Criteria of Totality

The comparison of systems may be relatively unproblematic. This is the case with F2 and F3. Nevertheless, the comparison of F2 ("Epicurus's system") with F3 ("dialectical materialism") presupposes, *inter alia*, elucidation of three key issues:

1. How are 'nature', 'world', 'matter', and 'mind' related within dialectical materialism? If matter is outside consciousness, then consciousness is outside matter, and thus consciousness is not material. Is consciousness without material being? What is the ontological status of consciousness?
2. What is the relation between *everything* as the word is used in sentence 2 in Epicurus's system ("Everything that exists is corporeal") and *world* in the addendum to F3?
3. What is the ontological status of "sensations" and "sense pictures" in Epicurus's system? What is their relation to "corporeality," to "the soul," and to the "consciousness" of dialectical materialism?

F2 and F3 are related systems insofar as they take up the same questions in the same, or closely related, terminology. The solutions are similar. The systems can, to a great extent, be compared and discussed in a common terminology. Especially important is the absence of differences in logic and (explicit) methodology. Even a very small difference in basic methodological norms can render comparison impossible.

It is easy to see that the formulations of systematizers after Aristotle are to some degree determined in terms of supposed (hypothetical) contrasts to the formulations of other systematizers. They no longer start from scratch but find their bearings in relation to a rich philosophical tradition. In the case of Thales and other very early thinkers in occidental philosophy, it is also evident that they contrast themselves to earlier or contemporary personalities and opinions, but the picture they present is not as well clarified philosophically.

We now need an example of a system that is formulated in direct opposition to another one. We select a "rationalistic" system contravening the "empirical" system F1.

Formulation Set F4

1. Everything that exists falls into two classes: that which can be experienced and that which cannot be experienced.
2. The ultimate source of all knowledge is rational thought.
3. Two statements that directly contradict each other cannot both be true.
4. Any noncontradictory statement has cognitive meaning.
5. Truth is found in the last instance by logical analysis; the thoughts must be made clear and distinct.
6. The good and right way of living is the one that obeys the law of reason.

To begin with, we accept that F1 and F4 express different doctrines, and that the differences, at least in some points, are such that F1 and F4 cannot both be asserted together without contradiction. Therefore, we accept the assumption that F1 and F4 are incompatible. Our assumption may be explicated by listing the vocabularies in F1 and F4 and postulating that the same words have the same meanings, hence that *experience* in F1 means the same as *experience* in F4, *knowledge* in F1 means the same as *knowledge* in F4, and so on. Such an approach presupposes a large set of intersubjective synonymy hypotheses. Substantial difficulties arise when one attempts to verify each synonymy hypothesis.

In such a comparison of F1 and F4 we will also require a definition and criterion for incompatibility, that is, a logic that includes a principle of contradiction. What is suggested of logic in F1 and F4—the principle of contradiction—is common to both systems. However, F1 and F4 have incompatible methodologies: the comparer's conclusion that "F1 and F4 have incompatible methodologies" is intelligible and testable only in terms of a particular logic and methodology. The question immediately arises as to whether this logic and methodology of comparison is neutral in relation to the logic and methodology of F1 and F4. Here we begin to approach the special difficulties that must arise if methodologically incompatible systems are intended to be complete.

If two systems have incompatible methodologies, the partial or total system that is to serve as a frame of reference for a comparison will have to have a methodology that is compatible with either one or the other. By

adopting one alternative as a metasystem at the expense of the other, one takes a stand against the latter even before any comparison has begun. In making such choices, the system comparer cannot be totally "unbiased."

More generally, if two systems are incomplete, the differences between them may be such that a metasystem does not have to side with one or the other, because the two systems may be entirely compatible from a logical point of view. In the case of two total systems, however, the situation is different. In this case, both systems must say something, implicitly or explicitly, about every subject that is taken up. The existence of a logical incompatibility implies that the metasystem takes a stance on one side or the other. *Tertium non datur*—there is no third option. If one system asserts *A* and the other not-*A*, it is of no avail to search for a synthesis or a neutral position. If it is asserted that the first system covers one aspect of existence and the second another, then both have failed to satisfy the pretensions of total systems, because each system was intended to cover everything.

What do we mean by saying that a system is or is not complete and total or that it expresses a total view? We shall try to identify some criteria by beginning quite vaguely:

1. A system is total if (but not only if) it comprises an ontology, epistemology, logic, semantics, philosophy of language, methodology, value theory, ethics, moral philosophy, philosophy of law, philosophy of education, political philosophy, philosophy of history, history of philosophy, and philosophy of nature.
2. A system is total if it comprises all categories.
3. A system is total if in principle no statement is irrelevant in relation to the propositions of the system.

One way of characterizing total systems is to say that if *x* is intended to be total, then nothing can be irrelevant for its validity and nothing can fall outside its categories. Stephen C. Pepper calls such systems "world hypotheses."

Among the variety of objects which we find in the world are hypotheses about the world itself. For the most part these are contained in books such as Plato's *Republic*, Aristotle's *Metaphysics*, Lucretius' *On the Nature of Things*, Descartes's *Meditations*, Spinoza's *Ethics*, Hume's *Treatise*, Kant's three *Critiques*, Dewey's *Experience and Nature*, Whitehead's *Process and Reality*. . . . These un-

restricted products of knowledge I am calling world hypotheses, and the peculiarity of world hypotheses is that they cannot reject anything as irrelevant.
(Pepper 1942: 1)

In practice, and at least to begin with, criterion 1 is the best of the three, but then, of course, its formulation must be made more precise. One must also determine what may be subtracted from the suggested philosophical disciplines without infringing on the totality of a system. An important point is that one must avoid inserting whole encyclopedias into the total systems. There can be no place for science or technology. The relation expression “*x* comprises *y*” must be made precise, perhaps in the direction of “*x* has the explicit or implicit consequence *y*.” Another quite different sense is also of interest, “*x* has *y* as a (genuine) part.”

Finally, for the sake of comparison, we offer condensed presentations of the more traditional type that highlight the main ideas in Hegel's and Schopenhauer's systems. The presentations demonstrate a tendency to use terms that are clarified only within the systems (esoteric elucidation). From the metasystematic point of view, this is the great weakness of the more traditional presentations; they do not lead into systems but rather presuppose that we are already inside them. If one uses ordinary interpretations of words in reading such a presentation, it often becomes meaningless or erroneous.

Hegel's System: Traditional Condensation

1. Appearances for us¹¹ are not only in our consciousness, but also in themselves.
2. The source of their being is the general divine idea (*die allgemeine göttliche Idee*).
3. Absolute reason reveals itself in nature and spirit, and is the substance of both.
4. Consideration of the realization of absolute reason in the development of history forms the dialectical method, which reproduces in the consciousness of the thinking being the self-movement of the content thought.
5. Absolute reason externalizes itself in nature and returns from its difference to itself in spirit (*enttäussert sich und kehrt ibrem Anderssein zurück*).

DESCRIPTIONS OF MAXIMALLY COMPREHENSIVE PERSPECTIVES

6. The realization of absolute reason is threefold: in the abstract elements of thought, in nature, and in spirit; as thesis, antithesis, and synthesis.
7. Freedom is the idea that has returned from its difference to itself, the spirit.

This condensed version of Hegel's system can only be of value to those who are already acquainted with a series of difficult terms that admit a variety of interpretations.

*Schopenhauer's System: Traditional Condensation*¹²

1. The only reality that is independent of our ideas is the will.
2. The will is not an object, but an urge and a desire to be.
3. Any object is merely some subject's idea, only phenomenon.
4. An urge cannot be fully satisfied, hence the presence of suffering.
5. The highest norm for a conscious being is to kill the urge.
6. Compassion alleviates suffering, asceticism kills the urge.
7. Nirvana is the happiest, final stage of asceticism.

If we proceed on the assumption that Hegel and Schopenhauer intend to create total systems, the first great task will be to distill out the common topics. The first sentence in the Hegel condensation says something about appearances for us (*Erscheinungen für uns*). What does Schopenhauer have to say about the subject that Hegel intends by the expression *Erscheinungen für uns*? In the third sentence of the Schopenhauer condensation, the word *phenomenon* occurs. There is reason to believe that by *phenomenon* Schopenhauer means something not far from Hegel's *Erscheinung für uns*. By successive reformulation it is possible (yet not without a certain arbitrariness) to establish that the two systems among other things have a common topic, the phenomena, and that they also contain some incompatible statements about this topic. From the Hegel condensation we deduce: any phenomenon is not only in our consciousness, but also in itself. From the Schopenhauer condensation we deduce, after "translation" to Hegelian terminology: for one phenomenon it is the case that it is not only in consciousness, but also in itself, namely the will.

In an analogous way, the metasystematizer will seek to delimit other common topics. It will soon become evident, however, that there are topics that only one of the philosophers appears to have taken up in his writings. In such cases, we must resort to extrapolation. Even after daring extrapolation, we must still expect the persistence of many topics on which only one of the philosophers can be said to have expressed his view directly or indirectly. This lopsidedness, however, cannot be taken as evidence that the systems are not total in their intention.

Standpoint Combinations: Extrapolation

According to the judgment of posterity, only a few philosophers have produced truly outstanding work in all the principal areas of human, system-creating thought. Others have made outstanding contributions in more restricted areas: Descartes in epistemology and the theory of science, but not in ethics; Nietzsche in ethics and the theory of values, but not in ontology; Frege in logic and semantics, but hardly in any other field. These facts make it necessary, in order to obtain a culturally rich and interesting set of systems, to proceed tentatively when combining doctrines that have less scope or depth into total systems.

From the perspective of psychology, sociology, and the history of ideas, syntheses constructed by combining doctrines from different thinkers, such as Aristotle and Frege, may be regarded as completely "impossible." Anthropologists might deny that any thinker could arrive at such syntheses and at the same time believe in them. From a systematic-cognitive standpoint, however, these "artificial" combinations may be of great value because they exercise our logical imagination and undermine our tendency to believe that the range of cognitively possible standpoint combinations is limited to the few truly comprehensive systems that occur in history.

Let us, for example, focus on the writings of philosophers who are most broadly reviewed in textbooks on the history of philosophy. If we make a list of one hundred questions that two or more of these philosophers have answered, it turns out that certain answers in their systems hang together. For example, answer R1 to question Q1 is more frequently combined with R5 to Q2 than with R6 to Q2. In other words, there exist positive and nega-

tive correlations between the answers when they are classified according to similarity and dissimilarity.

Since we do not, in general, find that different philosophers give exactly identical answers to any definite question, we must make an effort to classify answers that fall into categories such as "very nearly the same answer," "not very nearly the same answer," "answers that are very nearly the negation of another one," "answers that neither very nearly negate nor very nearly affirm a certain other answer," and so on. When we say, for example, that Pierre Gassendi, Thomas Hobbes, and Julian Offray de La Mettrie are materialists, we have tacitly understood that they give nearly the same answers to nearly identical questions. If, however, we add that Gilbert Ryle and Ludwig Wittgenstein are also materialists, either our requirements for similar answers (and the feasible questions) are considerably weaker, or we must reformulate the questions relevant to "materialism" so that they are more abstract or incomplete.

Most philosophical topics are so intricate and the methods of justification so imperfect that the assumption of "cognitive affinity" should be regarded as no more than a working hypothesis. By "cognitive affinity" I refer to a relation between *A* and *C* such that if *A* is tenable, then probably *C* is also; or if *A* is untenable, then *C* is also probably untenable. (If both relations hold, it is perhaps appropriate to speak of "mutual cognitive affinity." The relationship is formal-logical if, for example, *C* follows from *A* in a formal-logical sense.)

Attempts to check answers for cognitive affinity will most frequently result in a weakening of the hypothesis or in a realization that it is untestable, at least with the methods currently at our disposal. The conclusion will often be that the hypothesis about cognitive affinity is useless except as a working program. This usually gives us more prejudices than fresh insights, and it applies perhaps to the majority of hypotheses that seem to be accepted implicitly by the authors of histories of philosophy. A series of clever special investigations in the history of philosophy concludes that opinion *C*, which has commonly been supposed to be accepted by philosopher *F*, is not *F*'s opinion. He has, on the contrary, meant *D*. The assumption that he meant *C* has been determined, for example, by the fact that he has the opinion *A* (and not *B*) about another topic, and one frequently assumes that *C* follows from *A*.

For example, one of Paul Natorp's main theses in his *Die Ethika des Demokritos* (1893) is that Democritus does not mean that pleasure is the goal (*telos*). While it is the case, he writes, that Democritus holds that pleasure and displeasure are the criteria (*tá kritéria*), it does not follow from this that pleasure becomes the goal. Natorp denies that Democritus sets pleasure as the unconditional goal, a position long since accepted by many historians of philosophy; but he shows, by distinguishing criterion from goal, that he can nevertheless make sense of Democritus's doctrine. Furthermore, Democritus, in his ontology, is a consistent materialist and a determinist (in a certain sense). Does he not, therefore, believe that both positive feelings and beneficial material things are most worthy of being coveted?

On the basis of standpoints for which there is no evidence in Democritus, but by association with certain other philosophers who may also be said to be consistent materialists and determinists, one might answer yes. In the sentences preserved from Democritus, however, the philosopher does not emphasize the material, but rather the mental benefits (*tá psychés agathá*). Is he not inconsistent then? Are the fragments spurious? Natorp thinks Democritus need not be inconsistent. Democritus's theory of values emphasizes psychic values, and since his theory of atoms does not exclude the condition that psychic atoms are important causally for one's happiness, the ontology does not prevent him from thinking so.

Natorp continues, and finds a very close connection between Democritus's epistemology and his doctrine that the soul's state of calm is more important than isolated sensations of pleasure. Pleasure and displeasure are ontologically on a par with color and taste; hence they exist only in people's speech and opinion (*nómo*), not in reality. Natorp finds a cognitive affinity, and perhaps even something close to a formal-logical, necessary connection, between Democritus's epistemology and his ethical doctrine. Here, however, Natorp seems to be skating on thin ice. He may be explicating a possibility that stands historically with other possibilities, but if these possibilities are compared with each other, the conclusion would have to be one of *isosthenia* (a state of suspended judgment). The alternatives are equally probable or improbable; there are equally strong reasons pro and contra.

It may well be that from a psychological, sociological, culture-historical, or idea-historical point of view it is unreasonable to imagine an answer of the kind *A* to question *x* combined with an answer of the kind *D* to

question *y*. Precisely because psychological and other kinds of knowledge about man are essential for understanding the opinions of philosophers, we can argue that the maximal variation of psychological and other factors will result in many more combinations of opinions than have so far been represented. One may point to a wide variety of psychological, or at least generally culture-historical (for example, science-historical), factors that have been common to many and perhaps all of the philosophers most frequently cited in history of philosophy textbooks. From a cognitive standpoint, such factors are fortuitous; they do not affect the tenability of arguments or their weight as evidence.

We conclude, therefore, that if one is predominantly interested in the elucidation of standpoints—exactly *what* the individual philosophers meant, rather than *why* they meant it (what motives they had)—then one must critically scrutinize all inferences of the following kind: since he means *A*, which he explicitly states, then he means *C*, which he does not explicitly state. Many of these inferences will turn out to be untenable.

Furthermore, if one is predominantly interested in the internal relations and relative tenability of possible philosophical positions (here equal to noncontradictory positions), it is desirable in principle to consider all and-connections that may exist between the various theses of the philosophers and then examine and evaluate the non-occurrent combinations (including negations). Hence, if *A*, *C*, and *E* occur combined in history, as do *B*, *D*, and *F*, then we should consider the combinations (*A*&*B*, *A*&*C*, . . . , *A*&*F*; *B*&*C*, *B*&*D*, . . . , *E*&*F*; *A*&*B*&*C*, *A*&*B*&*D*, . . . , *D*&*E*&*F*, . . . , *A*&*B*&*C*&*D*&*E*&*F*) and also the negations (*A*&-*B*, -*A*&*B*, -*A*&-*B*; *A*&-*C*, -*A*&*C*, -*A*&-*C*, and so on).

On the whole, however, system-philosophical combinatorics is poorly developed. Interest has presumably been focused until now on views proclaimed by great personalities; but the intellectual pleasure of peeking into worlds with great internal order and harmony is not diminished just because no one has yet believed in their existence.

How can systems vary demonstrably with respect to concepts such as 'meaningful', 'analytic', and 'a priori'? One way to understand this is to take these concepts as points of departure; then their occurrences (and traditional translations and equivalents) are compared. A survey of precizations is developed as one clarifies, step-by-step, differences in usage. For these differences in usage one then postulates conceptual differences, inasmuch as one

assumes that conceptual characteristics vary in parallel with the differences in usage. Some such differences were clarified by Nordenstam (1972).

The precondition is to have a sufficiently broad common reference frame so that occurrence analysis and precization analysis can be given a common description. Furthermore, the words employed in the formulations of conceptual characteristics must also be shared. Closely considered, this turns out to be a radical requirement, because the less two systems have in common, the more difficult it becomes to map uses and conceptual differences in a tolerably neutral and complete way.

One difficulty in principle arises because the level of depth of intention (and the level of discrimination) cannot be assumed to be infinitely high. The history of the words used in classical logic and modern debate makes it probable that strong precizations become more or less transintentional. Hence, one gets constructed system differences rather than purely descriptive ones. Regarding the limits of precization, Anders Wedberg's (1958–66) offers helpful points of departure for a thorough study. He characterizes 'meaningful', 'analytic', and 'a priori' as "exceedingly problematic distinctions" (1958–66, vol. 3: 247). It is doubtful if any consistent transintentional precizations are sufficiently close to the original, vague uses regarded as plausible, even with very modest requirements for "plausibility."

The limits of precization inhibit system variability. If we view these precizations as branches of the philosophical family tree, they become small and stunted. Why? Perhaps because the imagination of researchers is hampered by the thought that only one concept can be the correct one and that it is to be found in the vicinity of concepts that have already been suggested in the philosophical literature.

Philosophical combinatorics is poorly developed because many imagine that truth is approached from only one direction, or, alternatively, that one searches for truth along certain lines and is impatient with others. Combinatorics presupposes a kind of *epoché* (withholding of assent or dissent in matters in which conflicting and equally plausible explanations exist) in relation to all attempts at solutions.

Introduction to Systems and System Comparison

Historiography of philosophical systems as total views involves problems of theoretical kinds that are rarely discussed. An attempt is often made to

introduce a system, for instance, writing a chapter on it and comparing it with a couple of others. It is sometimes uncritically assumed that the historiographer can start as a neutral being without presuppositions of relevance to the system he is going to introduce. This is scarcely possible because a philosophy of history is part of the system, dependent on epistemological and methodological views. Perhaps we must acknowledge that the introducer implicitly depends on his own system.

An introduction to a system may be conceived as leading from one system to another or from systemlessness to a system. In either case the introduction begins in territory foreign to the system being introduced. If the reader is already familiar with the system, no cognitive introduction is required. Instead, the introduction may take the form of a pedagogical arrangement or a condensed, explicit representation. In this section, however, our reader is presumed to be, cognitively speaking, an outsider, one who has no prior acquaintance with the system.

Presenting a system may seem easy as long as one does not perceive one's presentation as number $(n + 1)$ in the series. A researcher, however, cannot ignore previous attempts at presentation, and a study of these prior presentations will illustrate how different persons, generations, traditions, and movements have presented contrasting pictures of one and the same philosopher. The primary aim of the current system introducer, then, is to give a presentation that does not attribute to his predecessors thoughts that are alien or foreign to their philosophy—at least not in such a way that their own intentions become obviously distorted.

Nonetheless, a researcher who is fully aware of these traps *in abstracto* may still fall into them when it comes time for concrete formulation of a representation. Our main concern now will be to examine one more or less direct interpretation. The example concerns the Spinoza researcher Harold Foster Hallett, who begins the preface to his *Benedict de Spinoza: The Elements of His Philosophy* with some weighty sentences of which I shall quote the first two:

Probably no philosopher of repute has been worse served by his expositors and commentators than Spinoza. Monist, pantheist, atheist, acosmist, ethical nihilist, mechanist, mystic, and even dialectical materialist, are among the epithets more or less commonly used to describe and pigeon-hole a doctrine which, nevertheless, though neglected, misinterpreted, and deplored, has never been despised as a mere curiosity of philosophical history. (Hallett 1957: vii)

The modern Spinoza researcher faces a difficult task:

[I]t is not easy for the modern mind, steeped as it is in empiricistic modes of thought subsequently developed, to take up the intellectual standpoint from which alone the thought of Spinoza is intelligible. Yet, of course, until this has been accomplished, exposition and criticism alike are futile.

(Ibid., p. vii)

Hallett is, of course, correct in demanding that one not apply current natural science conceptions of causal connections to Spinoza's texts. The texts must be understood as much as possible in the context of the traditions that were a part of Spinoza's life, traditions that were dramatically different from "ours" today. Is this possible?

As I see it, it is impossible in practice for us today to see the world as Spinoza did and to find out exactly what he meant by his words, and to express this in the vocabularies of our time. Those who try very seriously through years of intense study arrive at differences of great interest. Exactly what did Spinoza mean by the term *causa* in the *Ethics*? What is the relation of the meaning of *causa* to the meaning of *ratio*?

When the goal is to search for more authentic interpretations of Spinoza, however, Harry Wolfson's comprehensive idea-historical *Philosophy of Spinoza* (1934) almost always proves helpful. Wolfson approaches Spinoza's concepts "cause of itself" and "essence that involves existence" through a study of Descartes, Anselm, Crescas, Avicenna, Averroes, Maimonides, Porphyry, and of course Aristotle. His approach may sound daunting, but it turns out that this long detour to direct interpretation permits one to ascribe to Spinoza a couple of very simple, but nevertheless fundamental ideas.

In the first place, God must be such that he has no cause. If God were caused by something, the very existence of God would then depend upon this causing agent, and this agent could at any point take over God's role. God would then be subject to the discretion of another entity.

Between this first thought and the next, that God is his own cause, one may imagine a broad realm of possible interconnections. Wolfson's presentation, however, if it is adequate, permits one to assume that there are no such intermediate links at all. That God is the cause of himself needs for Spinoza to have been nothing but a manner of speaking, another way of expressing what is a relatively simple thought: that God himself cannot have any cause.

A third thought arises when we compare this idea of God's causality to the idea of the "accidental" existence of objects of experience. For example, although we can give an exact description of a thing's nature, this does not necessarily imply that the thing must actually exist. Things must be seen as links in chains of causes or conditions. We assume that for a thing to exist, certain conditions must be realized. We must see the thing as a link in a chain of causes or conditions. We can reformulate Spinoza's concept of God in a new way: God's existence is independent from such conditions that must be realized. Hence, God exists purely as a result of his own nature. In short, God's nature hinges on existence itself; the nature of things, however, does not even necessitate their existence. They may be conceived to exist just as well as they may be conceived not to exist.

Wolfson's contribution here is not so much that he actually puts forward such an interpretation, but that he demonstrates its plausibility through his comprehensive review of historical material.

Hallett's basic formula for the essence-existence relationship is introduced as follows:

Finally, the relations and distinction of essence and existence in 'God or Nature', i.e. of the infinite indeterminate primordial potency-in-act and its infinitely determinate enactment or actuality, serve to determine Spinoza's account of the divine causality as *free* and as *immanent*, and being both free and immanent, as *eternal*. With 'God or Nature' essence and existence are at once identical and distinct as the indeterminate is identical with and distinct from its exhaustive determinations—a complex relation which is generally expressed by Spinoza in the form: 'The essence of God *involves* existence'.

(Hallett 1957: 27)

Some of the expressions that Hallett uses here and elsewhere to bring the modern student to an elementary understanding of Spinoza are neither Spinoza's nor the modern student's. Hallett interposes a third system between the two. A third system in itself is entirely reasonable, but his mediating system fails didactically. This failure owes, for example, to the expressions "potency-in-act" and "identical and distinct." The basic concept 'action' is introduced by the phrase "by 'action' is meant the *actualization of potency*" (ibid., p. 5).

"Cause of itself" is mainly explained by the following third-system passage:

[S]ubstance actualizes and manifests itself in the mode—it is the active cause, and the mode its enacted effect. Self-actualizing and self-manifesting substance is thus essentially real and intelligible as ‘cause of itself’, i.e. as creating its own actuality, exhaustively and eternally. The primordial Real is substance as infinite indeterminate potency eternally actualized as exhaustively determinate mode, and is thus self-existent, self-manifest, *causa sui*.

(Ibid., p. 12)

Ordinarily, we do not notice interposed third systems, but Hallett’s is so forbiddingly expressed that we notice what happens, and take umbrage. Hallett states that his book “is devised as a monitory preparation for deeper study of the philosophy of Spinoza” (1957: viii). He does not, however, mention that he presupposes a third system.

I will not deny that this third system has potential for bringing the modern student closer to Spinoza, but the system needs to be introduced before it can bring such help. As far as I can detect, Hallett has not been sufficiently concerned about this issue. A final evaluation of his introduction to Spinoza, via this third system, must therefore be postponed until this introduction to the auxiliary system is at hand. Hence, we must employ a fourth system. Or does Hallett have in mind those modern students who are already familiar with Thomas Aquinas?

In the case of “cause of itself” and “essence that involves existence,” Wolfson provides a shortcut, since he succeeds in giving the expressions a sense, at least on the level of everyday understanding, that is relatively easy for the contemporary student to grasp. A third system is therefore not necessary because the expressions employed are not likely to be perceived as outlandish.

A philosophical author who works with his system his entire life, as Spinoza did, and who constantly makes changes, tends to write in the form of an introduction intended for himself. Indeed, such an author can never be entirely inside his system. The author feels that he is on his way into something, into something that he both dimly perceives and incompletely grasps. The system is created on the way; it is always more of an intended than an actualized entity, a regulative idea, something not yet existing—a fiction in Hans Vaihinger’s terminology (1911). It is not merely a question of giving something already existing a coherent, written form; the purpose is not merely autodidactic. Spinoza’s *Ethics* was, after all, gradually modi-

fied from an essentially introductory style with numerous appeals to the reader, to a form in which the system was more self-contained—a ship in which all gangways were cleared away.

In writings by systematicians, written partly in an introductory style, one must recognize that a terminology, a conceptual structure, and a set of propositions and rules are employed. Moreover, these formulations may only gradually be ascribed the senses they are to have in the system. Furthermore, in the course of the presentation, the formulations may be criticized and even eliminated. The reader must attribute an outsider's meanings to the introductory formulations. He is then led in the direction of the system, and step-by-step the meaning is altered.

In principle, it should be possible for the author to let nothing be affected by what is presupposed or used in the first chapters of the introduction. Only when the introduction is completed, when the reader has been led into the system, can he assess what had a merely didactic function and what was to become an essential element of the system.

In a dialogue between a representative of the system and an outsider, the same introductory process must take place. If the representative of the system remains both in the system and in the dialogue, the result can easily be an esoteric style in the derogatory sense: the listeners who are already in the system can nod approvingly, while the outsiders have to find their own way. This approach seems to be employed more frequently in the Dionysian-Pythagorean milieu than in the logically and empirically minded philosophical milieu. Such an approach excels when the representatives of the system (1) avoid statements that they must retract when a more advanced step is reached and (2) can permit themselves to regard everything from the point of view of the system, even questions from outsiders. When the system approaches being total, outsiders' attempts to enter the system will be interpreted in the light of the author's views of the system representatives, so much so that they cannot find any way to accommodate an "antagonist" by going beyond their own system.

The purpose of these remarks is, in part, to lay the groundwork for examining the philosophical literature that focuses on evaluating the border between didactics and systematics in the writings of individual authors. A question arises as to whether Hume's formulations might have a purely didactic component. For example, Cassirer criticizes or comments on Hume's doctrine of the impermanence of the "I" by saying that Hume, in his own

formulations, appears to accept its permanence (by virtue of acting as the author of each sentence?). Might such a didactic element be precisely what Cassirer bases his criticism on?

Hume compares the mind to a scene in which perceptions appear and disappear without any other “I” than the scene. But how can Hume himself reflect on his perceptions?

How can the I in the form of pure “stage” for the perceptions, at the same time step out of its passivity and autonomously intervene in the play of the perceptions? How can we instead of merely receiving the content “reflect” on it and thereby put a new stamp on it? (Cassirer 1906–07, vol. 2: 386)

Hume’s response might be that Cassirer’s description of the “I” is not adequate. Hume’s way of writing gives an introduction. The picture employed, that of the stage on which perceptions appear, is didactically determined. As to Hume’s text about perceptions, this picture changes nothing about those perceptions.

How Hume would contrive to achieve a consistent answer, I dare not surmise. The essential point is that when Hume speaks about perceptions, and when he says that he has never come across any “I” outside the succession of perceptions, one cannot immediately conclude that there is an inconsistency. One should first consider whether Hume’s statements might be given an interpretation within the system or be defined as didactic statements, signposts for traveling into the system. The formulation “I have never found the philosophers’ stone” may be ascribed cognitive meaning without introducing a concept ‘I’ that makes the “I” something permanent. In a way, it is more reasonable that one will be less likely to find the philosophers’ stone as one’s “I” becomes less durable.

Cassirer may perhaps be blamed for having applied a neo-Kantian perspective to the “I” too early in his presentation of Hume. If one’s account is to be cross-systematic, one must try to give each system every possibility of maintaining consistency and totality. Whether a philosophically committed person can manage this is an open question; however, without commitment it makes no sense even to try to understand a single system.

Both Hume’s and Kant’s systems are pyramidally formed. From certain basic principles, rules, and methods the “rest” of the system emerges. Since Hume and Kant do not start with the same principles, rules, and methods, they do not place the same topics on the apex of the pyramid. The result is,

for example, that part of what Kant places on top is far down in Hume's pyramid. If we, perhaps unconsciously, use Kant's system (in weakened form) in our representation of Hume, we will see Kant as more profound than Hume. For we notice that Hume places certain things (for example, the concept of the stream of perceptions) at the top, but Kant places them somewhat further down in the pyramid. On the other hand, we do not notice that Hume goes beyond some of Kant's top-level concepts and sentences (for example, the existence of synthetic a priori sentences in Newton's physics).

The preceding is not meant as a criticism of Cassirer, but rather as an example of the difficulty or impossibility of creating an idea-historical crossing from one philosophical total view to another. Either one relies on the fiction that identical words have the same meaning for different philosophers, or one goes deeper than the purely verbal and accepts that the same words can have different meanings. In the first case, one obtains an easily intelligible but fictitious continuity or identity of problems. In the second case, resorting to the nonverbal, the comparability tends to cease. In other words, either one is forced up to the surface of the system, or one remains in the deep but employs a system in which both philosophies are forced together, with the result that, at best, only one will fit adequately.

The discontinuous transition from intimate understanding of one system to intimate understanding of another can be systematized by imagining a reader who is familiar with Hume's texts without knowing Hume's name, and is then given an English text of Kant to read but fails to identify Kant as the author.

Let a represent the first sentence of the text by Kant. The reader, H , interprets it in the light of his entire reading of Hume (including nonphilosophical reading, we presume). The interpretation may be symbolized thus:

$$(1) \text{Int}_H(a)$$

Where, in accordance with the symbolization outline in my *Interpretation and Preciseness* (1953: 115; a revised edition appears as volume 1 of the *Selected Works of Arne Naess*), (1) refers to the interpretation of sentence a made by person H , a reader familiar with Hume. It is naturally a Hume-systematic interpretation.

Upon reading assertion a , the reader is taken aback because it seems obviously erroneous or inconsistent with earlier texts. Let us assume that the reader guesses that a is written by another philosophical author and that he therefore does not take the “inconsistency” as an objection to the interpretation. He then reads the next sentence, b , and adopts the following interpretation:

$$(2) \text{Int}_H(b, S:a, \text{Int}_H(a))$$

Where (2) refers to a Hume-systematic interpretation of b in the context S with elements a and the interpretation $\text{Int}_H(a)$.

This proposition, however, clearly seems wrong or inconsistent with a , so the reader attempts other interpretations. He ultimately embraces another interpretation of b , which, nevertheless, recognizes that he was aware of and considered (2):

$$(3) \text{Int}_H(b, S:a, \text{Int}_H(a), \text{Int}_H(b, S:a, \text{Int}_H(a)))$$

which can be rewritten in shorthand form as:

$$(3a) \text{Int}_H(b, S:(b, S:a))$$

This proposition, while not obviously erroneous or inconsistent, still seems incomparable with the Hume texts. The reader now seeks a better understanding of a in light of (3) and finds a new interpretation of a :

$$(4) \text{Int}_H(a, S:b, \text{Int}_H(b, S:a, \text{Int}_H(a)), \text{Int}_H(b, S:a, \text{Int}_H(a), \text{Int}_H(b, S:a, \text{Int}_H(a))))$$

This situation can also be abbreviated to avoid making it completely unsurveyable:

$$(4a) \text{Int}_H(a, S:(b, S:(b, S:a)))$$

We now have a new interpretation of a in light of the “accepted” interpretation of b . This new interpretation of a may engender a rethinking of the

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“accepted” interpretation of b , (3), yielding an endless recursive process or a validation of (3).

These symbolizations refer to readings of only two sentences of Kant’s text. Unless “intuition,” with attendant certainty, rapidly brings the reader further, the interpretation complications become stultifying. On the other hand, one has no guarantee that the “intuitions” increase one’s understanding of Kant’s intentions. In any case, the eventual understanding of the Kantian view in question results from a very complex process that is never entirely free of the influence of Hume and that, perhaps, may never be completed.

What I have tried to convey, roughly formulated, is the problematic character of writing an exposition of a philosophical system in general and of an all-embracing one in particular and, furthermore, the even more problematic character of a cognitively acceptable description of the movement from one system to another, whether as systems or as part of a history of ideas. The problematic character of all this, however, does not necessarily impinge on the present and future cultural value of philosophical systems.

II

Comparison of Different Total Views

Common Sense, Ordinary Language, and *Lebenswelt*

When comparing different systems, one must first consider all explicit definitions. All defined words (all definienda) are omitted and replaced by their definienda. For example, when a philosopher employs the word *substance*, but defines it, there is every reason to replace the word with the definition in comparisons, because other philosophers can easily mean something different by the word; likewise, if a philosopher has theses about empiricism, but defines *empiricism*. Of course, the definitions will deviate substantially according to the positive or negative attitude of a philosopher. At the outset, one does not assume that words from the vernacular are used in any special sense that has to be taken into account in order to understand the system. (Exceptions may occur, however. Some philosophers—Heidegger, for example—apparently use everyday words in unusual ways.) In accordance with this, one also assumes that the comparer's everyday speech is neutral in relation to the systems compared.

A great problem can now be formulated as follows: is it feasible to make everyday words and sentences more precise so that a conceptual system results? If it is, all systems would contain a common core. However one varied the systems, the variations would never affect the implicit concept world of the vernacular.

An obvious possibility is to identify the supposed conceptual and propositional system of everyday speech with a commonsense world picture. The philosopher's task, then, is to seek a kind of clarification and harmonization of the commonsense world image, not to *correct* the commonsense world at any point. The notions of a common, changeless, universal human *Lebenswelt* also belong here ("the fourth Husserl," and others).

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On the basis of, *inter alia*, empirical, semantic investigations, I have concluded that words and sentences of the vernacular in everyday use have a depth-of-intention contour that does not permit one to make intra-intentional and plausible precisizations in such a way that specific systems of concepts and propositions emerge. The depth of intention in a philosophically relevant direction is insufficient.

Precisely this characteristic—that everyday speech is philosophically amorphous—makes it suitable for system comparison in its first phase. It allows for histories of philosophy; the historian can converse in everyday speech about philosophers and their different theories. The speech is not philosophically adequate, but then this cannot be the historian's goal either.

One of the most interesting attempts at catching everyday speech in philosophically relevant conceptual networks was carried out by Ottar Dahl (1956) in his investigation of historians' use of the word *because* and certain other related words and expressions. His efforts to catch such everyday speech in this case were stymied, especially so in the case of top historians—those who do not mix philosophy or other subjects into their historiography. The use of *because* among historians is neutral in relation to concepts of cause and doctrines of historical causality of different systems.

Our conclusion, as suggested, is that words and sentences of everyday speech have a depth-of-intention contour that precludes the emergence of systems of concepts and propositions from plausible interpretation. This conclusion contains an essential restriction: that it be a question of words and sentences of the vernacular in everyday use. In other words, a vague but important demarcation of a field of situations or use is indicated. By precisizing the conclusion in certain directions, we can, owing to this demarcation, push it into the analytic. It is indeed in those directions that we intend to go.

There is no possibility of delimiting an everyday language that can be specified and rendered independent from a kind of function or use that is one of "everyday" speech. In everyday speech, certain words and sentences are used in certain ways. The *same* words and sentences are used in other ways in other kinds of situations. The vocabulary of everyday speech is common to the systems, but this does not imply that there are certain concepts or propositions that are common. When we eliminate all technical words and technical sentences from a system formulation, we have a system that is expressed in the vocabulary of everyday speech, but we do *not* get a variant of common sense, a system within the "concept world" of daily life,

in other words, a *Lebenswelt* philosophy. David Hume and George Berkeley, for example, attached importance to avoiding technical terminology, but their philosophies do not therefore stand closer to common sense or *Lebenswelt*. Richard Avenarius employed an enormous contrivance of technical words and sentences, but he is at least as close to common sense or the *Lebenswelt*. In fact, he is much closer, I believe.

As a system is developed, the words and sentences of the vernacular acquire more definite senses in relation to a technical conceptual framework. The vocabulary of everyday speech is drawn into the system; it becomes precizable—more exactly, sender-precizable—for the system builder in a philosophically relevant direction. For one who seeks to “dwell” in the system—for example, to see life as much as possible *sub specie aeternitatis*—everyday speech is also infected. In a certain sense, “daily life” ceases to be daily life anymore for the philosopher, but he can make himself fully understood in daily life through everyday speech.

If two philosophers who have radically different systems put forward a series of mutually comports sentences in the vocabulary of the vernacular, this cannot be taken as an expression of mutually comports propositions. The difference in propositional content is difficult to express. The system builder has not defined the words of everyday speech.

In view of this, it is not strange that the system builder finds it difficult to be understood and that it sometimes seems to him that neither critics nor adherents understand him. Edmund Husserl has stated, “All the criticism that I know of has missed the basic idea in my phenomenology by so much that it has not been hit at all, in spite of the quotation of my words” (Fink 1934: Husserl’s foreword).

In a letter to Karl Löwith in 1937, Husserl adds:

Maybe you understand that Scheler, Heidegger and thus all previous “pupils” have failed to understand the true and deep meaning in the phenomenology—in the transcendental phenomenology as the only possible one—and how much that depends on this meaning. (Husserl 1959: 50)

Buytendijk says in his assessment of Husserl’s importance for modern psychology:

One would be mistaken if one considered the rapidly growing frequency one can observe of the words *phenomenon* and *phenomenology*, *intentionality* and *act*,

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Lebenswelt, put in parenthesis, essence-view and intuition in psychological treatises as a clear sign of an immediate and decisive influence from Husserl's side on the development of psychological thinking and methodological principles.

(van Breda and Taminiaux 1959: 78)

For the person comparing two systems, the preceding is of great importance, because the composer himself ordinarily seeks to describe the systems precisely in the vernacular. He seeks to express the systems "neutrally," hence either in everyday speech (so he thinks) or in a third system-language.

The vernacular used by a systematician who speaks within the system, and hence is not concerned with leading someone into the system, is *not* neutral. Words and sentences have meanings that can be precized within the frame of the system. They are not precized in the systematic account, but they may be made precise. They are intersubjectively precizable, but not sender-precizable for the systematician himself. When leading others into his system, the systematician can precize what he means for them.¹ To himself he cannot precize, however, if he is completely clear in his system and the system is fully constructed.

Comparison and Evaluation on the Basis of Adequate Presentation

Human beings are evaluated by each other. We accept this or that declaration as an expression of person *A*'s opinion of person *B*, regardless of how little *A* knows about *B* and regardless of how superficial and indirect the connection between *A* and *B*. We do not say, "You have not evaluated *B*, because you do not truly know *B*, the whole person *B*. You have merely seen *B* mentioned in a newspaper and you evaluated something that you more or less associate with *B*'s name. It is not *B* you have formed an opinion about, it is a fictitious person."

Usually we do not demand that a comparison and evaluation be performed on an adequate basis. The opinions of persons *A* and *C* about person *B* are independent of how *A* and *C* define *B* or indicate *B*'s essence (in Spinoza's terminology). In daily life I can say, "I do not really know *B*, but I am entirely opposed to the ordinary opinion of him." *Of him*. As if one had been in contact with the whole person and reacted to the whole person!

If we now conclude that we almost never compare and evaluate—we only think we do—then there is little left of what we usually do. Suspicion arises as to whether it is not merely misunderstandings (mostly philosophical) that have led us to criticize the modes of expression of daily life. From this viewpoint we are tempted to postulate that we *can* compare, and that when *A* and *C* disagree about *B*, it is precisely about *B*—an identical, common object—that they differ. That *A* and *C* do not agree at any point about the representation of who *B* is, apart from his name and elements of his appearance, cannot have any relevance.

If this commonsense attitude is applied to philosophical systems, we are liberated from the problem of whether presentation and evaluation are possible—since they are frequently done, they are of course also possible. Everything that *is*, is possible. In Spinoza's own system, precisely *his* standpoints have been represented and evaluated in different ways owing to differences in the people who encountered them.² Whether or not Spinoza's system is total makes no difference, according to such an attitude; one expresses one's own opinion and likes or dislikes about Spinoza. The depth of intention does not need to be greater than that of a mouse that nibbles on books by various authors and also has its own likes and dislikes, based on their paper and binding—a basis sometimes also employed by the highest mammals.

When we realize that this is the case and always has been the case, we become incapable perhaps of imagining clearly what it might mean for things to be different. How could clear but unsatisfiable criteria of representation, comparison, and evaluation have originated? Most likely thought ran amok. It turned to idling. The evidence of this is that when such comparisons and evaluations are consistently carried out, they frequently in practice lead to unacceptable conclusions—conclusions one will not accept.

It is healthy to keep in mind this commonsense or naturalistic attitude when the speculative and reflective consciousness leads to results that are peculiar or paradoxical, to put it mildly. An attitude, however, has no direct application to a way of thinking. If one's train of thought forms a chain—if *A* then *B*, if *B* then *C*—it may be criticized by evaluating premises or deductions. Attitudes do not enter into the picture. Regarding the premises, many things are relevant—correspondence with observation and so on. For the rules of inference, a more limited, logico-semantic kind of criticism

is relevant. That the conclusion, C, seems paradoxical or absurd in a sense other than (logically) self-contradictory or false cannot be employed as an intellectual criticism of the train of thought.

If the conclusion regarding deliberations about systems is that no one has accomplished a comparison of *total* systems, it is not necessarily a valid counterargument to say that adequate comparisons have actually been undertaken. We must object to premises and rules of inference, including the semantic rules, not the least of which are the definitions of *comparison*. If the definition of comparison is such that it requires something other than, or much more than, the demands that are generated from precization in at least one plausible direction, then one can perhaps on a semantic basis construct arguments against its feasibility.

If we are to list requirements for the adequate comparison of systems, we can learn from the basis of comparison employed in responsible art criticism. A critic must hear the entire piece of music — know the score, be present, and be seated in a location that is satisfactory acoustically during the entire performance. With regard to paintings, the critic must see the entire painting. Here, then, it is not the same as when personalities are compared in daily life.

To present Spinoza's philosophy to an audience (for example, oneself) requires more than just copying and rendering it, in Spinoza's words, one by one. It involves a communication of the thought-content, at least in its essentials. Hence, if one of Spinoza's main points is that "Everything that is, is either in itself or in something else," the representation must impart precisely Spinoza's intent regarding this point. If one does not know about the formulation, does not understand it, or does not include it in the presentation, then the presentation will not be adequate. If one does not include any other main point either, it might be proper to say that the presentation "is really (actually, strictly speaking, indeed) not a presentation of *Spinoza*." In examinations in philosophy in which a student "mistakes" one philosopher for another, we say that he did not "give a representation of Locke, even if he explicitly speaks about Locke all the time."

From the preceding, it emerges that an adequate representation of a philosopher's doctrine must be based on hypotheses about how the philosopher himself understands his individual sentences and their connections. An evaluation of a system must be presupposed to be an assessment of the thought-content intended by the system's author. Hence, when comparing

two systems, the comparer must collate two sets of thoughts. It may then *perhaps* turn out that the two doctrines that one assumes answer the same questions actually have no common reference points, meaning that they, strictly speaking, have no single object or theme in common.

I have emphasized “perhaps” because it is likely that the fewer points of contact there are, the greater the differences the metasystematician must catch in his net. Against this, we might object that the two philosophers may well have ways of viewing things that differ only in small but characteristic nuances. The world and self may have a somewhat different coloring for the two, or a somewhat different distance. To one of them, everything may seem nearer than to the other, and the proximity is not perceived as a property of the relation between things (entities), but as an essential characteristic of the things. If something like this can be the case, the two philosophers perhaps have no object in common, since the distance accent differs and applies to everything.

Even if different philosophers use the same words, we must be careful not to assume or postulate the identity of a basic concept or object just because it is expressed or designated by the same word. The question of identity is something that must be demonstrated independently of the similarity of words. *Being* and *existence* are examples of words whose different usage by various philosophers has led to a belief that the philosophers make statements about exactly the same thing. The statement “God is, but does not exist,” for example, is interesting because it directly employs the subtly different meanings of these words.

Discussions about the concept ‘substance’ sometimes conceal the belief that one is referring to a common fundamental concept or an object that has been common to philosophers for centuries. Do Descartes’s, Spinoza’s, and Leibniz’s so-called doctrines about substance have any identical object in common? Or is the occurrence of the word *substantia* confused with the occurrence of a common concept ‘substantia’ or with a common entity called substance?

It is essential to note that the occurrence of a common word, *substantia*, does not imply common use. The word can have common functions, such as expressing a fundamental ontological concept, but such functional similarities may serve as much to mark differences as to mark conceptual similarities. An assumption that a common use is at hand is a type of synonymy hypothesis, and it must be treated as a hypothesis.

That Spinoza and Descartes do not have the same substance-concept is the most plausible working hypothesis in view of their own definitions of *substantia*. Their definitions are very different. Descartes defines it as “Everything in which there resides immediately, as in a subject, or by means of which there exists anything that we perceive—for example, any property, quality, or attribute of which we have a real idea, is called a *Substance*” (Descartes 1931, vol 2: 53). Or more precisely, “Any thing, *A*, in which something, *B*, inheres immediately, as in an object—or by virtue of which something, *C*, that we perceive, exists—and this, *B* or *C*, is a property or quality or attribute of which we have a real idea, any such thing is called substance.”

Spinoza’s definition, on the other hand, is: “By substance I understand that which is in itself and is conceived by itself: that is that whose concept does not need the concept of another thing, of which it must be formed” (Spinoza 1914, part 1: 1).

When the definiens formulations of two thinkers are as different as they are in this case, the presumption is that they express different concepts. The burden of proof lies with the one who claims that the concepts are identical. Furthermore, it is easy to believe that the term *substance* in these cases denotes something different. This assumption, however, may be a mistake, because two different concepts may have the same extension.

Evidence that Descartes and Spinoza have different concepts for substance emerges with the first suppositions that they derive.³ Descartes concludes that the soul (*mens*) is a substance, while Spinoza finds that it is not a substance at all, but a *modus*. We can accept both derivations as long as we also accept that the two philosophers have different concepts for substance. If we hold that Descartes and Spinoza have the same concept (if so, which one?), it becomes difficult not to reject at least one of the derivations as erroneous. Hence, we must say to either Descartes or Spinoza: “The soul in the concept of substance that you hold, but have not defined yourself, is assigned an ontological status opposite the one you say it has. You have unfortunately made an error in your derivation. If we correct it, you will see that your philosophy is actually somewhat different from what you have thought.”

The preceding account of the difference between Descartes’s and Spinoza’s systems with respect to the possible concept or object expressed or denoted by the common word *substantia* is written within the frame of a se-

mantics in which the Semiotic Triangle is employed. The account might also have been written in a somewhat different frame. The more an account is made precise, however, the more a metasystematician must make use of a system and, furthermore, the more one's conclusions cannot be identical under variations of this system. Hence, the investigations with points of departure in Descartes's and Spinoza's texts will themselves be perceived differently when the semantic system is varied.⁴

For the metasystematician, it is easier to compare two systems based on the assumption that the two systems have at least one fundamental concept in common, rather than that they have no concepts in common. The latter, however, does not make his work impossible. It is much worse if the systems do not have a single rule in common and hence have no rules of inference—implicit or explicit—in common. This is particularly difficult since the comparer himself has a set of rules whose terms he must use to understand the authors: that is, he must ascribe and acknowledge understandability to them in terms of his own basic rules.

The distinction between the three classes—adequate representations, inadequate representations, and nonrepresentations—can, of course, be subjected to a critical examination. One may object to setting the requirements of adequacy too high in relation to a plausible development and sharpening of the requirements of daily life, but the concepts I have suggested can hardly be challenged in that respect. One must distinguish between principle demands of adequacy on the one hand, and practical requirements in the spirit of compromise on the other hand. The latter cannot replace the former without misconstruing the goal of representations.

Comparison with Respect to Truth-Value

When we compare two standpoints with regard to truth-value, we may definitively conclude that both are true, that both are false, or that one is true and the other false. (The result may be definite without necessarily being *certain*.) If the standpoints are not comprehensive or not very deep (merely scratch the surface), the result of the comparison may be justified by referring to a set of statements that we immediately accept as true. Such statements then acquire the character of standards and paradigms. The standpoints are confronted with the paradigm set and accepted or rejected depending on whether they agree or disagree with that set.

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Already in such simple comparisons, then, it is obvious that a “third system,” a basis for comparison, a *tertium comparationis*, is implied. It provides the standard of truth or falsehood against which each of the statements is judged.

Is it different with measurements of length? Apparently not. Instead of quantitatively comparing the length of two lions by means of a yardstick (as a third “system”), we can put the two lions side by side and see directly which one is longer. The standpoints that “Lion A is longer than B” and “Lion B is longer than A” are compared simply by looking at the lions. If the lions are put in their position by competing hunters, a certain minimum of apparatus may be necessary. For example, we might need to place some straight sticks perpendicularly to the longitudinal direction of the lions, so as to establish a “zero point.” We then need to establish a set of rules with regard to the manner of estimating the longer lion, a criterion for deciding that “x is longer than y.” The criterion is not a part of zoology; it is brought in from outside.

If we arrive at contrary results, we must either examine the statements of the hunters for self-contradiction, or perform another measurement that then constitutes the standard, the frame of reference. Hence, a system is created if there is not already one at hand.

We may conclude, then, that the isolated comparison of two statements with respect to truth-value is not possible, if by “isolated comparison” we mean that no third statement at all is introduced and accepted in advance as true. Furthermore, a methodology is presupposed whereby one can *find* truth-value, and possibly probability, tenability, or other qualities in case one finds that truth cannot be found immediately.

Limits of Precision and Depth in Comparison

Let us imagine that a serious debate develops in which one person, Peter, justifies his standpoint by claiming that it is tested in his way, and another person, Paul, justifies his own standpoint by saying that it is tested in another way, Paul’s way. Peter justifies his point in his way, and Paul justifies his in his own way, and both reject the other’s justification.

The chains of justification come to an end, and if there is disagreement about the deepest links, the question of truth must remain open. The chains may perhaps be continued, and the participants may change some of

their opinions, but insofar as there is disagreement about the underlying theoretical principles, the question of truth is undecided. The deeper a dis-sension lies, the more slender the basis for a conclusion other than "undecided."

Something else also becomes apparent when standpoints are justified deeply: it becomes more and more difficult for the opponents to understand each other. Peter says, for example, that in the final analysis such and such is clearly evident. Paul says that something contrary is clearly evident. Then they offer long lists of evidence justifying their claims. Different philosophers have great difficulty understanding each other when the discussion takes such a turn. The result is that not only is the truth-value of their standpoint questioned, but also the very meaning of it. For example, one party does not understand *exactly* what the other means by "point at," "show that," "explain," "mean that," "believe that," "reply to," and so on.

Deep differences arise when it appears that Peter and Paul adhere to different concepts of truth. Peter thinks that truth and falsehood are something different from what Paul says they are. This may affect all statements they make, including anything they say about what truth and falsehood are.

I have spoken until now about Peter and Paul and the possibility that their world pictures are so different that comparisons of truth-value and meaning from their respective standpoints are not or cannot be undertaken. For each party, the other's system is false or meaningless. From the standpoint of a third party, however, the "Ashlad," this conclusion is highly suspect. Does the Ashlad understand both Peter and Paul? Does he have a system doctrine that does not presuppose either Peter's or Paul's special premises, but instead is neutral and wide enough to formulate the opinions of the other two? If the Ashlad does manage this, it is natural for him to say that Peter and Paul are each right in their own way. Their systems are relative, and thus truth is relative. This, however, leads us into a blind alley.

Two reasonable possibilities exist. Either the Ashlad's thinking goes deeper and thereby makes Peter's and Paul's opinions comparable with respect to truth and meaning, or the talk of relative truth no longer applies in this case. Peter and Paul have gone to the bottom and represent (nearly) total systems; in this case, the Ashlad cannot stand outside.

It is pertinent to ask how it is possible to speak about the difficulties of Peter and Paul. The answer lies in the vagueness of everyday speech and our

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limited depth of intention. We do not need to have definite, sharp thoughts to obtain a feeling of understanding, but if we try to think as precisely as possible, we become more and more dependent on increasing both our depth and our breadth. In such attempts, it becomes clear that the Ashlad cannot describe what happens with Peter and Paul without speaking in terms of a definite system himself. His neutrality ceases, and with it the decidability of the questions of truth and meaning that Peter and Paul cannot decide. The Ashlad himself becomes hopelessly involved and has merely complicated the matter: we are left with an undecided debate among three parties instead of two.

III

Metaphysics as Exposure of Presuppositions

Collingwood's Concept of Presuppositions

The points of view of English philosopher and historian Robin G. Collingwood in relation to metaphysics warrant consideration.¹ Under the designation "metaphysics," he classifies Aristotle's first philosophy (*prima philosophia*), or doctrine of principles, and the traditions issuing from it, as well as the great system formations, regardless of their affinity to Aristotle.

Implicit in Collingwood's writings are (at least) three concepts of metaphysics (cf. Rynin 1964: 330):

1. "x is person *P*'s metaphysics," which means that "x is the set of (absolute) presuppositions in *P*'s systematic thought."
2. "x is *P*'s metaphysics," which means that "x is a clarification and representation of *P*'s presuppositions within *P*'s own systematic thought."
3. "x is a metaphysics," which means that "x is a text that presents what person *P*₁ hypothetically assumes to be person *P*₂'s presuppositions in *P*₂'s systematic thought."

Collingwood's concept of 'absolute presupposition' is interesting and valuable regardless of how one intends to connect or separate metaphysics and system theory.

David Rynin gives a condensed, nine-point characterization of presuppositions as Collingwood construes them. The following are his first six points (Rynin 1964: 308):

1. They lie at the base of systematic thought in the sense that by virtue of their logical efficacy they and only they enable its questions to arise.

2. They are not themselves answers to questions.
3. They are expressed in the form of declarative sentences, thus giving rise to the feeling or belief that they are genuine propositions, true or false.
4. They are not verifiable or falsifiable.
5. They cannot be undermined by experience, being the yardsticks (in some society) by which experience is judged.
6. They are not true or false.

According to Collingwood, "Every event has a cause" is a presupposition in relation to Kant's system, but not in relation to all later systems. Other examples of presuppositions include "There is a God" and "Space is infinite." Within statements such as these that articulate presuppositions, we can find concepts about 'God', 'existence', and 'cause' that function as basic concepts or categories of the entire system. They alone permit questions within the system to be posed and answered (Rynin 1964: 310–11). Presuppositions, however, also give rise to new questions; they generate questions, but they are not themselves replies to (living) questions posed by the systematizer himself. This is true by definition for Collingwood's concept of presupposition (Rynin 1964: 310, 315, 328). Presuppositions are not statements in the ordinary sense, but rather expressions of the deepest faith—they are symbols that seek to express something that lies beyond complete understanding.

A person may be mistaken as to what his own presuppositions are (Collingwood 1940: 31). In other words, if *F* is a presupposition in relation to system *S*, then *F* is not part of *S*. Hence, one cannot speak as if there are presuppositions within or in a system. This point, however, is questionable and is discussed on pages 76–79.

Collingwood gives a concrete example of a presupposition in relation to a discipline. A physician, for example, determines the cause of a disease, and Collingwood asks why it must have a cause. The physician replies, "Because everything that happens has a cause," and he adds, "That is something we take for granted in my work. We do not question it. We do not try to prove it. It is not something that someone has discovered, the way it is with bacteria or the circulatory system of the blood. It is something that we simply take for granted." Collingwood responds, "He tells you that it is a presupposition in the branch of science in which he works" (Rynin 1964: 329).

The physician regards himself as a member of a specific profession with limited tasks. It is not difficult for him to understand that in other professions there may be other presuppositions, and he can, without any particular difficulty, articulate some of his own. In the case of philosophical systems, the situation is entirely different, since they encompass everything.

According to Collingwood, clarification of general presuppositions is of central concern not only for occidental, but for all cultures. The fall of Rome was, in the final analysis, attributable to the fact that the Romans lost metaphysical clarity, that is, clarity about the implicit presuppositions of the Roman civilization (Rynin 1964: 309).

A theory on the variation and constancy of civilizations seems to be implicit in Collingwood's concept of presupposition: If *A* is an absolute presupposition in relation to civilization *S*, then another presupposition *B* cannot later (or earlier) be so, if *B* is logically prior to *A*. If *B* is "There is one and only one space," *B* permits the question "Is this space finite or infinite?" If, on the other hand, the question "Is there one and only one space?" is posed, it is, according to point 3 (Collingwood 1940: 152), no longer posed in the civilization *S*, even if the persons involved might conceivably be the same. A new civilization *T* would be emerging, according to Collingwood's definitions in Rynin's versions. Likewise, if civilization *T* had "There exists one and only one space" as a reply to "Is there one and only one space?" and "Something exists" was an absolute presupposition, but a question in the manner of Gorgias was asked, "Does something exist?" then civilization *T* would be exceeded.

Not All Sets of Presuppositions Are Equally Acceptable

According to Collingwood, not all sets of presuppositions are equally acceptable. Conceptual tensions arise within a system that depend on the nature of the system's presuppositions. These tensions, which do not have to do with truth or falsity, may be of varying strengths, and if they become too strong, the system breaks down. This is the subject of some of the last points in Rynin's condensed characterization of Collingwood's views on presuppositions (1964: 309):

7. In a given system of thought they occur in clusters, no single absolute presupposition suffices to do the job.

8. To the extent that the presuppositions are not comparable, i.e., cannot be conjointly held without giving rise to inconsistencies and confusions of thought, they generate strains that, if sufficiently great, eventually lead to the collapse or alteration of that system.
9. Some clusters of absolute presuppositions exhibit fewer strains than others and are consequently better able to do their job of underpinning systematic thought.

Two Concepts of Presuppositions: One in Cognitive Daylight and One in the Twilight Area

Rynin is well on his way to accepting Collingwood's concept and doctrine in spite of being a logical positivist. To quote Rynin, "[I] am, I think, the only living logical positivist, or at least the only one who, as far as I know, is willing to be called this—provided that I explain what it means to be so" (1964: 331).

Rynin's strongly positive attitude is valuable and remarkable because Collingwood's doctrine is easy to attack, and Rynin is one of America's most merciless philosophical critics. Here I will not undertake a general analysis, but rather seek to clarify some sensitive questions.

It appears from Collingwood's examples that a philosopher can have a well-articulated presupposition expressed by an ordinary kind of formulation such as "Every event has a cause." Spinoza's first axiom, "All [things] which are, are in themselves or in something else," is a good example of a principle that is well articulated as a formulation and that the systematician has not at any point sought to go outside, as far as can be judged on the basis of our sources. The distinction "in itself/in something else" stands for Spinoza at the absolute limit of thought. It seems that the axiom does not emerge as a result of pondering the question "Are all things in themselves or in something else?" or other questions that the axiom then answers in good order. (With regard to Kant's "Every event has a cause," the case is more complicated since, at least when reading Hume, he must indeed have posed questions that "lie underneath" this formulation.)

For principles of the kind to which Spinoza's first axiom belongs, it is hardly the case that the author—or more correctly, the creator—is not convinced of their truth. They are true and self-evident for the system builder. If presuppositions are by definition beyond truth and falsehood,

and one uses the creator's intention as a base, then these kinds of principles are not presuppositions in Collingwood's sense. It seems that one needs two concepts of presuppositions, one on a level with what I have called basic sentences or basic rules *in* a system, and one that might be located just outside (before, under, or behind) the system, in a cognitive twilight area. Whether the latter can at all be constituted as a (tolerably precise) concept is something that I shall refrain from making a final decision about, but it is well worth the effort.

"All is connected," "There must be a God," "The world is evil," "Man is a nothingness," "I must not give up," "The individual human being has infinite value"—such exclamations or maxims are employed by many and can perhaps on particularly serious occasions exemplify the level on which the presuppositions that are "deeper" than the clearly formulated principles lie. It involves a matter of pronouncements with an expressive function approaching the artistic-expressionistic one. One is involved with all one's heart and has no energy left for a cool analysis. The pronouncements function as justifications in the sense that they put an end to further questioning or doubt. They cut short the discussion and clear the ground for getting on with life. One can then "proceed." Therefore, it seems almost quaint if one has misgivings, analyzes a bit, and tempers oneself: "*Almost* everything is connected," "Maybe there must be a God," "The world is almost or completely evil," "One might think man is almost a nothingness," and so on.

In the case of such exclamations it is obvious that the question of truth is not always relevant. "*Is it true* that there must be a God?" This question seems premature. Indeed, we *must* presume that there is a God! Alternatively, one enters the twilight area: "Yes, because otherwise everything becomes meaningless." If one then asks, "Why shouldn't everything be meaningless? Perhaps everything *is* meaningless?" and continues along these lines, one will most likely dig too deeply. The person who says "There must be a God" is no longer concerned; the questions are not living issues for him. The justification that is given for having to presume that there is a God is not an attempt to justify the truth of the "postulate." A compelling motive is indicated. To the one who does not feel it as a compelling motive, there is nothing more to say.

If we proceed from nonphilosophers or amateur philosophers to the great system builders, we are likely to retrieve such presuppositions from the twilight area. Some are explicit, but most are implicit.

In Spinoza's system, an identity seems to be presupposed between existence (as a human being) and perception (cognition), so that by realizing (knowing) something, one in a way "gets into" the things themselves—a form of spiritual occupation. Historians in this connection often quote the famous seventh proposition in the second part of Spinoza's *Ethics*: "The order and connection of ideas is the same as [is *identical* with?] the order and connection of things." This proposition, however, has been derived from something else *within* the system and does not concern the identity that is assumed "prior" to the system.

Collingwood points to a consideration of the basic concepts as a means to trace presuppositions. In the case of the twilight zone, this is clearly important. We can in a meaningful way ask about the justification of the distinction between God and non-God. What is it that justifies such a distinction? Spinoza presupposes that the reader of the *Ethics* comprehends the distinction between being in itself (*in se*) and being in something else (*in alio*). It is indeed possible that he has not allowed for the prospect that someone who knows the history of philosophy as he does will declare "I see no difference!" or "I cannot comprehend the difference!" Here is perhaps a basic structure in Spinoza's thought (founded, in this case, in a tradition in which he himself stands) that for him is "prior" to the system, in the sense that he uses it without asking about the justification for doing so.

Could Spinoza, however, have understood a question posed to him about the justification of the distinction? It is difficult to see why this would not be the case. We may imagine the question being posed by followers of Ockham, or another tradition in part coterminous with Spinoza. We leave open the issue of whether there are common presuppositions of such a kind that the one who uses them will in principle not be able to understand them without thereby rejecting something that is asserted in the system. If Spinoza had been able to give an introduction and justification of the distinction "in itself"/"in something else," what he justified in terms of it might have been grafted to the system as it now exists. The pyramid has acquired a new step.

Principles Within a System and Presuppositions of a System

If we re-examine Rynin's condensed characterization of presuppositions (pp. 83–86), we find that it suggests two concepts: principles within a system and presuppositions in relation to a system.

The category of principles applies to point 1, but we must add the proviso that it must be identified as basic to the systematic thought *within the system* it concerns. Point 2 applies similarly, but here too “within the system” should be added. Point 3 falls into the “presupposition” category, that is, sentences that are not principles but are nevertheless in the cognitive twilight area. To point 4 we must add “in relation to the system.” It is doubtful but not impossible that a principle in one system is a verified or falsified sentence within another, more comprehensive or profound system. To point 5 we must also add “in the system.” Point 6 applies to presuppositions and must perhaps be supplied with an indication of a relation: “They are not perceived as true or false (by the one who presupposes).” It may happen that one person’s presupposition is another person’s principle—for example, “We *must* believe that there is a God.” Points 7 through 9 are characteristic of Collingwood’s system theory. The question of their tenability provides a program for philosophical research.

These additions, which make Rynin’s characterization more explicit relative to a specific system, may well do Collingwood an injustice. In some places it seems as if he also has nonrelative concepts in mind: that it is a question of “absolutely absolute presuppositions” and not merely “absolute presuppositions relative to a system.” It makes sense for one who considers various historically existing systems to ask, Are there principles and presuppositions common to them all, and are there any that must exist in relation to any conceivable system, indeed to any form of systematic thinking at all? Are there presuppositions that make systematic thinking *möglich überhaupt* (feasible in general)?—are there necessary conditions of such a cognitive kind that they may be called presuppositions?

Collingwood’s main contribution lies in his studies of historically occurring systems and in his appeal for serious study of these systems—with the aim of understanding the foundation of one’s own civilization. He wants a “Know thyself!” approach to be applied to the study of civilizations—but a “Know thyself from within!” approach, not a study from the outside, such as a causal or natural science investigation. For Collingwood, this study is possible only by seeing one’s own system in contrast with that of others.

As a historian, Collingwood has no use for a philosophical system concept that emphasizes explicitness. Nor can he have any particular predilection for the special systems of outstanding, atypical personalities such as

Aristotle and Spinoza. It is, therefore, not to be expected that his system theory is adequate for the study of philosophical systems. True enough, the definitional statement here (pp. 11–12) can be made to cover Collingwood's system, but attempts to make it more precise will go in a direction philosophically that is not a historically important one. The main application of Collingwood's doctrine should be sought by historians and cultural anthropologists.

Collingwood's Metaphysics Presupposes a Supersystem

As long as we are sure that the systems created by others are inferior with respect to the breadth and depth of their conceptual structure, or at least not superior, then Collingwood's metaphysical program is sensible for exposing the presuppositions of all the other systems. In this case, one takes for granted that one's own methodology is entirely sufficient for comprehending another system. One will not take seriously the possibility that the other system might have a methodology, a doctrine of interpretation, exposition, textual analysis, observation, verification, and justification that in part conflicts with one's own. One will seek to clarify and explain all difficulties in terms of one's own system and will apply one's own methodology to the foreign system. If that system says something that does not agree with one's own, it is noted as a weakness, a lack of clarity, or an error in that other system.

Collingwood's idea for comparing the relative intensity of tensions within different systems can seem realistic, however, only if we imagine ourselves as supersystematicians with superior instruments of study. We must see clearly the highest goals of the others, but still perceive them as groping and stumbling. We must see how they stretch their arms out to reach what we have already attained, while recognizing that they will not quite succeed in achieving it. Others are led by conflicting wishes; their assumptions lead to internal contradictions; and they struggle to become clear about what happens to them, but they can never reach full clarity.

Throughout this, the system comparer does not consider the fact that he himself operates with a certain methodology and a concept of truth. If the latter requires something to be true, that it correspond with reality, his own conceptions of what can be real immediately become relevant. For example, if a system accepts the objective validity of norms, it must be possi-

ble that there is a reality of norms and a "world of values." Do intersubjective accessibility and temporal permanence belong to that which one must require of reality? Or is a Heraclitean reality conceivable, in which everything is in flux — without a logos in the background?

To conclude that "This proposition (perhaps this principle) does not correspond with reality," one must hold (at least implicitly) a theory about what signs indicate that a proposition agrees with reality. Does one rely on evidence criteria, sense experience criteria, or logical coherence? What about the applications of the criteria by a particular person — taking into account his own specific background — to decide whether the proposition agrees or not? What kind of "personology" can we presuppose that a person brings?

Already in the indefinite plurality of possible truth-concepts and criteria lies a source of misinterpretation of the other's system. If the other's system goes deeper and also *presupposes* another methodology, one has no chance to comprehend, let alone judge, his system. Since very few people can, or have occasion to, clarify their philosophical-systematical position, most evaluations of a system's truth-value are without cognitive value. The norm "You shall take a position!" works as grandiose deceit. One makes up one's mind, but without knowing with which system one is aligned.² Especially as a historian, one can be led to understand a system more or less deeply by immersing oneself in it without ever developing clear standpoints in relation to the issues covered by the system. Such an immersion tends to lead to a favorable evaluation. When one has an absorbing and understanding attitude, one's own thought acquires the other's style. The result is an evaluation from the inside, and any potential criticism becomes immanent and therefore irrelevant for the system as a whole. The evaluator has become a representative of the system, and therefore his potential to familiarize himself with other systems becomes almost negligible.

The original philosopher arrives at his standpoint through much labor. His pronouncements acquire their status via ongoing comparison with other standpoints. If the opposition disappears, the system's cognitive value also disappears. It loses its punch and has no personality. What remains is simply a terminology.

IV

Can There Be, Ultimately, Only One Valid Total System?

Are Total Systems Identical After All?

“Fundamentally, all philosophers agree” (Durant 1938: 185). Perhaps all controversy, all attempts by new generations of philosophers to assert oppositions to the old, is merely a gigantic example of pseudo-disagreement. In other words, perhaps the appearance of opposition lies in the fact that systematicians have not been able to “translate” among different styles of expression or different languages and that this has led to polemicized characterizations of each other’s work. Another possibility is that they *have* understood each other’s work but simply (1) assert that only one *style* of expression is correct and (2) believe that argumentation must be formulated as if disagreement concerns fact and not expression.

Felix S. Cohen (1939) argues for a principle of “logical tolerance” within philosophy and other disciplines. He supports the use of “transformations” that permit “every statement within a system to be transformed to a statement within any other equivalent system.” About the applicability of this principle within philosophy he says: “two philosophical systems may be so restricted in scope and so far apart in origin as to have no content in common. But increase the diameter or scope of the two systems and you have an increasing common content . . . [and there is] no content in either system which can not be reached and included by an extension of the other system” (1939: 61).

Cohen maintains that the content of a statement is, to a considerable degree, a function of the system in which it occurs. Expansion of the system’s scope should therefore affect the content of the system’s original statements. This is an important point with which I fully agree. Cohen, however, draws dubious conclusions from this point. When two, initially very different philosophical foundations are expanded to encompass the same

scale, the differences decrease and become less important; that is, the differences become less cognitive and more verbal. When the “diameter” is extended, the reservations that philosophers must add to their original propositions lead to a gradual elimination of real differences in cognitive content. Finally, the differences become purely ones of terminology (see Cohen’s discussion of Spinoza’s and Leibniz’s systems as, respectively, modified monist and modified pluralist systems [1939: 62]). Cohen maintains that the distinctions between monism and pluralism “do not constitute a true-false issue . . . [rather] these differences are differences of structure, of perspective, of emotive value, social symbolism, or practical usefulness, rather than differences of objective content,” meaning that they are not differences concerning what is actually asserted (1939: 63).

The Spinoza/Leibniz example fits Cohen’s theory tolerably well, but when the approach is applied to Spinoza and Descartes it does not fit. To some extent, the content of a statement may be regarded as independent from the system in which it occurs, but I would prefer to consider the expansion of the system as an extension of the original idea so that it can encompass (explain, cover) a steadily increasing spectrum of phenomena. In order to understand a statement within the system, one must approach the fundamental vision, the basic assumptions on which the system is based and by which it is expressed. This involves appropriating, to a certain extent, the original systematician’s fundamental view. Cohen, on the contrary, sees the fundamental idea as merely an accidental effect of one’s point of departure.

The task of describing the world may begin at one specific point, but all roads lead to Rome. The more comprehensive the system becomes, the more it becomes a description of Rome itself instead of the journey to Rome. When considered as descriptions of Rome, all systems are equivalent. If the philosophers were not so semantically “ill-bred,” they would agree that they all say the same thing. Some historiographers have suggested something to this effect, but most often only in a mitigated form.

If all historically developed systems are “after all identical” in the sense that *the same is said about the same thing*, we might expect that historical research would find its way to this step-by-step, by seeing through and correcting all terminological disagreements. Historical atlases might even be published to show in detail how the same things are covered by different concepts.

A somewhat more moderate theory would contend that the systems *in all essentials* say the same about *essentially* the same things. Incongruities in the systems might be the result of the authors' own errors in the application of common principles or rules of inference—as well as different inductive inferences or different working hypotheses.

Specialist works that take up the fundamental assumptions of the great philosophers at a relatively high level of precision do not indicate any concealed identity of these systems. The phenomenology of philosophy points as little toward one philosophy as the phenomenology of religion points toward a single religion. Speculations about a common primal foundation, origin, or source cannot change this conclusion, even if one takes a favorable attitude toward the possibility of a common *x* (for example, with respect to basic intentions). The special literature on Spinoza and Leibniz, and their mutual relationship, may uncover similarities in intentions, but this does not affect the conclusion that, when one considers the expression of their systems in their entirety, the two thinkers' images of the world and themselves seem profoundly different.

The conclusion that deep comparison of philosophies does not point toward a common propositional content, however, is problematic. The designs for phenomenology of philosophy, like phenomenology of religion, are themselves parts of systems, and therefore, to a greater or lesser extent, are influenced by one's own position. There appear to be rich possibilities for different systematic designs of a phenomenology. Hence our conclusion can only be a guiding general pronouncement at the level of everyday speech. Attempts to make it more precise produce the standard complications involved in a doctrine of systems.

The well-known Oriental parable of the elephant and the blind men's description of it is worth mentioning: some latched on to the tail and took it for the whole elephant, others grasped the ears, and so on. The descriptions complement, rather than negate, one another. They are all correct as far as they go, but they are all incomplete. It is part of the story, however, that the individual describers were reluctant to recognize the incompleteness of their own characterizations. They embarked on a discussion from their different, partial systems, from their different elephant parts, as if they had the whole elephant in mind.

This view differs from Cohen's in that it completely recognizes the degree of difference in cognitive content in the case of fully developed cogni-

tive systems, but justifies this by making an illicit analogy with mistaking one elephant part, perceived by oneself, for the whole elephant. Such a view is not compatible with an effort to compare Descartes, Spinoza, and Leibniz. It is no use saying that Descartes apprehends one part of reality (the ears of the elephant), Spinoza another (the tail), and Leibniz a third (the trunk). One who says this must be capable of knowing ears, tail, and trunk. Who is?

Reality is often said to be so rich that it cannot be apprehended by one person. Everyone is able to perceive *some* features, but no one perceives them all. The systematicians make generalizations from within their small sectors or their limited perspectives, and polemicize because they do not recognize their limitations. This viewpoint sounds most convincing.

Who recognizes clearly his own limitations? This notion leads straight to paradox. He is a great metasystematician who is able to make pronouncements about *all* the systems, who must be conceived to be free from the oppressive naïveté and narrow-mindedness of others. We have in the preceding, however, explained sufficiently why such a metasystematician can hardly be found. Perhaps there is such an overwhelming *Geist*, a Hegel of entirely new dimensions and perhaps far more difficult to understand than the Prussian philosopher king.

Back to our point of departure, though. Let us imagine that we have a common system up our sleeve, that we have at our disposal a kind of map on which we can draw all possible systems. We should then, in principle, be able to make surveys and comparisons of systems without having to construct any new framework or metasystem. It is of interest, however, to inquire on what basis one can *demonstrate* that two systems are in fact identical in their foundation. Let us suppose that a Cartesian god (a nondeceiving, omniscient god) has whispered in our ear that all systems are identical in basic categories. Do we have criteria that enable us to discover the identity? Can we demonstrate that systems have a distinction between 'being' and 'nonbeing' as (part of) their foundation? Do they share the principle of contradiction? As far as I can understand, we would only be able to accept the dictum of the Cartesian god without argument; we could not remain critically questioning.

The father of all occidental philosophical systematics, Aristotle, sought an ultimate, absolutely certain, and self-evident foundation for all knowledge, and thought he had found it. His fundamental distinction was be-

tween “that which is” and “that which is not,” and his fundamental ontological principle was that any and every thing either is or is not. A third alternative is impossible. The concept of truth is connected with this fundamental ontological distinction and principle. ‘Truth’ is introduced by a definition to the effect that to speak the truth is to say about *that which is* that “it is” and about *that which is not* that “it is not.” ‘Knowledge’ may then be introduced as *knowing* (having verified) about something *that is* that it is, and so on. Thereby, we are already well on the way to an Aristotelian system construction.

The possibility for non-Aristotelian systems arises with the “primeval intuition” to guarantee the meaning and truth of the fundamental ontological principle.

Can we imagine that *that which is* in a certain, particularly qualified sense, cannot be ascribed any well-defined quality or characteristic, but is beyond human structuring? If we pose this question, we approach the problems in Plato’s dialogue *Parmenides*, in which “that which is,” in the eminently qualified sense, is the One, and cannot be ascribed the property of being! The request perhaps involves an absurdity. If one thinks, one thinks of *something*, and then one does not think of something else; hence, the principle of identity applies, and thereby the principle of contradiction. Thinking involves structuring.

Aristotle can (of course) incorporate these doubts in his own system by saying that the one who proposes a theory of the unstructurable already presupposes that something is either unstructurable or not.¹ Thereby, the ontological statement is already implicitly accepted by the one who seeks to doubt it, but the doubter can then question Aristotle’s attempt at incorporation.

Of more practical importance for philosophical-historical expositions is the question “Is there only *one* concept of ‘being’, the one that Aristotle seems to intend—considering his entire *philosophica oeuvre* (not just the place where he speaks about the fundamental ontological distinction)?” Actually, metaphysicians operate with different degrees and kinds of being. Aristotle seems, in the beginning of the fourth book of the *Metaphysics*, to want a very wide concept, one that is so inclusive that *that which is not* also is (namely, in the sense that one can say of *that which is not* that it is not). Other metaphysicians, and Aristotle in other contexts, introduce, or seem implicitly to presuppose, narrower concepts. A limiting case is the one in

which *that which is* is identified with divinity, the One, the nameless, or that which is in other ways “highest” or “most” being. What does “being” stand for when being is graded and evaluated according to its kind? What is common to all degrees?

Aristotle’s truth definition can perhaps be made to cover all concepts of being, since it can be seen as an abbreviation for “True is the statement that about that which is—in terms of the criterion of being of subspecies *x*—says that it is in terms of the criterion of being of subspecies *x* or of that which is not—in terms of the criterion of being of subspecies *x*—says that it is not in terms of the criterion of being of the subspecies *x*. And this applies to all kinds of *x*.”

Nicolas Rescher and Robert Brandom (1980) distinguish between standard worlds and nonstandard worlds. The latter are inconsistent but may exist just as well as standard worlds. Although they may be rational, they violate principles that have been accepted as common for all system creations that refer to that which really *is* and not merely *seems to be*.

If all systems have identical basic categories, even if their authors do not accept this premise, then must those who in their explicit formulations take into account fewer kinds of being than the philosopher who introduced more types be supposed to have intentionally left the others unmentioned? Those who polemicize against the many degrees and kinds of metaphysical being would have to have misunderstood their opponents, or perhaps misunderstood their own systems. In this way one might try to keep a common framework for all systems, an expanded identity- and contradiction-principle, but I cannot believe that we will find such a framework, and I do not see the importance of finding one.

“The Common World”: Postulate or Reality?

The standpoints toward the possibility of the common world may be placed between two extreme limiting cases:

1. All human beings—including philosophers and poets in all cultures—respond to one and the same identical reality, but they respond in part in extremely different ways, and to some degree to different parts of this reality. Furthermore, they see reality from different perspectives, from different needs and expectations, and from differ-

ent talents and gifts. They throw different conceptual nets over the same reality and live to that extent in different worlds.

2. Human beings respond in all essentials similarly, but toward partly different worlds. Any individual lives, strictly speaking, in his own world. The situation to which man reacts is different and specific for each individual. Life situation, life space (Kurt Lewin), choice situation, and internal reality are different, and change with time for each individual, but talents, endowments, needs, expectations, conceptual nets are in all essentials the same. The different specific worlds are not parts of one comprehensive (unexperienced) common world, nor are they aspects of such a common world.

Conceptions other than those that can be pressed between these two extremes are worth close examination—for example, the one in which it is meaningless to claim that there are two radically different worlds or realities. Since we cannot indicate any experiences that could connect us with both, even indirectly, it is senseless to claim the existence, and hence also the possibility of the existence, of two. (If we were to experience both, we ourselves would become a connecting link, and thereby the two worlds would be combined into one.) This standpoint, which of course is based on a semantics and epistemology that is very much alive, might then be combined with the semantic claim that if a sentence is meaningless, then so is its negation. From the meaninglessness of "There may exist two worlds" the meaninglessness of "It is not the case that there may exist two worlds" follows. Now one world is a special case of "not two worlds," from which follows the meaninglessness of "There can be one world" and therefore also of "There is one world." All discussion of the one world disappears!

Many systems seem to presuppose that reality is one. The representatives of such systems think that what their system covers *is* that reality and that their system covers reality in an adequate manner (in all essentials), whereas the other systems fail to do so. The expression "stipulate a reality" is perhaps apt since it is not a matter of having found out that reality is singular, but a kind of requirement. Insofar as the expression is involved in a concept 'reality', and this is the one employed in metasystematic statements, it applies without restriction: "Within all possible systems, reality is regarded as singular." When one speaks of living in different worlds, or in general about the existence of different realities, one must employ other

concepts of 'reality'; otherwise, inconsistency results. We do not have to discuss here whether systems that have only explicitly formulated concepts of this latter type must be said to employ the former implicitly (besides the others). I assume that no system needs to assume one reality if the unity involves more than something purely formal or something purely stipulative. That there is only one reality, or that there is at all a reality of the kind most systems say there is, is no more than a possibility if such a reality is conceived to be something of which at least one specifiable proposition is true, beyond what is said in the definition. Statements of the kind "The only reality has the characteristic *C*" may possibly be true, possibly false, or possibly neither (such as "The present king of France is bald").

It is usual to contemplate an atom as both a material object (or "physical system," to speak in textbook terminology) and a model. The same duality applies to a number of other entities. Does it apply to the world as well? The history of ideas, including the history of scientific ideas, can tell us about different world concepts. They have functioned as intellectual models in a somewhat wider sense than the atom. In the case of the atom, it is clear that a duality concept is not necessary. Various continuum concepts provide alternatives.

Does the same idea apply to the world models? Are we at all necessarily bound to employ world concepts? This seems doubtful. A kind of solution has to be found for the question before one makes a decision as to whether there can be several worlds. Most philosophers seem to accept that we all live in one, single, definite world.

Even if philosophical systems are incommensurable or if commensurable are contradictory, they nevertheless have one thing in common: they all deal with *one and the same* "world." There is something that is given for all. The world is that which philosophers struggle with: they seek order and meaning in it. It is an impossible thought that different worlds should be passed out (by God?) or that different sets of data should be distributed to different philosophers. If one could only manage to characterize the given in a neutral way, one would be able to describe the systems within a common frame of reference as different attempts at orientation in the same terrain.

The system comparer, however, would have to confront the systems in the same way that the rat psychologist treats his rats: he lets some of them out in a problematic field of mazes of his own construction and control,

while others are exposed to a cold airstream that blows on their tails when they must choose a path in a hurry, and so on. The rat-psychologist decides high-handedly what is to be called the right path and what is the wrong one. The rat has no say in the matter. The system comparer is no such super rat psychologist or supersystematician. Most system creators may imagine a *common* world (among the clear exceptions are Leibniz and Max Stirner, and others), but the system comparer has no special abilities that enable him to characterize such a common world independently from each system. He is in no better position than the others. From the moment he draws his first distinction, he is caught in a system—he has performed a system-constituting act: he himself is in the mazes, not in an armchair outside.

Everett Hall (1960) has introduced a concept of "categorio-centric predicament" or "category-centered dilemma" in relation to a similar problem: committing oneself to *special* categories that might have been chosen differently. Hall, however, accepts the given as something that all systems must tackle, and thinks that by assuming that something common is given, one avoids an untenable relativism:

[Each one of the various rival philosophical systems], in its dispute with the others, judged the issues in debate from its standpoint and used its categories to set them up. The world does not appear in its own right to decide this contest. One can get at the common object here only through the various rival characterizations of it. What to do? A sheer relativism that simply says, "Be happy with any philosophy which, by some chance, you have got yourself involved in, for there is no rational choice between them," will not do. This is indeed one of the contestants and should not be favored without reason above others. So we are faced with the problem of trying to get out of our categorio-centric predicament and coming to terms with the universe by means of something given to all philosophical constructions. (Hall 1960: 127–28)

Nevertheless, Hall's reflections about the given are marked by specific attitudes and positions that are not the only possible ones. His explanations do not provide any fixed point beyond the systems, a *corpus alef*. Indeed, elsewhere he seems also to state that he operates with certain categories. These are involved and make the hypothesis about the common given system relative.

To the world pluralism that I think is a living philosophical possibility, it is not in itself destructive to assume that all philosophers proceed from the "given," but then it becomes a completely formal category. It says

nothing about the given as content; nothing is said from a material point of view. The same applies to certain world concepts that can be used in combinatorial logic and in statistics: they are completely formal concepts that concern structuring, not that which is structured.

One approach is to work on the basis of a program: let us seek to delimit a commonsense world, our common world. Such a delimitation lies within the scope of modern social science insofar as one wants to arrive at a social net of relations. "The problem of social reality" may, I suppose, be posed as a completely research-worthy problem, as Alfred Schutz (1962–66) suggested. When one speaks about discovery of the presuppositions of the commonsense world, however, a serious problem emerges: where does Schutz himself stand during such a discovery and from where does he get his terminology? Columbus could discover America because he came from outside; the Indians he encountered could not. In his introduction to Schutz's *Collected Papers* (1962–66), Maurice Natanson makes Schutz's approach appear as a kind of intuition; he says that Alfred Schutz's philosophy "articulates a single intuition, the discovery in full depth of the presuppositions, structure, and signification of the common-sense world" (Natanson in Schutz 1962–66, vol. 1: xxv).

Schutz constructs a model of different strata in man's social reality. The whole schema is described as if he finds these very strata and places himself safely and securely by one of them, just as a geologist sits down by a rock and takes out his little hammer to break loose a piece in order to analyze it. Natanson writes that "[T]he taken for granted everyday world of living and working is the nuclear presupposition of all other strata of man's reality, and it is this ground of social reality to which Dr. Schutz turned and which he took as a point of departure for analysis" (Natanson in Schutz 1962–66, vol. 1: xxvi).

Natanson states further that the indispensable foundation for earthly existence remains unnoticed by ordinary men, even if their life is structured by and based on the mold of everyday life. "The philosopher's privilege is to render the taken for granted object of his critical inspection, and this indeed was the procedure of Dr. Schutz" (Natanson in Schutz 1962–66, vol. 1: xxvi). This is possible, I suppose, but it can only be done from a system that treats "taking *x* for granted" ("the obvious") as an *object* for inspection. When N. N. crosses the floor, he takes for granted that it will hold up. To describe this view presupposes a structuring: floors that hold

are distinguished from floors that do not; the goal of not falling through a floor is derived from a more general goal of not hurting oneself. These goals are ascribed to the one who strides across the floor. The phenomenologist looks at N. N. as he crosses the floor, thinks of all that N. N. takes for granted, enters into N. N.'s way of experiencing—but from a system, a conceptual structure. One notices the structure when, as a phenomenologist, one thinks that one makes a mistake.

The model called "social reality," "commonsense world," or "our common world" tends to be underestimated. One postulates a social reality but does not think one has employed any model for this: indeed, the social reality is *just precisely* the reality in which we live. The situation is perceived in the same way that Karl Marx perceived communism in his *Die Deutsche Ideologie* (1938): idealism is an intellectual construction, but communism is *the development itself*. Communism is no ideology; it is not a name for a doctrine. The sentences that seem to express communism as an ideology have merely a kind of index function: they point to the very development itself in its progress. This is a completely natural way of speaking as long as one is within a system with all one's heart and brain. Then it is of course not perceived as a system, but if there is even a tiny discord in one's mind, one will sooner or later have glimpses of the system from outside. One is again in the maze of possibilities, a maze not of one's own making, but not of anybody else's either, . . . or so it seems.

Cultures Construed as All-Embracing Systems

“Philosophical Systems” as Designations for an Articulated View of Reality as a Whole

The aim of this chapter is to examine the extent to which problems dealt with in relation to philosophical systems also arise in relation to cultures. This especially concerns the possibility of describing a philosophical or cultural system within the framework of another system. It also concerns the possibility of comparing systems in a neutral way—hence without relying on the concepts of one system or construing the other in terms of them. In addition, we are interested in both examining the possibility of indicating criteria for the depth of philosophical and cultural differences and exploring the influence of these different levels of variance on mutual understanding and comparability.

It turns out that it is not easy to approach these problems without saying something about concepts of culture and modern cultural anthropology. First, however, a repetition of the system idea as applied to philosophy is needed.

Philosophical systems are verbally expressed by sentences, in part descriptive and in part normative. Each sentence has a sense (a meaning), in terms of the Aristotelian trichotomy, as: (1) linguistic expression, (2) sense or meaning, and (3) reference or state of affairs. The system becomes a *B*-entity.

The systems “pretend” to provide justified truth about reality, hence a kind of insight or wisdom. The unique characteristic of *philosophical* systems is that they refer in direct speech to reality as a whole. They address everything that is required to comprehend reality in all its essential and fundamental traits. They do not refer to every single thing in reality indi-

vidually. Such an approach would lead to an all-inclusive, detailed encyclopedia. The systems thus comprise more than what is ordinarily intended by such expressions as “the world,” “the universe,” and “nature,” but also less, since the details are not relevant.

From a historical point of view, a philosophical system is a construction. History reckons with an incessant development in the thinking of individual philosophers and acknowledges the fragmentary and context-determined character of all written texts. Philosophers conceived as historical persons do not write down systems in the sense of the expressions introduced in this work. Even in the case of Spinoza, one must take into account more than the central text, the *Ethics*. Spinoza's unsystematic letters to colleagues are also a source when one seeks to describe what is called Spinoza's system. The systems should be construed as units.

If one takes the *Ethics* in isolation, however, it is inordinately well suited as an example of a connected set of statements about reality, even if one does not fully comprehend the methodology and political philosophy. Within the cultural tradition to which Spinoza belongs, these subjects must be counted as important in descriptions of reality. If only for this reason, one cannot identify the *Ethics* with Spinoza's system.

The main purpose of this chapter, as suggested, is to examine the extent to which the problems concerning philosophical systems that have already been discussed apply also to cultures. If the answer is something like “to a great extent,” then this must have consequences, not only of a theoretical nature, but also for national and international cultural politics.

Cultures as Information Economies

The analogy between philosophical systems and cultures cannot be made effectively without a certain simplification. Introducing some culture concepts will make the analogy less complicated. Those culture concepts that view cultures as embodying more or less uniform sets of normative and descriptive knowledge or information fall into this category. Because of the comparatively uncomplicated analogy with such a choice of concept, I shall begin the comparison of cultures and philosophical systems with the description of one of these concepts, the so-called information-economic one. It is strongly influenced by Euro-American information theory and social economics. The concept must appear rather peculiar from the point of view

of other traditions, such as the Indian, but that is, if anything, an advantage rather than a drawback in view of our special task in this chapter.

According to John M. Roberts (1964: 438), a culture is an “information economy” in which information is generated, stored, recovered, communicated, used, and in part also lost. A philosophical system, as the expression was introduced, corresponds to the content in the stored (cultural) information. The difference between two systems then corresponds to the difference between the stored information of two cultures.

About this storage of cultural information Roberts, among other things, says:

In any culture information is stored in the minds of its members and, to a greater or lesser extent, in artifacts. Since the four tribes under consideration [Chiricahua Apache, Mandan, Omaha, and Cheyenne] were preindustrial and nonliterate, it is not necessary to consider artifactual storage here. . . . For purposes of discussion it will be assumed that the entire informational resource of each of the four cultures was stored either in the heads of tribal members or in the heads of those nonmembers of the tribes with whom tribal members communicated. (Roberts 1964: 439)

Among the people who are *not* members are researchers from the industrial countries. They are the ones who have attempted to *describe* the cultures. Such descriptions belong in the information-theoretical category, in which *the stored content of the culture* is represented systematically in English or another language that is employed by cultural anthropologists from industrialized countries. This sounds straightforward at first blush but appears very problematic after closer analysis.

On the basis of the systematic, purely descriptive account, one might then conceivably formulate a system corresponding completely to a philosophical system, consisting of norms and hypotheses. Recovery of philosophy takes place by communication, the use by application in action, loss by imperfect communication, and in other ways. The same might be said about cultures. Thus, the analogy between cultural system and philosophical system is very clear in terms of the information concept and information theory of culture.

Conceived in terms of information theory, cultures may be more or less complex, and hence the individual members will in a lesser or greater degree be able to be carriers of great parts of the whole culture. According to

Roberts and Morris Opler, whom Roberts quotes, Chiricahua Apache exemplify the latter. They had no formal ceremonies, no institutions that only the leaders knew about. "An adult male and an adult female actually commanded a substantial fraction of the total Chiricahua Apache culture" (Roberts 1964: 446). A natural comparison is with the conditions in the medieval, extremely decentralized Lapland.

The circumstance that most cultures, to be preserved in their entirety, require many members, makes the analogy with philosophical systems somewhat more distant. In addition, special problems arise for the information-theoretic model when the content that is stored by one group, one particular profession, for example, is in part incompatible (inconsistent) with what is stored by another group. If ordinary axioms for " x is an instance of knowledge" are applied, x must not contain a set of inconsistent parts of knowledge. For example, if a culture contains assumptions about the kinship between persons A , B , and C , then the set of assumptions, x , cannot contain both " A and not B is the mother of C " and " B and not A is the mother of C " without losing its status as a coherent whole of knowledge. In other words, according to these axioms a culture cannot be represented as a coherent whole of insights if two so-called insights are cognitively incompatible. We will have more to say about this later.

Let us make the unreasonable assumption that we stand cultureless and are offered membership in a culture. In view of the preceding, this offer does not seem to be of quite central importance. As information economies, cultures offer various ways of generating, storing, recovering, communicating, using, and losing information. These are useful things, but useful for what?

Cultures as Total Forms of Life and World Images

In what follows, I take up a culture concept or rather a family of culture concepts that are akin to Roberts's, but that nevertheless differ sufficiently from his to give another shape to the problems we are addressing. I shall in all essentials keep to Fredrik Barth's accounts within the subject he calls social anthropology. It is very close to what kindred researchers have called cultural anthropology.

That Barth's concept has intimate relations to philosophical systems is suggested by some of his central statements (1977), for example, that a cul-

ture is a specific way of interpreting the world, organizing society, and finding the meaning of life.¹ A particular culture designates the main features of an intended organization of society and seeks to denote the meaning of life as an essential part of reality. The term *intended* is worth noting. A description of a culture does not have to include ways of behaving that are not intended.

It is presupposed that no culture exists that denies life a meaning. This presupposition is not decisive. Philosophical views that deny a meaning in life presuppose at least implicitly that a stand is taken on all branches of philosophy.² If the denial characterizes a culture, it will implicitly be all-encompassing.

Barth (1977) contends that "each culture can be understood as a world alternative to our own." The wording here is close to my own in the question "Which world is the real world?" My response is, *inter alia*, that no single world can be pointed out as more real than any other. An analogous answer is given by Barth in relation to cultures!

Barth further assumes that each culture contains a wealth of traditions and visions of the good life. Analogously, I argue that the same holds for philosophies: such content is a necessary condition for motivating the exertion required for conscious development and verbal articulation of philosophies.

In a conversation in 1972, Barth mentioned to me that he estimated that there were several thousand such cultures on earth. Given the homogenizing effect of globalization and the stresses placed on indigenous societies, one can only conjecture how radically reduced the number will be in the future. I perhaps require more originality in a culture than Barth does, and thereby reckon with somewhat fewer cultures in the following analysis. In any case, numerical indications are highly arbitrary. The demarcation problem is similar to the one of distinguishing among languages and dialects. The number of living languages depends on where the line is drawn.

The word *tradition* is central in cultural anthropology. Its closeness to the concept of culture appears from the following statement by Barth, which is after all very similar to the statements about cultures. "Every cultural tradition defines premises and meanings of life for the population that carries it . . ." (1977).

"Cultural premises" and "system premises," in the sense of nonderived descriptive statements and prescriptive rules, are not the same, but there is

a kinship between them that is important for the comprehension of both. Many anthropologists have indirectly presupposed such a kinship. "[I] have shown how nearly everything that happens in Kalabari life can be interpreted in terms of a scheme which postulates three basic *kinds* of forces: ancestors, heroes, and water-spirits" (Horton 1970: 133). Robin Horton, who made the preceding statement, contends that the system of explanation corresponds to the Western one in which atoms, molecules, and waves are introduced as nonobservable entities. The three sets of theoretical entities serve to introduce "unity into diversity, simplicity into complexity, order into disorder, regularity into anomaly" (Horton 1970: 134). Horton thinks that when anthropologists do not see the similarity, it is because they are unfamiliar with their own occidental cultural theory constructions (for example, as in physics).

I. C. Jarvie and Joseph Agassi (1970) emphasize that the strength of the magical worldview lies in its completeness. Everything can, in principle, be explained in terms of either magic or failed magic. After all, the difference between magical total views and "scientific" ones, argue Horton and others, is that the scientific tradition involves a permission to criticize theories and their basis. The relevance of this point for the problems in this chapter, however, is only partial. Cultures are more than total views of how events can be explained. They also contain, among other things, *descriptions* of that which needs to be explained.

Structure, Experienced Content, and Correct Conduct

A word that is central in cultural anthropology, as in many other social sciences, is *structure*. We use the word daily. A boat can have a beautiful, solid, complicated, or awkward structure. The structure of positions in certain universities in the postwar years was top-heavy: there were many professors and few junior positions. The structure of Ibsen's dramas appears to be meticulously thought out. Barth (1977) says that "anthropologists seek to demonstrate the structure in each culture—how it constitutes a cognitive system, a set of codes for communication between people, and a world picture whereby they interpret their external reality."

A whole series of deep questions might be posed by using this quotation as a point of departure. One of them concerns the relationship between structure and content. As fellow beings, we are interested in the cultural

content rather than the structure. The codes of communication are important, but in human relationships they are important only because they are necessary to obtain access to content. Can cultural anthropology as a science do more than describe the codes as part of structures?

The quote also suggests that the anthropologist seeks to show how the structure constitutes a world picture, whereby the carriers of the culture "interpret their external reality." This formulation begs the question "How then does reality look, how is it experienced, when a carrier of the culture uses the code and the world picture in his interpretation of reality?"

My reason for posing the question is primarily culture-political. The ever better organized, but also constantly unsuccessful, attempts to preserve cultural diversity on earth presuppose that cultures differ not only in their structures, but also in their experienced content. This point of view presupposes that lived worlds must be different. It is not enough that the machinery differs.

To remind ourselves of this point of view, it is perhaps appropriate to let "world picture" stand for something more concrete than some code. The emphasis might be placed on perceivable entities, not merely an abstract representation, a model, or a world depiction. When applied, for example, to Ghanaian music, it involves an attempt to gain knowledge of how the Ghanaian music affects the listener and is experienced by him in comparison with how German or Norwegian music appears and is experienced. Hence, in addition to knowledge of structure, we must also consider knowledge of the experienced worlds and their mutual relationships. Some insight is obtained simply by asking. Ola Kai Ledang (1981) asked a chieftain's son from Ghana what he thought of Beethoven's symphonies. The spontaneous reply was, "For me the symphonies of Beethoven are not very interesting or valuable music. It sounds almost like a march all of it" (Ledang 1981: 22).

There is an essential difference between behaving "correctly" in a situation within a culture and experiencing the situation in the natural way that a carrier of the culture would experience it. A robot can, in principle, behave correctly, that is, within a wide or narrow frame defined by norms and hypotheses (seen from a system viewpoint).

Let us suppose that two anthropologists or a tourist and an anthropologist conduct themselves correctly in a foreign culture. Their levels of insight into that culture may nevertheless differ. Their depths of intention may be different. Deep comprehension of a culture is presumably impossi-

ble without *intimate familiarization* with the culture; this presupposes identification with the culture and thereby affective transference. At this point, too, the similarity between philosophies and cultures appears.

Erik Dammann describes his and his family's experience of attempting to *live* in a foreign culture:

The [Samoan] village itself had enthusiastically accepted what we in our naive ignorance said the first day: We do not want any separate treatment, we want to learn to live *your* life! In practice, this turned out to mean that we had to live more closely and more constantly together with them than we were used to even with the closest family at home. In order to manage such a form of common life, it was not enough to "understand" them. We had to become fond of them. (Dammann 1981: 25)

It is worth noting that "understand" here is in quotes. For if understanding refers to content and not merely abstract structure, then the researcher must to some extent experience the world the way the foreign culture experiences it. This means that in Dammann's Samoan village, people look at each other and perceive each other in ways that are affectively normal in terms of the Samoan culture's premises. Hence, the understanding has an affective aspect that cannot be detached from the intellectual one.

It is difficult to believe that a working anthropologist who seeks to enter as far as possible into a foreign culture does not, at least in glimpses, experience a situation very nearly the way a carrier of the culture does. It is, however, methodologically very difficult to confirm the degree of proximity.

If one emphasizes the uncertainty of this assumption of "glimpewise" similarity, one must also stress a corresponding uncertainty within one's own culture. The problem is then no longer specifically a problem of cultural anthropology, but one that is relevant quite generally to the question of demonstrating interpersonal similarity or difference in experience.

The differences between the Baktaman culture and my own (Norwegian) are overwhelmingly multifarious and evident in all types of everyday situations, not just special situations such as participation in rituals or hunting. Suppose we try to list all the norms and hypotheses that are both relevant for the Baktaman during an arbitrary five-minute period of their everyday life and different more or less from life in Norway. I predict that there would be hundreds of entries before we got to listing their perceptions of their external surroundings (trees, paths, dwellings, light, shadow,

rain, sun, and so on). From a philosophical point of view with an emphasis on Gestalt thinking, the list would in principle be amenable to nearly limitless expansion.

All these differences are relevant for how the world and one's self are experienced. They work together to form total experiences, not by summation, but by Gestalt laws—I suppose. Indeed, we can obtain more or less secure knowledge about the structure of such differences, but if we are concerned with culture content, we cannot avoid asking, To what extent and in what way can we experience five minutes in the way it occurs in the foreign culture?

Furthermore, we have the additional problem of the maximum possible difference in experience and the hypotheses of biological or other factors that can tell us about the limits of these differences. It seems unreasonable to assume that biological similarities, such as the make-up of human sense organs, do not determine limits of difference in experience.

"Culture," 'Culture', and Culture

In the preceding we have in our reflections about systems first employed a rather special and relatively clearly delimited concept of culture: Roberts's (1964) information economy. In addition, we have used a more diffuse, but intuitively more plausible concept presented in Barth's paper (1977). That concept, too, makes cultures cognitive units, units of insight. In cultural anthropology, however, very different, much broader definitions have been prevalent. Let us consider three examples (Keesing 1974: 138):

1. The complex whole that comprises knowledge, art, morals, law, custom, and any other skill or habit that a human being has acquired in its capacity of member of society (Tylor).
2. The sum total of knowledge, attitudes, and habitual patterns of behavior that members of a particular society have collectively and transfer (Linton).
3. The man-made part of the milieu (Herskovits).

These ideas are clearly important for any description of culture, whether or not one includes artifacts in the description—that is, the cultural objects. Furthermore, if one includes factual, mostly statistical information

about learned behavior, or keeps to the patterns and norms of behavior, it becomes necessary to consider more carefully the semantic and the science-historical peculiarities of the word *culture*.

Like *democracy*, the word *culture* shows a high degree of ambiguity and vagueness—although this does not prevent it from being useful cognitively. On the contrary, such words may indeed be irreplaceable in political science and cultural anthropology, respectively. It is, however, important to avoid the illusion that such words stand for any specific cognitive content.

It is also important to understand the reaction in contemporary (1970s) cultural anthropology against an influential concept of culture that was developed by E. B. Tylor and continued by Alfred Kroeber and Clyde Kluckhohn. One of the leaders of the “reaction,” Roger M. Keesing, formulates it as follows:

[W]e increasingly realize that the holistic, humanistic view of culture synthesized by Kroeber and Kluckhohn includes too much and is too diffuse either to separate analytically the twisted threads of human experience or to interpret the designs into which they are woven.

The challenge in recent years has been to narrow the concept of “culture” so that it includes less and reveals more. As Geertz argues, “cutting the culture concept down to size . . . [into] a narrowed, specialized, and . . . theoretically more powerful concept . . . has been a major theme in modern anthropological theorizing.” (Keesing 1974: 73)

The approach suggested by Keesing in this example is clearly profitable in the short run, but its value over the long run is more questionable. It turns out to be infeasible to obtain agreement about one concept as the scientifically fruitful one. Key words such as *culture* are simply not amenable (fortunately) to constriction in such a straitjacket. Attempts to standardize are likely to lead to unfruitful propaganda for the concept that suits the research of one’s own group in the short run. The compendia *Psychologies of 1925* and *Psychologies of 1930* provide discouraging examples from a related discipline (each psychologist has his own concept of psychology).

On the other hand, it is clearly fruitful to make *culture* more precise in special directions and use it in specific ways definitionally for particular purposes and in particular texts. Then, however, one introduces stipulative definitions without pretensions that everyone in the discipline, regardless of research program, should or can keep to the definitions.

The information-economy concept introduced by Roberts provides a good example of a precization of *culture* adapted to new, presumptively valuable, but very special research programs. In what follows I shall occasionally use the word *culture* the way it is used vaguely in the literature that Keesing characterizes as holistic and humanistic. I do not pretend that the word expresses any *specific* concept, but that the use, *if it is made more precise*, falls under the family of concepts that are also exemplified by Barth's use, hence a cognitive concept.

For the comparison between culture and philosophical systems, it is fruitful to eliminate noncognitive elements. It is helpful to keep to concepts of culture that do not include cultural products, such as paintings and ships. Similarly, one should focus on the rules of accepted practice rather than try to include all actual customs and their gross deviations. The intentions of behavior, not the way a society lives, then become part of its culture. The prescriptions for behavior, not the descriptions of behavior, become relevant.

With this precision, a culture demarcates an all-purpose set of norms and descriptions (of the world). In what follows, then, I keep to such a concept of culture.

The application of a cognitive concept of culture is, of course, not unproblematic. Suppose we film a dance. What belongs to the culture and what does not?

An older woman rises and dances out on the floor. After a while we understand that her movements illustrate what has happened.

At first she dances with bowed head and serious face. Sometimes she stretches out her arms imploringly, then she lets them fall again and dances slowly on with her eyes towards the floor. Then she suddenly looks up and smiles, grasps in front of her as if she receives something. The song becomes quicker, the clapping becomes rhythmical and fast, the old woman wiggles and hops merrily while she laughs and pats her plump stomach again and again.

It is not difficult to see what she represents: the hungry have received food and have become happy again. (Dammann 1981: 115)

For this culture, there are guidelines for the dance, the method of representing the historical material. In this case, the guidelines are the fact that the Dammann family in the village of Manase went almost without food for several days, and many other dramatic details related to this sad

event. Problems arise, however, when one wants to give a precise description of the cultural content of this dance. It becomes no less difficult to describe Erik Dammann's own subsequent dance, which was received with laughter, shouts, and vigorous applause by the community.

Now I tackle the question "Do similarities between cultures and philosophies disappear because philosophies have an appreciably higher level of integration and are furthermore assumed to be logically consistent?"

Integration Level and Consistency

The philosophies of Aristotle, Thomas Aquinas, and Spinoza—to take some prominent examples—consist of elements joined together with great accuracy. Researchers who study such philosophies usually hold that the mutual relationships of all the parts are carefully thought out—they are completely integrated in a consistent whole. Here, no deviation is tolerated. One might have another impression, but through specific interpretations that do not have to be completely unreasonable historically or by distinguishing the "mature" philosophy from fragments created during the philosopher's development, the researchers perhaps manage to verify that the system is consistent.

It is more realistic, however, to reckon with imperfect integration and only partial consistency, even if only a minority of specialists has such a view. In the case of Spinoza's philosophy, it is comparatively common among specialists to complain about the inconsistency in it. I myself think that it is, in any case, exceedingly difficult to arrive at an interpretation of the texts that embodies a clear, all-encompassing integration and consistency. Generally, I suppose we should keep our minds open to the possibility that philosophies may be highly integrated and consistent, while recognizing that perfection is hardly attainable and not at all demonstrable, thereby clearing away an objection in principle to seeing a close similarity between philosophical systems and cultures. Cultures are indeed incompletely integrated, and it is the anthropologist's obligation not to overlook this. According to Clifford Geertz:

The problem of cultural analysis is as much a matter of determining independencies as interconnection, gulfs as well as bridges. The appropriate image, if

one must have images, of cultural organization, is neither the spider web nor the pile of sand. It is rather more the octopus, whose tentacles are in large part separately integrated, neurally quite poorly connected with one another . . . and yet who nonetheless manages to get around and to preserve himself, for a while anyway. . . .
(Geertz quoted in Keesing 1974: 80)

Even if the level of integration within a culture is never perfect, as sometimes seems to be presupposed when there is talk about cultures, it is easy to demonstrate deep connections. This can be done by using simple sentences as points of departure. Consider, "I am a parakeet" spoken by a Bororo (and translated to a kind of English) and "I am a sinner" uttered within a Christian sect. Both statements are important indications of reality as these people perceive it in their culture; both are integrated in their social life and can be understood in terms of the context. Being a parakeet in Bororo entails many norms for behavior and decision making in conflicts; the same applies to being a sinner.³

A clear difference with regard to philosophical systems lies in the fact that cultures are collective. A culture is something that the culture carriers have in common. Here, too, a certain graduation must be introduced, although with caution: features of the culture may be more or less common, but we must remember Geertz's admonition about independencies and crevices. More important, perhaps, is the possibility that the culture carriers may, within the group, have so much independence that we would have to speak of subcultures, part-cultures, and cultural minorities.

With the Baktamans, the integration does *not* take the form of a consistent whole of insights, information, or knowledge. On the contrary, people moving through the seven steps of initiation plus those who are uninitiated often hold mutually inconsistent units of information. At each step, things are asserted that are denied on at least one other step. This point seems to me to reveal an essential weakness in the conception of cultures as information units or other things akin to units of normative and descriptive knowledge.

Against this negative conclusion, however, we might object that there is no inconsistency at the step of initiation that is, within the culture, generally recognized as the highest one. At this step, the purpose of "misinforming" culture members on lower steps is also explained.

The objection does not hold, because the members who have essential misinformation are clearly also culture bearers. Their purported insights

are genuine parts of the culture. They are important parts of the total complex of conceptions about what it is right to do and to think, which constitutes the culture according to the intention- and information-oriented culture concepts.

The complexity that arises from the presence of conflicting information within different groups, however, need not prevent us from perceiving the Baktaman culture as, in some degree, perhaps a very high degree, integrated. It may be highly uniform, only not in the form of consistent insight or knowledge among the culture bearers. One might try to find knowledge concepts such that consistency does not belong to the necessary conditions. As far as I can see, however, no fruitful concept has emerged from such efforts.

Moreover, in cultures other than the Baktaman, one may also find contrary information where the maintenance of conflict is deeply tradition-dependent and protected by norms for what is right to do and think. Cultures cannot be comprehended as wholes of knowledge. Complex totalities, yes; totalities of a kind similar to knowledge no.

Within many cultures, certain groups are generally recognized as the most competent carriers, and their systems of insights may be consistent. This applies to the Baktamans. When we, in what follows, continue to compare philosophical systems and cultures generally, we shall, in the case of those similar to the Baktaman, keep in mind the conceptions of a subgroup. Those who are completely initiated define the unity and totality of a culture.

Message and Knowledge

In Western tradition, philosophies are mediated by texts. In other traditions, oral communication can be the decisive or only form. All systems presuppose at least some form of language.

How are cultures communicated? In linguistically accentuated theories of culture, *message* is a key word. The message does not have to be transmitted by words, but even if it conveyed without words, linguistic terms may be applied. A harsh look without words may have the same function as a verbal expression of disapproval. One can employ a nonverbal "vocabulary."

The sender-receiver relation is central: it must be taken as the normal case of a message that precisely what the sender *intends* to impart is picked up by the receiver. In cultural communication, however, the intention does not have to be conscious, as it must be for philosophical systems. For the present, I shall nevertheless stick to a concept of message that requires that conscious intention. This requires a person or at least beings that are capable of teaching each other something and eliciting or performing messages.

If a *unit of behavior* (with or without words) functions as a message presented by a person or group *A* and the receiver *B*, one may distinguish among three cases: the groups *A* and *B* are identical, they partly overlap, or they are entirely separate. The first case is conspicuously important if one is interested in comparing cultures with philosophical systems. *Cultural content must be constantly repeated among the culture bearers if the culture is to survive.* The new generation must learn much and thoroughly. If the transfer is inadequate, the content of the culture shrinks.

One may, in a linguistically oriented theory of culture, speak of what *is said* in a ritual. The apprentice learns something by seeing and performing a ritual. When the learning is complete, the learner has acquired cultural knowledge. The concept 'knowledge' that is thereby introduced is such that the anthropologist who employs the expression does not himself regard the content as knowledge, but, for example, as superstition. The content does not have to be cultural knowledge within the anthropologist's own culture. A ritual, then, is said to contain (as a whole and in its parts) behavior units that impart knowledge. The receivers, however, frequently differ widely in their competence to comprehend the message. This, of course, applies to philosophical systems as well.

During initiations, the apprentice is normally frenzied, and so much is new that only repetitions of the messages can lead to clear reception. A senior Baktaman says, "You know how it is during your initiation: your *finik* (spirit, consciousness) does not hear, you are afraid, you do not understand. Who can remember the acts and the words?" (Barth 1975: 101).

The second part of Barth's book about the Baktamans deals with the seven degrees of initiation, from the first and lowest to the highest within the culture. His anthropological objective is as follows: "I wish to provide a substantive documentation of the forms of Baktaman ritual activity, and as far as possible establish not only what may be understood and intended by

the leaders of these cult activities, but also *what is actually received* as messages by audiences and participants" (Barth 1975: 47). If it succeeds, the procedure will ascertain "the nature and extent of the knowledge and insight" that is communicated by the seven successive initiations.

The goal set by Barth has important features in common with, but also important features distinct from, that of a university educator who wants to investigate what knowledge and insight is actually received, say, in an introduction to Kant's philosophy. One difference is that the educator may conceivably apply his own standard of knowledge and not Kant's. The anthropologist will evaluate the knowledge in relation to the culture that is to be "learned" through the initiation ceremonies.

As an example of messages without, or with entirely subordinate, verbal features, one might mention the torture involved in some of the initiation ceremonies among the Baktamans: "Water and fire are also used for torture, reinforcing the basic messages of earlier initiation: that these forces are powerful and dangerous; sacred knowledge is costly and must be paid for with hardship and its value thus confirmed" (Barth 1975: 66).

The study of Baktaman culture makes it clear that the many complicated ritual details are exceedingly important for both the learner's individual welfare and the culture's. Violations of the rules can cost the apprentice his life and cause disturbances in the whole society. Use of painful initiations emphasizes the gravity of the rules. Torture must presumably be counted as a much stronger way of communicating this knowledge than merely saying quite calmly, "Water and fire are powerful and dangerous forces."

It might be said that the comparison with such a sentence is unsatisfactory because the cultural message is so much stronger and more complex. We must, however, consider for *water* and *fire* in the above sentence words that in the Baktaman language give rich and complex associations.

The use of the words *knowledge* and *message* among linguistically oriented cultural anthropologists is close to the use that I as a metatheoretician of philosophical systems employ when I describe a specific system, or more exactly, when I seek to describe a specific system "from within," *in terms of its own premises*. The use of *knowledge* is in both cases subject to the axioms most commonly established for sentences of the class "*x* is a case of (an example of) knowledge."

Cultural Knowledge in Relation to Status

The use of the word *knowledge* in cultural anthropology is far from being unproblematic philosophically, as the preceding may have intimated. I shall now consider more closely a complication regarding the evaluation of Baktaman culture.

In relation to ritual and much else, there are eight status levels in Baktaman society. More than half of the members, namely women and children, are not initiated into the cult. Most others, on the other hand, go through a series of initiations. At the top status level is a small circle of men who have been through all seven steps.

A social structure with eight levels does not necessarily lead to a situation essentially different from the communication of philosophical systems if a new initiation only adds knowledge to that given at earlier initiations. Consistency would then be preserved. Consistency would also be preserved if the initiations made successively more precise an original content. The peculiar circumstance with the Baktamans, however, is that misinformation, hence simply (in part) false conceptions, is deliberately imparted on the lower levels. Only on the highest level is the complete truth revealed. The apprentices are systematically misled concerning particularly important and dangerous knowledge.

This presents a problem: do only the messages on the eighth and highest level express (cultural) knowledge? The messages on the various other levels are such that their conjunction is a contradiction. (I view efforts to make deep philosophy from contradiction as either failures or pointless attempts to introduce another terminology of little use.) If only the small minority that has been through all seven initiations has consistent knowledge, and Baktaman culture is defined in linguistic terms as the *sum total* of cultural knowledge, then most members of the Baktaman society lack Baktaman culture.

Such a conclusion is not drawn in Barth's presentation, and it would also go against well-worked-out points of view on culture in general. Barth uses the word *knowledge* in such a way that misleading messages also express knowledge. Another solution is to give up the linguistically oriented concept of culture, which seeks to construe culture as the *sum total* of cultural knowledge. Then a split must be made by saying that hypotheses and

norms on each of the eight levels belong to Baktaman culture, but that as a whole their eight-level system does not represent a unifiable totality of knowledge.

For the comparison with philosophical systems, this problem is not essential since it is natural to compare only the knowledge of the fully initiated minority with a system.

Description of Foreign Cultures and Untranslatability

The Swedish language may be translated almost directly into Norwegian in the sense that a Swedish sentence can usually be coordinated with a Norwegian sentence of about the same length and sense. The degree of similarity in content or sense can be tested without great difficulty because the Swedish and Norwegian ways of life and manner of thinking do not usually differ in confusing ways. It is quite another thing, however, to translate Chinese poetry or Vedic myths into Norwegian. The linguistic differences are more substantial; but much more problematic are the extensive and deep cultural differences. By employing suitable linguistic conventions on the vocabulary level, one can "translate" sentence by sentence, but anyone who has endeavored to study cultures knows that the meanings cannot be even approximately the same. One way of getting closer to the meaning is to append footnotes to almost every word in each sentence and, furthermore, to footnote each sentence as a whole. One explains, to the best of one's ability, the proper senses of the words and the sentence. Even with classical Greek it is natural to proceed in this way. One conventionally translates *areté* as "virtue," but then in a long footnote seeks to explain what the word signifies. The longer the footnote, the more it dawns on the reader that more than a slightly higher level of approximation cannot be reached.

A sentence in the Nuer language is conventionally translated to English as "Twins are birds." Without a long footnote it is natural for the reader to ask whether the Nuers really think that this is true. Do they mean that twins fly or lay eggs? The footnote can provide the information to answer these questions, but can the "translator," by means of footnotes (not foot-works), impart precise knowledge to the reader regarding the meaning that is intended when the sentence is spoken by the Nuers themselves? I think it is appropriate to use a concept 'translation' such that we can consider it a confirmed hypothesis that the Nuer sentence is untranslatable. This stance,

I believe, will be viewed as reasonable as soon as one gets an impression of the Nuers' way of life and manner of thinking—in short, their culture. It seems very different from English and Norwegian culture, so the burden of proof falls on those who maintain a hypothesis of translatability.

Anthropologists emphasize that when describing a culture one must take the culture's own terminology for one's point of departure. This is ambiguous. If one means the culture bearers' own words and sentences—in short, their own language—a sort of description results: the Baktamans' Baktaman responses to the researcher's questions formulated in the Baktaman language. This, however, is not what is meant ordinarily. One usually thinks of the concepts and what the foreign words mean. These must be the foreigner's cultural property; they should not be replaced by the anthropologist's, but it is highly problematic to imagine how this might occur if the cultural differences run deep.

If we say about a foreign culture that witchcraft is a central part of it, a reader from our culture will interpret the word *witchcraft* in terms of our own culture's understanding of it. Clearly, however, that interpretation will be colored by what we consider to be the essential characteristics of witchcraft. We will, as readers, seek precision in terms of our own culture's concepts. Anthropologists seek to avoid this kind of problem in various ways.

In his central work on the Azande, E. E. Evans-Pritchard (1937: 8) states that he does not strive "to define witchcraft, oracles, and magic as ideal types of thought," but wants to describe what Azande understand by *mangu*, *soroka*, and *ngua*. When someone explains *mangu* by saying that it is "a material substance in the bodies of certain persons," it is clear that distorted ideas are being generated. *Material substance* and other words in the explanation still give associations in terms of our, not Azande, culture. This can indeed be corrected, but the corrections become more and more copious. It becomes increasingly obvious that all aspects of the world picture are involved in all explanations. To clarify the meaning of one word, one ends up—and for practical reasons, indeed one has to end—with an enormous number of explanations. The verbal road to the goal is lost in the horizon.

If one does not end up with an enormous number of words, but reaches comprehension after a few steps, this is a sign that the cultural difference is not deep, such as the one between Swedish and Norwegian. It seems as if insufficient semantic analysis conceals the problematic character of pro-

ceeding from "the terminology of the foreign culture" and "describing the culture on its own premises." In spite of the risk of dwelling too much on elementary semantics, I shall offer an example.

The cultural anthropologist learns about nearly countless *forms* of marriage. The power of words over thought is constantly shown by the fact that one thereby presumes to have a convenient point of departure for comparison of cultures. One culture organizes marriage like this, another like that. It is difficult to avoid the thought that there is one thing, one object, one relationship, *what we all call* marriage, that is common to all cultures but simply has different names and detailed features. If all marriage forms in all cultures are to be defined as subspecies and species of one and the same genus, however, then we must include only characteristics that are common. These are few and highly abstract. A selection of them may be included as conceptual characteristics.

In biology we have the concept 'vertebrate'. All and only vertebrates have certain forms of vertebrae. This is straightforward for ordering some types of thought, but when we are dealing with similarities and differences between dogs, cats, human beings, salamanders, and eagles, the characteristic of having vertebrae has very little significance. The same applies to the marriage forms. "Marriage as such" exists just as little as "the vertebrate as such." The existence of the two words, *marriage* and *vertebrate*, and the concepts introduced by making them more precise, does not secure comparability except on a very abstract level. On the level of way of living and form of life, hence on the cultural level, they are practically worthless. If one encounters angry dogs or an insulted married couple in some culture, it does little to remember that they are vertebrates or spouses.

Information of the kind "Their *marriages* are like ours except that they . . ." may have a limited value, but such constructions make deeper understanding difficult since one continues to keep a concept, or an association center, drawn "from home."

It is more realistic to distinguish among a great number of relations in which human beings are involved. One must seek to describe them as far as possible in terms of a culture's own premises (Barth); hence, without hanging them on familiar pegs. The latter expression is misleading since the frame of reference is and always will be the describer's, not the one of those being described.

The relations in a culture infiltrate one another. Forms of marriage cannot be characterized except in relation to relationships between children and parents, between brother and sister, and hundreds of other relations. Differences in relations, hence in structure, determine differences in the experience of brotherhood, sisterhood, marriage, and all other *content*. Cultural politics seeks structures that affect attainment of the highest purposes of the culture.

In earlier chapters we mentioned differences between philosophical systems and the reduced comparability that results when their differences become deeper and wider. The same holds for differences between cultures. In addition to the catchwords in philosophies, a long series of other catchwords is added in the case of cultures. Instead of mentioning examples from various nonphilosophical realms, I shall mention catchwords from ethical aspects of the cultures and keep to the important words in Harald Høffding's once influential *Etik* (1926): authority, pleasure, displeasure, the individual, sympathy, welfare, motive of evaluation, motive of action, conscience, sanctions, duty, freedom of the will, external constraint, internal compulsion, ability, force, skill, autonomy, personality, evil, stupidity, callousness, purpose, means, society, altruism, justice, self-assertion, and devotion.

These words are taken from the beginning of the table of contents of Høffding's systematic exposition. The list serves as a reminder of the deep and general connections that exist among the individual phenomena or relations suggested by the words. If we extract one of them, say autonomy, it is clear that autonomy within one culture cannot be characterized thoroughly without going into all the phenomena suggested by the other catchwords. Isolation is possible only by more or less superficial and abstract consideration. We must use our ability to think in terms of *gestalts* in order to seek experience of the world of others.

The Depth of Cultural Differences

It is indeed clear that, construed as information, cultures impart different messages. If we compare cultures with philosophies, we can at once (with our own culture as a frame of reference) establish that two cultures may have different *ontologies*. The Baktamans are surrounded by spirits. The spir-

its are an important part of their world. Insofar as spirits are recognized as existing in our culture at all, they have another character than for the Baktamans, and if we compare the Baktamans with cultures in which spirits are also an important part of their world, we find different views of what spirits are. Closely connected with this issue are differences in *methodology* and *epistemology*, or more correctly, differences in how one establishes and perceives knowledge of spirits and other things. The rules and standards for governing conduct (*ethics*) and perception of history and time all vary — the same issues apply to social and political problems. In short, there are differences in all branches of philosophy.

It is not thereby claimed that all kinds of differences exist in all cultures. Lapland was no state but was nevertheless a rather “close” society in the sense elaborated by Nils Christie in his book *Hvor tett et samfunn?* (How tight the community?). The culture exhibited common basic political views, in the wide sense, but perhaps not a philosophy of state.

Furthermore, it must not be claimed that viewpoints within all the branches of philosophy are directly communicated in language or in other ways within all cultures. Some points of view emerge only on the meta-level, in the cultural anthropologist’s comparisons, not as parts of the cultures that are compared. With these reservations in mind, we can claim that differences between cultures may be deep and all-encompassing, just as deep and comprehensive as differences between philosophical systems.

The all-inclusive character of the differences between cultures owes in part to the fact that the differences concern fundamental positions such as those defined in philosophy and in part because cultures are more or less all-embracing and integrated. The latter has the consequence that differences at one deep point generate effects in all other areas. The culture as a whole is colored by the particularly deep-seated features, features that in terms of our classification system appear as profound and all-embracing.

If we compare Baktaman culture with our own, we see, for example, that the constant presence of the ancestors, and their constant praise and blame, emerges as a profound feature. I must emphasize that my use of *profound* here is meant in relation to a Norwegian frame of reference, not profound in any absolute sense. The presence of the ancestors is an ontological trait that describes the (“factual”) character of the world. The Baktamans are thus never alone; they are always supervised. The doctrine and experience of the ancestors cannot be described adequately in a few words. One

must consider each and every aspect of Baktaman daily life and specify in which way the ancestor factor comes in as a codeterminant.

If we compare Samoan culture with our own, property becomes a key word. "This is mine, that is yours" becomes a type of sentence that acquires another cognitive meaning, is involved in other structures, and gives another content of experience than in our culture. In the West Samoan village of Manase, the Dammann family could not "own" their provisions the way people can in Norway. Their supplies were theirs in a certain sense, but not in the way they would be in Norway. Illustrative of this difference is an episode from their first days in which a Samoan woman unabashedly inspects their belongings:

Speechless with insecurity because of all that we did not yet understand, we saw her examine minutely our stock of provisions from the village and not least a stack of dry-milk boxes that we had brought with us from the neighboring island. But when she then, abruptly and resolutely, grabbed half of the boxes and wandered out again, waving her hand, we finally awoke. Full of moral indignation, we ran after her to recapture our legitimate property. She was not going to get away with theft! But all our protest was in vain. When she saw our rage she herself became serious, pointed at us, at her own palm hut, at herself, and our storage shed, patted our shoulders, and walked away. What was it that she had wanted to tell us? (Dammann 1981: 24)

Within the groups of thirty to fifty related, intermarried, adopted, and cohabiting persons, there is "communal ownership, common responsibility and work" (Dammann 1981: 26). With such a six-word, abstract characterization, one gets an inkling of the difference, but no deeper understanding since indeed *community property*, *responsibility*, and *work* are catchwords that to us have a different meaning, are included in other structures, and give another content of experience.

It is easy to emphasize that the differences in question here are universal, that they affect all relationships. More, however, is required for characterizing them as deep. It must then involve great difficulty to adapt to the foreign culture. The difficulties may be divided roughly among two dimensions as purely cognitive, purely affective, and combined affective and cognitive, depending on the experience one has in attempts at adaptation. Property is affectively loaded when the situation is of the kind exemplified above. It is presumably affectively very hard for us to adapt to communal ownership and common responsibility. Besides, it requires that things be per-

ceived in other ways. The cognitive difficulties are considerable but there is also the normative dimension: theft is wrong, morally reprehensible.

To what extent can the depth of the differences be isolated from the scope? The question is too vague for a precise answer, but a couple of examples may help to illustrate how a deep difference can be of a relatively modest scope.

If, in Manase, one points to something in someone else's home and expresses that one likes it, one receives it as a gift. The decisive feature with respect to the depth of difference from Norwegian society lies in the enormity of what one can then receive (and according to the norms should not refuse):

A little girl whose name was Norin (Nolini in Samoan) aroused the enthusiasm of the whole family. She was nine months old, had large eyes, was golden brown and lovely. Every day her mother came and placed her in our *fale* so that Jaran could play with her. One day when she was delivered, Ragnhild said jokingly, "Norin is so delightful that I think I must take her along to Norway!"

Of course she should not have said that. The next day the mother came with little Norin, her cheeks shining of coconut oil, adorned with dress and flower in her downy hair: "Please! We have talked about what you said yesterday. Now she is yours."

Good heavens, what had we done? Stammering in a desperate attempt not to offend the giver of this unbelievable gift, we eventually managed to explain that even if there was nothing we would rather do than keep her, we could not take Norin from Manase where she was so comfortable, to Norway's cold climate, which she would perhaps not endure. . . . (Dammann 1981: 27)

The episode is copiously rendered so as to make concrete the difficulties in adapting to a deeply different culture. These are such that one must comprehensively and profoundly change oneself—albeit, as a cultural anthropologist, only temporarily—if it can be done.

Cultural anthropologists sometimes write in a way that many readers misunderstand. The readers get the impression that the anthropologists pretend to be able to *understand* many widely different cultures, while at the same time maintaining their own cultural identity. What is pretended, presumably most often, concerns a limited number of schematic cultural structures rather than cultural contents.

Most likely, one's perspective on anthropology must change when one acquires an intimate understanding of a culture that is profoundly different from one's own. It is difficult to understand how an anthropologist could be

able to walk in and out as an ordinary tourist and hence keep one and the same frame of reference for his own field, so that after a dip in ten different cultures he or she could compare them within the old framework. Even a talented actor may have difficulties keeping the same perspective after performing in different dramas.

Back to Manase. Since gift situations of the kind exemplified can to some extent be isolated, the difference does not necessarily involve the culture as a whole. It is deep, but not all-pervasive. Consider an example of depth, but without maximal scope — also from Manase:

The children's importance as helpers in work is also connected with an interesting Samoan tradition.

If the exploitation of the children's working capacity goes too far, they can move to another palm hut and simply choose a new pair of parents as their closest guardians. And because the children's work contributes to the provision of food as well as diminishing the workload of the adults, they will always be welcomed with delight.

What, then, if the original parents refuse to accept the child's change of residence and come to fetch the child? Then the new parents will refer to *fa'a-Samoa*, the old Samoan customs that give the child the right to make up its own mind about where it will live. The parents will be turned away even if they have the highest chieftain-titles in the village.

This right gives the children protection, also against brutality, neglect, and injustice. In a society where the borders between the families are as diffuse as in Samoa, it is not a difficult breakup for a child to move to an uncle or aunt in the same *aiga*. (Ibid., p. 128)

If we concentrate on the right of a child to choose with whom it will live, this can to some degree be isolated as a deep difference that does not penetrate all life's conditions. This trait, however, is intimately connected with something all-pervasive about the family structure: everyone within an *aiga* has such close relationships that they border on family relations. Thereby the concept pair 'home/dwelling' is also affected. Furthermore, the work concept is affected. The parents have a right to require the children to participate in work, but otherwise they meddle very little. In Norwegian society, adults and children seldom work together, and the demands on children rise with full force outside of work.

Within cultural anthropological and philosophical literature there is today (the third quarter of the twentieth century) not much discussion of

how one could systematically compare depths of the kind addressed here. It is, therefore, worth the trouble to seek somewhat greater clarity.

Let us say that we want to know how profoundly our conceptions are compared to those we (purport to) encounter in Spinoza's *Ethics*. In the *Ethics*, part 3, note to theorem 11 (IIP11S), Spinoza says that joy (*laetitia*) is a feeling (*passio*) whereby the mind (*mens*) increases its perfection (*perfectio*). This conception of joy is explicitly used as a premise in twenty-three later proofs in the *Ethics* and in two explications (*explicatio*). If another system builder differs in his interpretation of *joy*, *mind*, *increase*, and *perfection* as these terms must be comprehended in the note about joy, then all twenty-three proofs and the two explications become the focus of attention. The possibility exists that the divergence in interpreting IIP11S has as a consequence hundreds of other differences in understanding. The disagreement in interpreting IIP11S is thereby a symptom of a relatively deep difference between the two systems.

If, on the other hand, the disagreement concerns Spinoza's definition of the essence of benevolence, the disagreement is likely to have few consequences, because benevolence affects very few elements in the *Ethics*. There is no basis for the assumption that the divergence is deep-seated.

Hence, we may take the position that the differences within the chains of justification represent an index for the depth of difference between two systems. If *A* is justified by *B* (and not *B* by *A*), then to say that a difference in standpoint *B* lies *deeper* than a difference in standpoint *A* means that: the difference yields *greater* consequences as more of the system is justified by means of *B*. The index is of interest even if the exact order of the links in the chains within a system that is well worked out logically is to some extent arbitrary.

According to part 1, theorem 14 (IP14), there can be only one substance (*substantia*), namely God (*Deus*). Only three other elements in the *Ethics* depend *explicitly* on this theorem. One of these elements is the proof of IP15, that whatever is, is in God, and that nothing can be or be conceived except by God. Implicitly, IP14 is used in substantial parts of the system insofar as a disagreement — such as Descartes's claim that there are two substances — would decimate much if the proposition were to be included in Spinoza's system. Some would say that including Descartes's claim would destroy everything. I will return to this thought, but for the time being let us assume that the theorem about joy could still stand.

A closer comparison of Descartes's doctrine of two substances with Spinoza's doctrine of one substance must, I believe, result in the conclusion that the difference is deep, although not necessarily abysmal.

The all-pervasive characteristics of a culture exclude isolation of economy, religion, and so on as parts of a culture that can be understood by themselves. The untenability of such a division is clear from other points of view as well. Consider Gregory Bateson's statement (1972: 64) that "Our categories 'religious', 'economic', etc. are not *real* subdivisions which are present in the cultures which we study, but are merely *abstractions* which we make for our own convenience when we want to describe cultures in words." Something similar applies to philosophical systems. The untenability is most noticeable when solutions to special problems are compared, such as solutions to "the free-will problem" within different philosophies. There is no precise problem that is common, because the conceptual words *free will* and *liberum arbitrium* stand in different relations within essentially different systems. The subject divisions in ontology, epistemology, methodology, and so on are expedient for our own surveys (which, of course, are part of our own system).

We are likely to find that, superficially seen, cultural differences and differences in philosophical systems have little to do with each other. This is because one *lives* in cultures, but not in philosophies. One has difficulties in adapting to cultural differences, but questions about adaptation do not arise in philosophies, presumably.

Since philosophical systems that are not fragmentary contain ethics and the many parts of social philosophy, they comprise philosophy of life and philosophy of culture—but they must not be confused with encyclopedias. They do not go into the details. The difference, however, is more formal than real in regard to comparison with cultures, for if it is not presupposed in the development of a philosophy that it is to be fit for use in practice, and hence in concrete life situations, then it becomes pointless.

Let us say that two norms in a radically simplified philosophy can be expressed by "Be honest!" and "Be friendly!" In practice, countless norm conflicts ("collisions of duties") arise. They may, as they arise, be solved in various ways. One can even, by constant modification of the definitions of *honest* and *friendly*, claim that conflicts cannot arise. The abstract, general norms "Be honest!" and "Be friendly!" lose meaning if they are not conceived to be applied in concrete life situations. Indeed, it is only through a

concrete context that a person can be judged as honest—or can be anything at all. Ask Søren Kierkegaard!

Thus, in a philosophy with abstract, general norms and hypotheses, it must be assumed that they acquire meaning in concrete life situations. Even if they do not say anything concretely about this, it must be assumed that given a concrete (singular) situation, philosophy *A* will provide guidance for a behavior that is either similar to or different from the guidance offered by philosophy *B*. Thus, the question arises, To what extent is it possible for one whose life is influenced by philosophy *A* to adapt to philosophy *B*? Moreover, in the case of philosophies, it becomes relevant to speak of more or less deep differences with respect to the kind of life that is led in terms of them.

I have up to this point dealt with the depth dimension without relating it to the question of whether comparison is possible at all. Is comparison possible except in terms of a grossly simplified semantics?

Within systems, some words and expressions are defined and others are not. Definitions often form chains: *A* is defined by *B*, *B* by *C*, and so on. Often a comparison depends on the occurrence of one and the same word, which is uncritically assumed to mean the same thing in the two systems. If it becomes clear or at least reasonable to assume—by the study of definitions or in other ways—that the word has different senses, the performed comparison with respect to subject, agreement, and validity no longer applies.

A concept 'semantic depth' with respect to a difference may be introduced by taking account of how deep-seated the difference is in the chains of definitions and the directions of precization. With sufficient depth, comparison disappears more and more.

If we then turn to a consideration of cultures as they are systematically represented in terms of the information model or Barth's model, analogous relations between deep and less deep differences may be drawn. Clearly, in the case of the Baktamans, the norms for the eight population groups' relations in ritual matters affect all aspects of their life. The norms are deep. The Baktaman world picture is influenced by them and by the ontology presupposed for the norms to make sense. A culture that has none of these norms, and of course there are many such cultures, will *as a whole* be different. The deep point-by-point differences in relation to ritual conditions

yield differences at all other points. We have no common basis for comparison. It is like looking for absolute time in physics.

Misinformation and secrecy among the Baktamans complicate the picture, since they appear to cause a general uncertainty about what is right and true.

The observance of taboo and secrecy thus takes on a new meaning, together with a restructuring of categories of purity and pollution. But this is not without profound epistemological consequences: deceit is not just an expression of opportunistic self-interest, or the supremacy and whim of senior men. It is shown to be a means to generate deeper truth. And some time, the question seems inevitably to arise: where does this end? Are there not secrets withheld in the 5th degree revelations, that create deeper knowledge for others by virtue of being withheld from me? And what about 6th, and 7th degree? Pursuit of true knowledge becomes like peeling the layers of an onion, or exploring a set of Chinese boxes: information on one level may be the deceitful cover that creates another kind of truth at a deeper level. How and when such doubts set in is hard to tell; but their eventual presence can be demonstrated. . . .

(Barth 1975: 82)

Such a strange and profound uncertainty about the innermost essence of reality is clearly a peculiarity likely to cause deep cultural differences. The same applies to the peculiar relationship between the sexes among the Baktamans, a relationship characterized by little trust. In view of the difference from our own culture, neither the concept of 'security' nor the concept of 'sex' can be taken as a point of departure for precise comparison. It is no use to reply that indeed Baktamans, Scandinavians, and all others eat, drink, and sleep and that we therefore have a lot in common. We are now concerned with how world and reality are perceived and felt.

Profound differences must be supposed to characterize the differences in *experience* of reality. The carriers of one culture can easily be imagined to be able to *suspect in which direction* the differences lie between their own culture and their experience of another culture. From this imagination to the point of actually experiencing the other culture, however, is a great leap. Is it possible at all? If it is possible, how can it be expressed?

The analogy between philosophical systems and cultures is clear: it seems possible to indicate both isolated differences and the direction in which deep differences influence entire systems, but to experience the

world in terms of different systems is another matter. It is natural here to emphasize the difference between knowledge of structure and experience. There does not seem to be any definite limit to how far one can mine the area of structural knowledge about differences, but in doing so not much is said about how far one can apprehend things in relation to experience. Structural knowledge will perhaps some time in the future be left to unfeeling robots. The question "How does the world appear to N. N.?" cannot be answered by abstract indications of structure, but perhaps it also cannot be answered by trying to *show* the world as it appears to N. N.

These sceptical statements should not be accepted without certain qualifications. They must be viewed in terms of questions—questions of degree, not absolutes. Even within a culture, indeed even within a family, there are similar limitations to insight into how the world appears "to the others," how it is concretely experienced. Here, however, we have some building blocks and elements. We can say, for example, that for my brother certain things are more threatening than for me, and we can indicate what those things are and train ourselves to perceive them as threatening. It is more difficult to take account of different second order attitudes—differences in reaction to the consciousness of being threatened. These color the very experience of threat, of course. Point by point, we must assume that we can experience the world in the same way as the others—but do these "points" really exist? When we are concerned with deep cultural differences, the distance from genuinely experiencing the other's world can be essentially much larger. The problems are not fundamentally different.

It is not unusual for people to say that this or that person or nation lives in another reality. For them, multiple realities exist. The terminology I employ distinguishes between *world* and *reality*. The expressions just mentioned will, in the terminology of this book, be rendered by saying that the person or nation lives "in another world." By "reality" I understand something that I cannot conceive to permit plural "realities." The most different cultures are all within one reality, and it is this that they seek to comprehend.

The Absolutization of One's Own World Picture

The absolutization of one's own cultural content ordinarily takes natural and innocent forms in cultural anthropology. Northwest Greenland, for ex-

ample, is the most hostile milieu human beings have ever lived in, according to Fred Bruemmer (1981: 43), but the Polar Eskimos did not *think* of it as especially hostile and called it Nunassiaq (the beautiful country). In the introduction to his work on the Azande, Evans-Pritchard (1937: 20) says that he wants to examine ideas which "though they do not accord with reality, are yet of supreme importance both to Azande and to Europeans resident among them." He cautions his readers, however, against believing that the Azande, in addition to expressing mystical thoughts, cannot also express themselves commonsensically and act empirically. They are not in such a bad way as one might think.

The world that Evans-Pritchard refers to simply as reality, can, we presume, hardly be distinguished from the reality he himself has been brought up and trained to understand. His ideas about the opposition between mystical and commonsensical thought and between ritual and empirical action would hardly be accepted in a cultural anthropology prepared by the Azande in Azande country.

Consider another and perhaps more controversial example: Keesing (1976: 142) suggests that one reason why we do not abolish discussions of cultures is that we are inclined to strip meaning from what we call physical reality. He asks why we must talk about this shadowy hypothetical entity 'culture' that we can by definition never observe or document? Why can't one simply investigate actions and events in a social system without invoking a metaphysical entity such as culture?

Keesing maintains that we must invoke such "metaphysical" entities because, for example, we cannot understand behavior without assuming that it has meaning. We cannot *measure* the difference between a wink and a twitch in the face. He suggests that much of what we perceive in the world, and dress up with meaning, is not in the physical world at all (Keesing 1976: 142). The existence of a physical world without meaning has been a feature of European cultures since the fifteenth century. In Keesing's remarks we also find hints of a methodology and theory of concepts that are not completely intercultural or culturally neutral. These remarks, however, cannot as a matter of course be taken as criticism. We are now in the midst of the problems of system comparisons.

If one requires that the cultural anthropologist compare cultures without using his own culture as an absolute reference, then we automatically enter the problem cluster concerning "the third system": a kind of superan-

thropology that permits one to survey the multiplicity of cultures without oneself taking a position on any one of them, indeed without being locked up in their terminology and conceptual structures.

How, then, is the cultural anthropologist to proceed? In his short paper, Barth (1977) mentions essential points about "the method," the way to proceed. An essential step is to avoid the ethnocentric point of departure and see all cultures, including our own, from the same comparative viewpoint. As a cultural anthropologist, one's perspective must be so broad that it acknowledges and respects the agent's own premises as well as the researcher's, so that each culture can be understood as a world alternative to our own.

These sentences open up enormous philosophical problems, but here I shall limit myself to a short comment. Let us designate the cultural anthropologist's culture by A and the series of n cultures he or she describes in research by B_1, B_2, \dots, B_n . The anthropologist could presumably then be said to perform a series of descriptions (b_j): $b_1(AB_1), b_2(AB_1), \dots, b_n(AB_n)$.

For example, in terms of A (her own culture), the anthropologist notes: "Her uncle enters by the door and greets." In terms of the agent's own premises and world picture, however, it is no uncle in A 's sense who enters, nor any door in A 's sense, nor even any greeting. What then actually occurs in the world designated as B_1 ? That can only be discovered little by little (insofar as it is accessible in terms of A) through thorough and prolonged "participant" observation. Each of the descriptions $b_1(AB_1), \dots, b_n(AB_n)$, then, will gradually change in reference to the B_1 component. These changes in description, however, will have to be formulated directly (by use of *and*, *or*, and so on) or indirectly (by definitions and other rules of use for *uncle*, and so on) in A 's language. In short, the descriptions, b_1, \dots, b_n , are registrations of life in all the cultures observed by A, B_1, \dots, B_n , in A 's language.

One question that arises immediately is whether all cultural anthropologists must belong to one and the same culture, for example Fredrik Barth's. If each culture can in principle produce anthropologists as genuine members of the culture, then we may envisage a multitude of registrations $b_{ij}(B_i B_j)$ written in the language of each culture. If not all cultures can produce anthropologists, then questions arise about the "fundamentality" of the cultures as well as about the intercultural status of science—to put it

somewhat abstractly for the present. This comment is only meant as a philosophical aperitif.

Back to the method, however; since the cultural anthropologist must, at least at the beginning of his studies, be presumed to be a member of some culture, he or she employs a frame of interpretation characteristic of that culture. To this frame of interpretation belong special culture-specific concepts, points of view, rules, and patterns of behavior. This at least seems to be the situation, judging from Barth's statements (1977). He writes that social anthropologists must seek, in the manner of researchers, to define their own frame of interpretation for the global cultural variation in terms of the character of the data, and preferably try to develop it (or "transcend" it) to attain a deeper understanding of reality.

The term *define* here is presumably very close to *delimit*. One must become aware of one's own provinciality incrementally — to say something in another way. The more one attains insight about a multitude of other cultures, the more exactly and deeply one can articulate the characteristics of one's own culture. Thereby, one's own culture can be "transcended" in a certain sense, but exactly which one? Does one break away from one's own culture? Does one find a more and more universal frame of interpretation?

The latter might conceivably involve anthropologists from the cultures B_1, \dots, B_n bringing forth a common language that becomes comprehensible only after study of global cultural variation. All the descriptions of the cultural anthropologists would then in principle be directly comparable, and they might even correspond.

These possibilities, however, raise enormous questions, such as those in ideology research that have been taken up by Gerrit Huizer and Bruce Mannheim (1979) and others. Can the researcher manage to stand outside each single culture? Must the researcher create his own type of culture, which has the special characteristic of being capable of acknowledging and respecting all cultures in terms of its agent's premises? This amounts to a requirement that each culture must be understood in terms of its own pre-suppositions.

It is natural in terms of a priori philosophy to reason as follows: There are demonstrable limits to the differences between cultures. In order for something to be understandable as human culture, the members must *think*, but there are universally valid laws of thought, such as the principle of contradiction. If these laws are violated, we cannot acknowledge that

thinking is present. Hence, by virtue of classifying something as a culture, we will already have claimed implicitly that the members intend to follow the same laws of thought as we do. They will thereby also have to presuppose the same concept of truth. The members of a culture, including our own, may fall into self-contradictions, and deviants may *claim* to have another concept of truth. Nevertheless, that does not affect the core of the matter: cultural differences do not penetrate so deeply that they affect logic and truth-concepts.

This view may be said to absolutize logic and truth-concepts, and insofar as one maintains that these topics are unique to our own culture and unavailable to those that are still often called primitive, it involves an absolutization of one's own culture. A very clear defense of this view is delivered by Steven Lukes:

It follows that if *S* [some assumed culture] has a language, it must, minimally, possess criteria of truth (as correspondence to reality) and logic, which we share with it and which simply *are* criteria of rationality. . . . But if the members of *S* really did not have our criteria of truth and logic, we would have no grounds for attributing to them language, thought or beliefs and would *a fortiori* be unable to make any statements about these. (Lukes 1970: 210)

This standpoint may well seem reasonable to all except two groups: (1) those who hold with Lévy-Bruhl that "the primitive" reveal "prelogical" thought, and violate the principle of contradiction, but may nevertheless be subjected to detailed cultural anthropology, and (2) those in our culture who are specialists in logic or the problems of truth-concepts.

The latter group is the most interesting from the viewpoint of this book. Just as one can distinguish between purely formal geometry, which does not say anything about points and lines, and physical geometry, which does, one may distinguish between purely formal logic, which does not say anything about whether something is logical or not, and natural logic, which does. Natural logic is not independent from what is supposedly meant when it is said that something represents "rational" thought. There are a great number of different formal logical systems, and much debate concerning which ones are most fitting as natural logics. Moreover, even among these logical systems, there are often many versions. This pluralism can be understood in different ways. The simplest one for me is to empha-

size that when we say that a set of thoughts does not violate the principle of contradiction, the depth of intention is not infinite—very different precizations may seem meaningful. Hence, we have no grounds for postulating that there *must* be one and only one universally valid logic, although we must then add that this does not imply that they all contradict each other. On the contrary, great difficulties in principle militate against proving or disproving contradiction among logics.

In the matter of truth-concepts, the debate is just as open. Pluralism seems to me the most reasonable conclusion, but it is not accepted by all specialists.

We can conclude from this that logic and truth-concepts do not represent unique, fixed structures that constrain cultural difference and pluralism. Euro-American cultural anthropology does not presuppose one entirely definite logic and one completely determined concept of truth. Furthermore, even if it did, that would not imply that cultural anthropologists in other cultures must have the same logic and the same truth concept. There do not seem to be any *a priori* limitations to the differences, but neither can it be excluded that future research will not find it reasonable to assume certain specific limits.

In view of this, there is scant basis for singling out our culture's rationality, logic, or truth concept as universally valid. Such an absolutization is not justified.

The philosophical problems in the border regions of cultural anthropology are perhaps not as multifarious as may appear from the preceding. For it may be that anthropology in the first place, primarily or as a minimum, seeks to investigate *the structure of cultures* rather than their content. One lives in a house, not in the structure of the house. Now one might object that the more one counts as part of the structure, the more essential an understanding of the structure becomes for the understanding of the house. This may be the fate of "structuralism": one seeks to include so much in structure that one fails, as a researcher, to describe the content adequately. The philosophical questions concealed here are very old and are most frequently discussed within the form/content terminology.

Whatever one means about the relationship between structure and content, it is clear that cultural anthropology has enormous and fruitful tasks in the matter of clarifying and comparing structures. This corre-

sponds to comparison of the structures of philosophical systems, such as the *in se/in alio* conceptual structures from Aristotle to Leibniz; autotelic/heterotelic value in the theory of values; and induction/deduction in methodology. The more studies of structure dominate, however, the more cultural anthropology will perhaps turn into (general) sociology, or it will become an explanatory science that those who live in a culture cannot recognize as a description of their own experienced world (see Claude Lévi-Strauss's research program).

By concentrating on abstract structures, researchers may perhaps make fundamentally different cultures comparable. The "small society of Bushmen in the Kalahari" and the "metropolis of London" have, for example, some linguistic structures in common—at least more in common than the most dissimilar languages known to date. The common structures at this abstract level are, of course, not known to the ordinary members of the different cultures. One of our problems is the cultural adherence of the comparative cultural anthropologist.

Like a philosopher working with his system, the members of a culture change it themselves. Like philosophies, cultures constantly reveal influence from outside. All cultures seem to be constantly changing; and most cultures are, in spite of their differences, dependent on others. This also applies to philosophies, which are in part developed in conscious opposition to others.

Each philosophical system and each culture construed as an all-inclusive system of meanings seeks to articulate reality itself. One finds as part of any developed system a doctrine of other systems. They are at first degraded to world pictures, but then it dawns on us that indeed our own system can provide only one world picture among the many. Which world picture, then, is the one that gives a correct representation of reality? There does not seem to be a possible answer to this question. We cannot step out of our own skin. Since the individual systems do not confine themselves simply to forming pictures, but rather attempt to give a direct expression of reality, the question can also be put thus: which world is the real world?

It should emerge from the preceding that the question cannot be made more precise beyond a certain limit. Among the not very precise answers that can be given, however, I find the following: any world is real that gives an all-inclusive content of meaning; the others suffer from the unreality of the fragment.

What Kinds of Cultures Can Develop a Cultural Anthropology and How Different Can These Anthropologies Be?

As soon as a culture has contact with one or more others, attention is (unavoidably?) drawn to differences in accepted practice. This is when the beginnings of cultural anthropology emerge. The culture carriers in Azande country make expeditions to other countries to fetch oracle poison. A man who reaches the Bomokandi River is aware of differences in customs, says Evans-Pritchard (1937: 277). Quite a number of people have reached the river, obtained poison, and brought it back wrapped in *ngbongborongba* leaves. This contact with other cultures has changed the Azande's own culture. Their conceptions about "the others" form the rudiments of a cultural anthropology.

One way of generating greater clarity regarding the absolutization of one's own cultural background is to try to imagine how a cultural anthropology might look within cultures that at least apparently do not have, and have not had, such a cultural product. As for the Baktamans, they have been so isolated that they have had only a few external contacts, and these have been almost exclusively limited to people with similar cultures, for example, the Seltamans. In recent years, the Baktamans' contacts with industrial cultures have increased, and their basis for developing conceptions of these cultures has expanded in step. Fredrik Barth himself has contributed to this situation. It seems clear to me that the nascent "Baktaman cultural anthropology" will differ deeply from our own, so deeply that some researchers may refuse to employ the designation. Here differences in philosophy of science are at play.

At the turn of the century, Euro-American cultural anthropology was so filled with cultural arrogance that the promoters of science hardly imagined the possibility of another form of cultural anthropology than their own. Now comparative history of science is an established field, although it tends to concentrate on a few Eastern and Western cultural traditions—Chinese, Indian, classical Greek. A pluralistic, or even possibilistic, view of research and conceptual structures in general, opens up the way to a relativization, or rather a relational view, of the conception of deeply different cultural anthropologies as parts of deeply different cultures.

Presumably, strong expostulations against approaches at variance with occidental anthropology manifest themselves even today, especially among

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those who still speak of simple and “primitive” cultures. To be “primitive,” says Bryan Wilson (1970: xviii), a sociologist solidly acquainted with anthropology, is to stop being an anthropologist, “since anthropology operates according to norms and values that do not inhere in primitive society.” Many traditional societies, however, have more or less lively dealings with others, and there are therefore also traditional forms of doctrine about “the others.”

The strong position of cultural anthropology today in the industrial societies perhaps results from the fact that we maintain the dominating distinction between nature as meaningless raw material and culture as human projections of meaning into the meaningless. More uniform conceptions of nature, man, and world undermine the potential for a kind of isolatable science of cultures.

VI

Some Conclusions

In the preceding chapters, I have tried to answer some relatively precisely posed questions with relatively precisely formulated answers. However, the questions (“Which world is the real world?” and so on) with which the investigation began are themselves not precise—they are vague and ambiguous. They are compelling because they are engaging, disturbing, universal, and demanding of answers. The relatively precise answers emerge by more or less arbitrary choice of certain special lines of precization and concept creation (for example, ‘system’). Therefore, they do not cover the questions in their natural wealth of meaning. The question arises, Could I offer brief answer formulations that cover the questions without introducing so much vagueness and ambiguity? What follows are attempts at such answer formulations. In keeping with the goal, the style is simple and dogmatic.

Most of the individual, numbered conclusions are divided into two parts: where there are entries, the first one concerns philosophies; the second, cultures.

- 1a. Is it possible to say something about everything, as a few great philosophers seemingly have tried to do? Yes, one must, however, add some technically philosophical, limiting precizations of the word *everything*. This is necessary to avoid certain logical paradoxes. We must avoid letting *everything* stand for *absolutely* everything and precize in the direction of “all *types* of things, collectively.”
- 1b. Is it possible for a culture to cover everything? Yes, if reservations similar to those described for systems are allowed.
- 2a. Is there any value in seeking to formulate such an all-inclusive system or to familiarize oneself with those that already exist? Yes. A person with his sensibility intact has a way of perceiving the world,

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himself, and “everything” that is viewed as related. It is valuable to obtain some degree of awareness of this total view. Our attempts at system formulation are often reassessed when we are confronted with deeply different philosophies. For the significant number of people with an intense need for conceptually articulated total views, it is necessary to go on working with existing system attempts as points of departure.

At this juncture, too, one must add caveats and make precisizations so that absolutism is avoided. However deep the sensitivity and however intense the search for both totality and consistency, one has to account for the likelihood that some things will be fragmentary, inconsistent, and changeable over time.

- 2b. In most cultures, it is regarded as valuable to nurture an awareness of one's own culture, in part for reasons that have to do with internal oppositions and other relationships, and in part to facilitate the resolution of confrontations with other cultures. A picture of one's own culture strengthens its traditions, increases respect, and provides guidelines for changes. Just like the culture itself, the picture of it tends toward totality and completeness. Articulation of these pictures results in total views of the types classified by Jaspers.

It is doubtful that the preceding rather general and abstract statements about cultures can be made appreciably more precise without losing meaning or becoming inconsistent.

As with contradictory philosophies, cultures will contain conflicting norms insofar as deviants from a standard are characterized by an inconsistent totality. The “criminal” and the “countercultures” within a culture belong to the culture.

- 3a. Can a philosophy in a meaningful way purport to be true and binding for anyone, at any time? Yes. It is not necessary to accept a relativism with respect to validity. The statements “All standpoints are historically conditioned” and “All fundamental propositions in a conceptually articulated view of life are subjective” are examples of meaningful system statements that for certain plausible interpretations are intended to be true for any subject at any time, regardless of the historical period.
- 3b. If a verbalized, systematized direct expression of a culture contains statements about the relativity of cultural values, these statements

are to be considered as genuine partial expressions of the culture. The same applies to appended statements to the effect that the statements about the relativity of cultural values themselves have culture-relative validity. The last link in such a chain of meta-metameta- . . . statements, which represents a direct partial expression of the culture, is nonrelativized and without reservation.

This point of view is controversial. If we assume that we can step out of our culture and *consistently* see it from the outside, we delve into theories that address the possibility of culturelessness; these are not to be discussed here.

- 4a. Insofar as a system characterizes itself as one all-inclusive system among many possible ones, relativism is admitted. The pretension of being the only true system cannot be taken as absolute. The original characterization of the system can be made precise only in terms of its own conceptual net; thereby an inconsistency arises. The solution lies in moderating the pretension that “everything” is encompassed (see point 1). Attempting to sketch cultural anthropologies within the conceptual frameworks of other philosophies, thereby admitting the relative validity of the system, also leads to inconsistency. The absolute pretension of the system must be moderated, but it can pretend to be “nearly” total, which can be made precise by the relation “more comprehensive than.” This relation replaces the predicate “all-inclusive.”
- 4b. A cultural anthropology may, at the outset, state that the description of all other cultures occurs in terms of the conceptual framework of one all-inclusive system.
- 5a. Are there all-inclusive systems? Yes, those of Aristotle, Aquinas, Spinoza, Hegel, and others. By virtue of forming a sentence with the pretension of articulating something conceptually adequate in the form of a proposition, we have already pretended to have a system. Conceptually adequate articulation requires logic, methodology, and other disciplines that in our time, as in a thousand years past, dominate the system formation. These disciplines, however, can no longer be expounded and justified in isolation from other types of problems.
- 5b. A culture, as defined in this work (see chap. 5, pp. 106–16), in contrast to philosophies, is all-inclusive. It must be emphasized, how-

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ever, that cultures are constantly changing and that they cannot be “carried” by any single individual.

6. Are the five formulations that are implicitly asserted by the preceding “yes” answers true for all plausible interpretations? No.
7. Are there limits to the formulations’ precisability, to the depth of intention, and to the fine-meshedness of the net of discrimination? Yes. They have validity and adequate meaning only to some extent. There are, however, also limits to the specifiability of the limits.
- 8a. Can an (approximately) total system be expounded in a manner that is exactly and entirely comprehensible to the outsider? No.
- 8b. In the case of cultures, the limitation in both the exactness of the descriptions and the depth of the understanding appears more distinctly. No single concrete thing or event is the same in two deeply different cultures. Culture *A* is, to begin with, expounded in *B* in terms of *B*’s framework; culture *B* is expounded in *A* in terms of *A*’s framework. Constant improvement of descriptions is likely to improve and further refine understanding, but still only within limits.
- 9a. Can one give a continuously more exact and comprehensible exposition? No. Understanding seems to have to be developed discontinuously, stepwise, precisely at the most decisive dividing lines.
- 9b. Point 9 is more obvious in the case of cultures.
- 10a. Can a total system be improved? Yes, but only by internal changes, in terms of the goals and standards given by the system.
- 10b. The term *improvement* is not so apt in reference to cultures. A culture has internal oppositions and inconsistencies that constantly challenge efforts aimed at richer and more consistent development.
- 11a. Can a total system be open like a science? Yes, but the openness of the sciences also has limitations in principle.
- 11b. The openness of a culture is obvious, but where is the borderline between further development of one culture and transition to another one? Even with a very wide border area, the answers will seem rather arbitrary, even if we seek to relate them to particular, tolerably precise concepts of ‘culture’.
- 12a. Do all total systems refer to one and the same reality (in a wide sense)? Yes. Any philosophy aims to discover reality through what it states. To assume that different philosophies speak of different realities

will not help us understand the differences in what they assert. It is better to say that the pictures they provide are different. The world pictures differ and none of the worlds as pictured can be identified with reality.

- 12b. A culture (in the form of norms and descriptions) has the same kind of relation to reality while simultaneously delimiting a "world lived in"—an experienced world and a way of living.
- 13a. Can two systems be compared exactly and intelligibly for an outsider? No. The greater the exactness and genuineness of the representation, the smaller the degree of comparability. This follows from point 8.
- 13b. The same applies to cultures.
- 14a. Do systems fall within fixed categories, and can the range of variation be exactly indicated? No. The more exactly a categorization (classification) is performed, the closer a typology of systems comes to being part of a particular system.
- 14b. The same applies to cultures.
- 15a. Similarity and difference in the *structures* of systems must not be confused with similarity and difference in *content*. The content must ultimately be precized as experienced content. An exposition of a philosophy conveys how reality is as it is experienced through that particular philosophy.
- 15b. Cultural anthropology as a science needs testable statements, and these must exclusively or predominantly refer to structures. As cultural content, however, its value depends on the experience of the contents of the cultures. If this were not the case, the anthropologist could in principle be replaced by a robot.
- 16a. Results of scientific research constantly change our world picture. What does a total system do? It changes the totality. It changes everything. Scientific research changes something, but only within a philosophical framework. Intimate familiarization with a system involves incorporating a total view in which the results of research are interpreted and reflected in society. Thereby research and its results also change.
- 16b. Insofar as the results of research are intercultural, they provide guidelines for verbal and nonverbal behavior, but they do not deter-

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mine behavior and they do not delimit the individual contents of experience. Hence, as part of deeply different cultures, “the same” physics will differ in regard to content.

Over time, a cultural change has an effect on everything, including science.

- 17a. How can it be correct, strictly speaking, to say something about systems in general? It cannot be strictly correct. Language, however, may be used to suggest and guide the attention without leading it to any definite object. The statements of the general doctrine of systems, if one could give some hints about systems with such a pretentious title, cannot be precized beyond certain limits.
- 17b. The same applies with greater clarity to statements about cultures in general.
- 18a. From the idea that “much might have been different,” Leibniz and other thinkers have introduced concepts of ‘possible’ and ‘imagined’ worlds in contrast to the actual one. Does scientific research or philosophy have the means to designate some specific world as the *actual* one? No. Factuality has criteria that vary according to the standpoints adopted in ontology, epistemology, methodology, and other fields. This does not reduce the usefulness of the distinction between possibility and actuality as long as a specific frame of reference is presupposed and not taken as absolute.
- 18b. These points of view apply to cultures as well.
- 19a. Does scientific research or philosophy have the means to single out some particular world as the *real* one? No. No indicated fact can be a common point of departure for all philosophies. Furthermore, the criteria of reality contain evaluations or norms of a more profound kind than merely instrumental ones. Even if all philosophies sought to accept the same norms as points of departure—for example, the same rules of inference—this would not imply that other rules must be excluded.
- 19b. In the case of cultures, it is even clearer than with philosophies that we cannot point out one culture whose norms and world description depict reality itself.
20. Does this mean that human knowledge has specific limits? No. Our vague negative conclusions cannot be strongly precized without ei-

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ther losing universality or becoming meaningless or inconsistent. Hence, they cannot be said to draw any definite borderline for the limits of human knowledge. We have no objective grounds for clearly delineating what the borderline should border on, and the very distinction between "real" and "possible," as used in the preceding, is itself problematic.

Notes

Chapter I: Descriptions of Maximally Comprehensive Perspectives

1. "We use the term *world pictures* to refer to that which for human beings has object character, that which confronts the mind and is conceived of as expressions of the power of the mind. World pictures are capable of being described as something objective in the same way as rooms or houses; spiritual kinds of views have such pictures as their essential form of expression and their necessary condition, but the nature of the forces creating worldviews is such that it does not always express them as object-directed, they are also expressed as value hierarchies" (Jaspers, 1919: 161).
2. This quotation from Koestler (1949: 68) is used by Polanyi in a similar context (1952: 218).
3. A Cretan says: "All Cretans always lie." Is this sentence true?
4. Russell's antinomy concerns the class of all classes that are not members of themselves. Is this class a member of itself? If it is, then it is not, and if it is not, then it is. Hence it both is and is not a member of itself: a contradiction.
5. For a criticism of the doctrine that different logics can be described in the way attempted by some social scientists, see Naess, et al. (1956: 203 ff.).
6. See chapters 1–4 of *Outlines of Pyrrhonism* in Sextus Empiricus (1933–44). Most authors connect the word *scepticism* with Academic scepticism, or more pertinently, negativism. The Pyrrhonic sceptic, however, can subject all of his own arguments and their presuppositions to examination. But he may participate in the presupposition-research that thereby results without pretension of (true) knowledge of what the presuppositions are—indeed without beforehand *denying* (with claim to truth for the denial) that a philosopher who says that not *everything* has presuppositions may be right.

The one who does research seeks. The one who seeks can stop and ask himself, Exactly what am I looking for? If it is a thumbtack, the answer is rather simple. But if it is the "presuppositions of all questioning," it is not simple. If he stops long enough, he gets at least one new task of research.

For the external spectator, it may often seem as if the questioner, by us-

ing certain distinctions in the question-formulation itself, presupposes that certain answers are *definitively* decided, but the questioner does not need to have a definite answer, he may have a questioning (not-definitively-deciding) basic attitude, an attitude characterized by the fact that any answer with him seems to acquire a nondefinitive status. For example:

Sceptic: Is *perleungetunge* a kind of cactus?

Dogmatist: Aha, you presuppose the existence of *perleungetunge* !

Sceptic: I read about them. I have neither time nor capacity to take a definitive standpoint to the truth of everything that I read. But I have much *trust*; that much I grant you willingly.

7. See Jaspers (1919); the fourth edition (1954) has an interesting foreword concerning the evolution of Jaspers's ideas.
8. For an example of such a debate, see the discussion between Naess and Sir Alfred Ayer on Empiricist vs. total views in "The Glass Is on the Table," which originally appeared in *Reflexive Water: The Basic Concerns of Mankind*, edited by Fons Elders (1974 [in SWAN VIII]).
9. A method for successively more adequate—and therefore complicated—presentation of an ethical system is described in chapter 3 of my *Gandbi and Group Conflict* (1974 [SWAN VI]).
10. Page references to the work of Nordenstam refer to his unpublished doctoral dissertation, "An Analysis of the Traditional Sudanese Virtues" (1968b).
11. Translation of "Erscheinungen für uns." The formulation of Hegel's system follows in the main Ueberweg (1898–1903, vol. 4).
12. Formulated in all essentials in accordance with the formulations in Ueberweg (1898–1903, vol. 4).

Chapter II: Comparison of Different Total Views

1. Generally, this cannot be claimed entirely, since the near-total systems perceive the other within the system (in the widest sense), and therefore do not reach "all the way" out to the outsider.
2. It is essential to believe that when one looks at the first page of the *Ethics* and reads the first sentence, one is confronted with "Spinoza's doctrine itself." It is merely required that one understands Latin.
3. On the different substance concepts of Descartes and Spinoza and their relationship to the things they derive, see an interesting but somewhat unclear note by J. Bennett (1965: 379–80).
4. Leibniz employs two of the Aristotelian substance criteria—substance as the place of change and substance as logical subject—but adds a concept of non-composedness. In the first paragraph of the *Monadology* he defines monad as

“nothing but a simple substance . . . ; *simple*, that is to say, without parts.” Elsewhere, Leibniz has stated that Spinoza would have been right if there were no monads and he also probably intended to stress the need for a substantial basis in the things different from God (see Leibniz’s “Refutation of Spinoza” in Leibniz [1908]).

Chapter III: Metaphysics as Exposure of Presuppositions

1. This section makes extensive use of David Rynin’s article “Donagan on Collingwood: Absolute Presuppositions, Truth, and Metaphysics” (1964), in which Rynin convincingly repudiates Alan Donagan’s (1962) narrow-minded criticism of the prolific and discerning British philosopher R. G. Collingwood. Donagan criticizes Collingwood’s *Essay on Metaphysics* (1940) on the basis of a kind of naïve empiricism seldom encountered among philosophers since John Dewey’s glory days. According to Donagan, systems can be verified or falsified by comparing them with experience: “The principle of theoretical simplicity enables us to subject not only answers to questions, but whole complexes of questions and answers, to the verdict of experience. When one whole complex is rejected in favour of another, its absolute presuppositions are rejected along with it; like the answers contained in that complex, they are rejected as false” (1962: 82). Here it is presupposed that when empirical tests are conceptually articulated, system differences will not result in articulation differences.
2. On the relevance of differences in truth definitions, truth criteria, and methodology, I have expressed my opinion in connection with the question of whether we *know* that norms *cannot* be true or false; see Naess (1962 [in SWAN VIII]).

Chapter IV: Can There Be, Ultimately, Only One Valid Total System?

1. If this is correct, one may say that Aristotle is about to make his basic view analytic. Perhaps it is a tendency of precisely the deepest premises that they seem obvious because they are analytical (for that purpose), while they simultaneously pretend to constitute the point of departure of a philosophy as a synthetic statement. If the statement is perceived as synthetic by opponents who question it, it is defended by being interpreted in such a direction that it becomes analytic. The tendency, however, can also be the reverse: one perceives something as analytic *because* it appears evident and fundamental.

Chapter V: Cultures Construed as All-Embracing Systems

1. Because Barth’s (1977) article is only five pages long, I have omitted page numbers in the many references that follow.

NOTES TO PAGES 109–117

2. In “Reflections About Total Views” (1964 [in SWAN X]), I try to show that whatever one asserts (or denies), be it ever so modest, one presupposes a total system in doing so.
3. The example is taken from Geertz (1973: 121), where the sentences in question are related to the conception, in cultures, of the basic nature of reality.

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Author's Introduction to the Series

At ninety-two it is a great honor to be still alive and to witness the publication of my selected works in English. Few philosophers have their work published in a series, fewer still receive this honor before they die. When I was originally approached with the idea of publishing my complete works, I was overwhelmed and overjoyed, but added that not all my books and articles were important enough to merit such an honor. Selected works? Yes, and I am extremely grateful for this initiative and the final result, which presents a representative selection of my work from the earliest to the most recent. [*The Selected Works of Arne Naess* are hereafter referred to as SWAN.]

My interest in philosophy began with Spinoza's *Ethics*, which as a seventeen-year-old I was fortunate to read in Latin. I appreciated Spinoza's grand vision and trusted him implicitly as a person. I accepted that human beings could, and should, have a general outlook with the grandeur of Spinoza's, but I recognized that our individual views on this grand scale will not be identical. Through the years I have realized that there is a splendid variety of interpretations of Spinoza (SWAN VI and IX). His texts are exceptionally rich. As the years have gone on, I have focused on how he leads us to realize we can increase our freedom and sense of connection with the world through strengthening and intensifying our *positive* emotions. For example, loving and caring for our place and others leads to an expansive sense of being part of a much larger world. Emphasizing hatred and anger, on the other hand, makes us feel smaller and isolated from the world. Spinoza, as I interpret him, would express this by saying that "We are as large as our love." Increasing our freedom as human beings leads us toward life in communities colored by friendship, sharing joy and sorrow.

AUTHOR'S INTRODUCTION TO THE SERIES

Before I left gymnasium [the end of secondary education] the headmaster asked me, "What do you intend to be?" My immediate answer was "A philosopher." In fact, I had already conceived of myself as one. I viewed the writings of many contemporary philosophers that I was familiar with, however, as vague and airy and certainly not as inspiring as Spinoza.

My doctoral thesis in philosophy of science was an effort to remind us that in science the content of a theory is not independent of research behavior—the activities of observing, confirming, disconfirming, and so on, and that these are set within a deep context of place, history, and culture. Later, as a postdoctoral researcher at the University of California at Berkeley, I studied the behavior of experimental psychologists doing animal research.

In 1934 and 1935 I studied in Vienna and while there became a member of the famous Schlick seminar, the main discussion group of the Vienna Circle. Their quest for clarity and cordial cooperation in pursuit of knowledge led me to appreciate that "What do I mean?" is an open question. I concluded that we never intend to express anything extremely definite, even in mathematics or symbolic logic. I saw the importance of using empirical methods to find out how we actually use certain expressions and sentences. I developed and applied a wide variety of such methods, which became part of the core for the empirical semantics that runs through my work. I continued to do this type of research into the 1990s, my last project being one in which I questioned experts and policy makers about their ideas of values intrinsic to the natural world (in SWAN X).

In one of my earlier studies, I reviewed about 700 articles from philosophers concerning their use of the word *truth*. For the most part, I found these unconvincing and soon started on empirical studies of the use of *truth* among ordinary nonprofessional people and schoolchildren (in SWAN VIII). Many philosophers seemed to assume that ordinary people hold very naive views about these deep matters. I found through research that, on the contrary, the views articulated by these "ordinary" people were every bit as sophisticated as those held by professional philosophers. This reinforced my conviction that, generally, we greatly underestimate ourselves. Much academic philosophy was narrowly focused and abstract. Philosophers who elicited interest in wide-ranging issues of practical and global importance, such as nonviolence and social justice, have in my lifetime said things that were considered creative, but often too far out. In

spite of consistent proclamations that science neither would nor could take over all the problems discussed by philosophers, I tried to argue in ways that reminded readers of science done as open inquiry, and I tried to emphasize that it is occasionally necessary to perform empirical research to illuminate or support a philosophical viewpoint.

My empirical and historical research led me to realize that there are no certainties and that there is a great diversity in our spontaneous experience as well as endless ways to describe and appreciate the complexities and values of the world. Thus, I realized that I am one of those lifetime seekers that the ancient Greeks called a *zetetic* (see SWAN II and VIII). From my research on scepticism and the foundations of science and logic, it became clear to me that pluralism (every event has many descriptions and possible outcomes), possibilism (anything can happen), and a healthy scepticism (always seeking the truth but never claiming it) make up the most consistent approach to respecting the perspectives and experiences of others, human and nonhuman.

From my empirical studies of semantics, and from my knowledge of several languages, I came to appreciate the complexity of communication. Being committed to Gandhian nonviolent communication, I saw the importance of avoiding dogmatism and fanaticism. One of the most important discoveries coming from this research, leading to the publication of my major book, *Interpretation and Preciseness* (SWAN I), was the insight that we cannot avoid values in any field of endeavor or research. There are no value-free inquiries or theories. Even if we refuse to express our values, this is itself an expression and choice of values. We must, therefore, be clear about our value choices and try to make them explicit. The choices we make, as Spinoza pointed out, shape the quality of our lives, and values emphasizing positive emotions or feelings are expansive and lead to our growth. We must become ever more aware of our choices and the values involved. Even pure logic assumes certain norms. Empirical research can shed light on these matters. My colleagues in philosophy often found my empirical work perplexing. I, in turn, grew to underrate the necessity of visiting great centers of philosophy, as I preferred to be close to or in the mountains.

When I visited the United States, it was mostly to climb in the mountains or walk and camp in the desert. On one fortunate visit, I dropped in at the graduate students' discussion room at Harvard.

AUTHOR'S INTRODUCTION TO THE SERIES

Speaking with students who were writing their doctoral theses in philosophy, I understood that my knowledge of contemporary philosophy, and of recent important contributions in its various fields, was narrowly limited to special themes of lively personal interest. Even in later years, the tendency to take personal inclination very seriously colored my contribution to the philosophical literature. As can be seen, though, from the titles in these *Selected Works*, my strongly felt interests span a rich variety of fields, philosophical traditions, and movements.

Since childhood I have experienced an intense joy in being together with animals and plants and in contemplating the immense evolutionary development of life on earth over millions of years. From an early age I also developed an intense love for mountains and for being in them. Much of my creative philosophical work was done at Tvergastein, my mountain hut in Norway (see SWAN X). My devotion to outdoor life is in the Norwegian tradition called *friluftsliv* (literally, free-air-life). In many respects, I approached philosophical and cross-cultural studies as if I were a field ecologist or naturalist. It was against this background that my work from the 1960s onward focused with close attention on cultural diversity, biodiversity, sustainability, and the deep ecology movement.

My work since the Second World War has been increasingly within movements such as those furthering social justice, peace, and ecological responsibility. During the war, I engaged in anti-Nazi activism, and from that time also in promilitant Gandhianism, a nonviolence that is not pacifist in the usual sense but insists that if it is a bloody fight for justice against injustice, we seek "the center of the conflict" and, if necessary, cooperate with people who use arms. During the Cold War, I participated in the "third side," against both communism and extreme anticommunism, for example, as the scientific leader of a UNESCO project bringing Marxist and anti-Marxist politicians and political science researchers together in an unbiased discussion of the essence of democracy and freedom. Some of the relevant publications are included in SWAN IX.

The broad spectrum of books and articles included in the *Selected Works* represents, in many ways, a chronicle of my passions and influences. The *Selected Works* record, albeit in an inevitably fragmentary way, one possible expression of these. My dream and hope is that some readers will be inspired by their sheer variety, and that young philosophers will be encouraged to let strong personal motivations steer their studies.

AUTHOR'S INTRODUCTION TO THE SERIES

Working habits vary. Some people write an article and go on to the next without looking back on the old one; others come back from time to time, radically revising and changing the old one. The latter is my way of working. Lecturing in many places about these subjects, I have found it natural to revise the old manuscripts until sometimes very little is left of the original. Therefore, I have always viewed my writing as preliminary; a year, five years, ten years after publication of the first editions I have itched to revise, *thoroughly* revise them. When my first book was printed in 1936, I went to watch the hulking presses printing out one page at a time. I was terrified, thinking of mistakes or some awkward sentences being duplicated again and again.

When I was offered the opportunity to have a selected-works series published, I immediately thought I would like to review all my work and ask how, from today's perspective, I might answer the difficult questions I had earlier attempted to probe. Such a task would have been a particularly difficult proposition, because although many of my books and articles contain new ideas, the ideas are often not developed as well as I might have hoped. But alas, I am saved—at my age there is not time for me to accomplish such a comprehensive reevaluation of my work; I do not even have the capacity to do it now in any case.

Who could contemplate undertaking a publishing project of such ambitious proportions? Douglas Tompkins, mountaineer, entrepreneur, protector of wilderness in Chile and Argentina, and creator of the Foundation for Deep Ecology, is such a person. "Miracle Doug," as I call him, likes the idea that the deep ecology slogans and the deep ecology approach were introduced by a philosopher. I am grateful to him for his firm conviction, inspiration, and great generosity. My gratitude, however, extends well beyond my thanks to Doug, to others who have supported and championed this project.

Quincey Imhoff, when executive director of the Foundation for Deep Ecology, supported SWAN with generous grants and other contributions. SWAN has also benefited from faithful assistance and cooperation in the preparation and editing of the manuscripts. The late Professor Ingemund Gullvåg prepared the initial translation of *Which World Is the Real One?* (SWAN III). Professor Alastair Hannay translated the first edition of *Communication and Argument* (SWAN VII) and offered invaluable suggestions for improving the readability of the first editions of *Scepticism*

AUTHOR'S INTRODUCTION TO THE SERIES

(SWAN II), *The Pluralist and Possibilist Aspect of the Scientific Enterprise* (SWAN IV), and *Gandhi and Group Conflict* (SWAN V).

Most of all, however, I am grateful to Harold Glasser, the series editor, and his assistant, Kim Zetter, who oversaw all aspects of the project from design to production. Glasser's unique combination of intellectual tenacity, attention to detail, mastery of my work, and cooperative spirit made him a natural to take on the monumental task of selecting and editing my works. Glasser not only labored to improve the English and clarity of each manuscript, but his keen ability to ferret out countless technical and pedagogical errors has resulted in substantial new editions of volumes II–VII that are both far more comprehensible and accessible than the originals. I thank him for his valiant work on this project, both during his stay in Norway as a visiting Fulbright professor, where we collaborated on a strategy for revising the previously existing material, and in the subsequent years it has taken to complete the project.

From the beginning of the SWAN Project in 1994, Alan Drengson has encouraged and helped to move this work forward in numerous ways. Especially in the last crucial stages of completing volumes I, VIII, IX, and X, his help and editorial oversight have been invaluable. Thanks for his devotion, good humor, and positive enthusiasm. Thanks to both Drengson and Tim Quick for their extensive bibliographic research. Thanks to Bill Devall for his support and encouragement and especially his help on the completion of volume X, *Deep Ecology of Wisdom*. Thanks to Anne Collins for her outstanding work as the copyeditor of the SWAN volumes. Thanks to George Sessions for his support and encouragement.

Last, but certainly not least, immeasurable thanks go to my wife, Kit-Fai Naess, who has worked beside me throughout the years to provide invaluable assistance, encouragement, and inspiration.

Arne Naess

2004

Author's Preface to This Edition

As soon as you start a scientific research project, a philosopher of science might interrupt you by saying, "Very interesting that you have implicitly made certain assumptions. . . ." You admit this, but defend yourself by saying that most of the assumptions can be tested, if deemed necessary. Naturally, the validity of the tests rests on further assumptions. The conclusion of an investigation may have the form "If the assumptions a_1, a_2, \dots, a_n are all correct, *then* such and such." When you negate or change one or more *basic* assumptions, you will get different total views. The scientific enterprise, including the Western and the classic Chinese and Indian, is a part of a culture. So is the above sentence and so is this one. There are always "presuppositions" in such undertakings, as R. G. Collingwood noted.

Both the pluralism and possibilism advocated in this book (SWAN IV) are motivated by feelings of claustrophobia that arise in me when I read about what science is "supposed" to be. There is too little room for the function of the wild imagination. I agree with the statement "In the wild is the salvation of humanity." The wild has in many ways reemerged with the demise of the view that a rigid form of determinism was assumed and shown by science. What has become apparent is that anything can happen (possibilism), and that the world we know through experience is incredibly rich and can be described in inexhaustible ways. Our spontaneous experience is rich and diverse, as is attested by the many languages and cultures in the world. This is one reason I am a pluralist with respect to the scientific enterprise. Science as wondering and inquiring is opened in many ways, but science as an enterprise is a social institution with many restrictions. Studies of science and culture have led us to see that openness and creativity are basic features of natural systems and human life.

AUTHOR'S PREFACE TO THIS EDITION

The latest conceptual cage some of us have run into is that offered by Thomas S. Kuhn. It arises from the historiography of science in general. Sentences like "The history of science shows that . . ." can only express knowledge in the form of "If we assume such and such, we may validly say that the history of science *shows* such and such." However, science as a complex enterprise is not a showcase. Moreover, as soon as you start to change any of the undemonstrated assumptions, new possibilities are seen, and then history looks different. You do not escape assumptions, but rather you live through a variation of them in a discontinuous way. You need not agree with me here. If you have time and energy, you can instruct me about some interesting assumptions I make in saying this. I might then afterward reveal some of your assumptions when you were talking about mine.

Arne Naess

2004

Author's Foreword to the First Edition

The motive for publishing the present study springs from numerous encounters through many years with people who feel coerced by “scientific results” to change their personal philosophy. They look on science as a vast machinery that produces correct views and opinions in a sort of vacuum—completely independent of its setting in a society or of the interests, motives, or purposes of those who attend to the machine, oil it, serve it, improve it, spoil it, or neglect it. There is, in fact, no such machine. More specifically, the scientific enterprise is *not* independent of the philosophic or general system-building enterprise. Only insofar as a person is autonomous and articulate enough to have value-priorities, action-priorities, ontological priorities, or views, do scientific results have a *rational* power of influencing—even drastically changing—attitudes, however basic. Only under such circumstances do the neat sentences expressing results of scientific research become sufficiently “juicy” in meaning to touch, in a rational manner, on personal situations. Otherwise, the influence is irrational—a kind of coercion, due to passivity, indifference, or simply crude misconceptions about the nature of scientific knowledge.

There is, in my view, no “scientific” worldview, no established scientific “knowledge” in senses current among those people I am talking about. Consequently, their efforts at personally creative world orientation should not be hampered by science. On the contrary, they should be able to make use of, and participate in, science—provided they “know who they are” to the extent of having at least provisionally articulated their basic priorities of valuation and action.

Because at a given time in a definite society only one or a small number of views on a certain topic are considered “respectable” scientifically, the coercion acts in the direction of conformity, spurious agreement, and

AUTHOR'S FOREWORD TO THE FIRST EDITION

other-directedness; in short, its effect is strongly impersonalizing and dehumanizing.

The sterility of the scientific enterprise in providing a worldview does not make it less interesting. On the contrary, many who turn their back on scientific activity do so through a misunderstanding and lose an invaluable opportunity for participating in a central human undertaking—indeed, one that in the long run may prove to be *the* collective enterprise of this remarkable species.

I have no great confidence in my ability to convince others of the tenability of the views put forth in this work. I feel I should stress, however, that only very special aspects of the total scientific enterprise are to be considered, namely its capacity for surprise, for unlimited novelty, and unlimited diversity of interpretation. Science has many other aspects, some of which may give the impression of inescapable, lawful development, necessity, and inevitability. It is the aim of history of science, according to one of the many legitimate conceptions of historical research, to make us “understand” that science *had* to develop exactly as it did, given the social and other forces operating at each moment. But it so happens that this monistic and totalitarian aspect is *not* the one I deal with in what follows.

With regard to the preparation of this book, I have many thanks to extend to the people who assisted me in the undertaking of this project. I am especially grateful to the Norwegian Research Council for Science and the Humanities for supporting my enterprise in its various phases since 1958, to Magisters Øyvind Baune and Nils Roll-Hansen for persistent discussion on how to make the central ideas clear, and to Dr. Alastair Hannay for invaluable assistance in making the manuscript readable to an English-speaking public.

I

The Impact of the New Historiography of Science

The Neat Image of Science

The general view of science in Western societies has been heavily influenced by certain opinions about the nature of scientific development and progress. These opinions may be briefly indicated as follows:

1. The mature sciences, such as physics and biology, are *autonomous*. Even if to some extent they answer questions within philosophical or other “nonscientific” traditions, they have now emancipated themselves for good. Philosophers may do “philosophy of science,” or better, “logic of science,” but not mix science and philosophy, neither extrapolating scientific results, like Herbert Spencer, nor crudely basing their semantical arguments on experimental evidence, such as I do. To mix science and philosophy is to succumb to a “category mistake.” There is, and must be, a discontinuity between the two.
2. The process of justification, testing, and validation of scientific theories, hypotheses, laws, and observational journals is a *rational process* to be carefully distinguished from the more erratic pragmatics and heuristics dealing with favorable conditions for discovery and invention of theories. Certain stateable, coherent rules dominate science in its supreme form—that of the systematics of science—as manifested in any authoritative advanced textbook. Scientific rationality is autonomous.
3. The sciences are *accumulative*—facts being added to facts and new, more comprehensive, and accurate theories being substituted for old ones. Theories tend to grow less different, less mutually inconsistent, and more comprehensive. This leads in the long run to a *unification* of science.

4. There is a parallel process of step-by-step *approximation to truth*. Although it cannot be directly tested, we are at least to assume there is a process of increasing confirmation, corroboration, and exactitude, sometimes expressible by increases measurable in terms of probability or certainty.

There are also discoveries that certain phenomena are physically, biologically, and so forth, impossible, adding up to an a priori part of science. The rest never reaches absolute certainty but attains higher and higher degrees.

As a result of this (very roughly indicated) image of science, combined with the tendency to accept that philosophical reflection is a kind of reflection sui generis, using logical analysis and leaving empirical investigations to the sciences, the education of professional philosophers does not generally include practical training in any science (except formal ones). This in turn strengthens the tendency to mutual noninterference and the tendency to underrate the extent to which science must be understood as a way of *doing* things. The noninterference also covers the philosophizing of scientists in what are generally considered naive "excursions" by professionals.

Compared with the history of art, religion, economics, or politics, the above image of the history and growth of science is conspicuous for its neatness or narrow rationality.¹

The New, Gaudy Image of Science

One of the first prominent historians to enter the field of history of science after World War II² characteristically opens the introduction to his lectures as follows:

Considering the part played by the sciences in the story of our Western civilization, it is hardly possible to doubt the importance which the history of science will sooner or later acquire both in its own right and as the bridge which has been so long needed between the Arts and the Sciences.

Perhaps in part as a result of the obvious influence of scientific research on the main development of warfare, one could discern in the postwar years a craving for a deeper understanding of science in its rela-

tion to general history and to modern industrial civilization. In particular, the intensive study of the history of science by professional historians or scientists with genuine historical training brought forth a wealth of astonishing details and new points of view.³

A revolutionary movement has started that still seems to be gathering momentum. With some simplification, one may speak of a *new, gaudy image* of science, stressing the human side of a human enterprise, and thus we find the rapidly expanding fields of history of science forming the main bridge between “the two cultures.” These fields provide a strong appeal to researchers in the humanities and even to professional writers such as Arthur Koestler.⁴ The development of science has become a colorful drama!

Impact of the New Image on Philosophy: Typology of Total Views

Aspects of the new image of science have already influenced philosophical research in some ways, but I suppose the main impact is still to come. In what follows, certain conclusions on how to conceive the dynamics of scientific thinking, including efforts of validation and justification, are formulated, but only insofar as they have important relations to certain philosophical positions in epistemology and ontology.

The examination is therefore far from neutral in choice of topics. I have had certain philosophical conclusions in mind during most of the argumentation. One of these conclusions is that sciences, when articulated with care, are like fragments; they constitute the most exactly formulated and interpersonally testable parts of existing, or more often potential, general views about man and the universe. Efforts to isolate “pure science” have failed and will continue to fail. Scientific revolutions, or more generally, any considerable change in scientific tradition, makes science (represented by the small elite of creative scientists) shift loyalties within the area of general (near-total) views.

History of philosophy, history of ideas, and contemporary philosophical movements furnish the most comprehensive examples and studies of general views. Philosophers who have been trained as scientists are able to make more explicit, broader frameworks of contemporary science and help work out *typologies of general views* with the ultimate aim of covering any

possible coherent total view, irrespective of whether it happens to fit in with the views dominant among the scientists of the contemporary scene. Facing the uniformities of technological civilization, the typologies serve ecological balance and diversity.

I say "typologies" rather than "typology" because the choice of categories or principles of classification (*fundamenta divisionis*) will be to some extent arbitrary. When near-total views comprise logic, methodology, and other basic fields of argumentation, their common ground, as far as it is articulated and conceptualized, diminishes, and therefore also the import of a critique of one of them based on any other. Near-total views may be considered to be valid, if coherent.

These preliminary remarks are intended simply to make it more understandable why I have selected below only certain topics for lengthy treatment and have skipped others altogether.

Claims of Refutations and the Historian: Echolalia or Critical Attitude?

As an example suggesting the direction in which the new historiography is moving, we shall discuss refutation and refutability.⁵

In describing scientific developments, part of the job of the historiographer consists in understanding and interpreting certain happenings referred to by scientists themselves in such proclamations as "theory *x* has now been *refuted*," "this law must now be considered *established*," or "my hypothesis is now *verified*." The historian cannot presume that "refute," "establish," or "verify" mean the same to these authors as they do to himself.

Various historical questions apply to all descriptions of a theory's loss of status. Exactly what was the relation between the theory and the *x* said by scientists or historians or both to falsify, refute, weaken, and so forth, the theory? (A relation of derivation? If so, what kind? Or perhaps a looser connection?)

Regarding the *x* itself, we find reference not only to "observations," but to "facts," "phenomena," "laws," and to other theories conceived to have been verified and now *believed* to be inconsistent with the theory or doctrine undergoing testing. The image of refutation as a simple "confrontation of theories with observation" is clearly not one that can be derived from a study of the scientific literature of the last centuries.

The disagreements among creative scientists as to the exact status of a theory at any given time makes it impossible for the historiographer to adopt the rule that a theory is refuted when a scientist with a solid reputation *in his community* says so. Application of such a criterion would often require us to announce a theory to be both refuted and not refuted, verified and not verified, established and not established, within one and the same time interval. But what of a rule telling us that a theory is refuted when *all reputable scientists who have recorded their opinion* say, or imply, that it is? This rule may be considered part of a comprehensive set of rules covering a vast number of happenings considered as normal features within scientific development itself. The rules may be roughly formulated and summarized as follows: when a presumably competent scientist says “*x* has now happened” and *x* is a normal part of the scientific enterprise proper, *x* *has* happened, and the historian’s job is not to make any critical examination or *report* it but simply to *repeat* it in his historical account (adding place and date).

Considering the scope and results of this rule, it might be called the principle of echolalia. The historian’s assessments are echoes of unanimous authoritative sayings. The historian tries to act as a mere servant, *ancilla scientiarum*, but he gets into serious trouble every time his masters disagree or there is a reversal of opinion.

A very different attitude is that of a historian who forms his own opinion as to what has and what has not been refuted, studying in detail the past situation at the frontier of science. The critical historian is in principle prepared to affirm that although this or that theory (or hypothesis) was unanimously *considered* refuted, it was not *in fact* refuted. The new, highly favorable external state of history of science permits a number of historians and scientists to delve into subjects deeply enough to take a responsible stand *within* the scientific debates of the past.

It is an important feature of the new historiography that the historian of science is considered to be just as free in his assessment of the truth-value and importance of any scientist’s utterance as the historian of politics is in relation to the utterances of politicians. Of course the historian is interested mainly in how the actors saw their situation, but he feels free to pass judgment, to declare them to have been completely deceived, deluded, in the dark. He applies the well-established methodology of textual criticism mercilessly and cynically to utterances of even the most

honored geniuses in science. Recently, the great physicists, who were active as recently as the 1920s, have been asked to "tell their own story." Needless to say, their memories do not furnish any exact, detailed, or coherent account of "what really happened," and the historians try to construct a survey or picture that is of a higher historical quality than that of any of the actors.

Applied to questions of refutation, the new attitude implies that if the historian can document ten scientists' opinions (within a certain time interval) concerning the status of a given theory, his job will be, on the one hand, historically to understand and explain the utterances and, on the other hand, to offer his opinion as to the truth-value, tenability, etc., of the utterances in the light of later and contemporary development and within the framework of a definite, or several definite, philosophies of science.

The historian who has studied recently at various centers of philosophy of science knows that there are deep differences of attitude and doctrine and that any attempt to make a coherent synthesis cannot but fail to do justice to any of them and can only result in a loose, valueless form of eclecticism detrimental to both history and science.

Therefore, explicitness as to his own idiosyncracies is one of the scientific author's greatest merits. But where it is lacking, the diligent student of the products of research will generally be able to see where the author "belongs."

Thus, if scientist *A* has declared *T* refuted, one modern historian, *B*, may find that *T* was not refuted (in the sense intended by *A*) but merely weakened, or perhaps even strengthened, or that the test did not affect *T*. Or, more likely, *B* may accept the verdict of *A*, insofar as it refers (of course) to *A*'s own concept of refutation, but reflect that it is a different concept from his own, according to which the event referred to by *A* may bear no relation (other than irrelevance) to *T*'s refutation. But due to the radical plurality of contemporary differences in philosophy of science, another historian, *C*, accepting at least one assumption or rule or postulate that *B* does not hold, may interpret *A*'s text differently and therefore arrive at a different reconstruction of *A*'s concept (intention) or, if not that, at different conclusions concerning both the correctness of *A*'s claim ("refuted!") and the relation of past events to what *C* calls "refutation."

In short, the contemporary historian of science no longer feels constrained to adopt the terminology and conceptual framework of the scientists he studies, or even all of those adopted by his own contemporaries, in regard to assumptions, rules, or postulates.

It is the historian's job to elucidate the way scientists conceived their situation, introducing us to *their* conceptual framework and, if possible, to their *praxis*,⁶ but he can only do this on the basis of his own assessment of the situation, his own impression of who were the actors and what were the forces operating upon them—scientific, economic, and political. Ernest Gellner's surmise concerning societies as a whole also holds for the “subcultures” comprising scientific communities:

[T]he general characteristics of societies, their culture and their language, which enable their members to conceptualize aims and beliefs about environments, and which consequently are presupposed by special explanations, are not perhaps correctly represented by the beliefs of those members themselves. (Gellner 1968: 431)

Do we understand the printed matter and correspondence of scientists of the seventeenth century better or more poorly than they did themselves? It is tempting to answer, On the whole, more poorly. But the formidable collections now printed or available in archives make it possible to interpret each sentence of each letter in the light of the collected letters and printings. Many misinterpretations by seventeenth-century scientists are next to impossible to succumb to today. The intense prejudices of earlier times, due to jealousy and other strong emotions, are gone or weakened.

Collections of pictures and old instruments make it possible to repeat experiments under better-controlled conditions. Chemists did not work with analytically pure chemicals but with grossly impure materials. Today we often know exactly from which minerals and by what process they obtained their strange “salts” and “spirits.” We are able to reproduce the impurities, and we understand better what they did and why they often got such queer results.

Yet there are, at any time, so many ideas “in the air” that are not articulated—but nevertheless influence the formulation and interpretation of words—that we must concede our ignorance or the crudeness of our guesses. And in any case, translation—at least in the ordinary sense of “translation,” which means a rendering of expressions in one contempo-

rary language into expressions in another language belonging roughly to the same family of languages—is out of the question. If, for example, in the correspondence on Spinoza's experiments with what he called “nitrum” we translate the word into either the Old English word “nitre,” or “ NaNO_3 ” or “ KNO_3 ” or “a mixture of NaNO_3 and KNO_3 ,” or by a long phrase “the salt obtained by . . .,” we get no such translation, but only a script that has certain isomorphies with the original.

The interpretation of a text from the seventeenth century will of course undergo considerable changes with time and will also vary greatly among historians with different preconceptions, interests, and philosophies. The same, only more pronounced, variation will characterize our *understanding*, more generally, of research at any given time in the past. This in turn influences our understanding of what we are ourselves doing, and therefore also of our text—which again influences our interpretation of historical documents (the hermeneutical circle).⁷

The tremendously complex genesis of our “knowledge of acids” is not a genesis of knowledge of a *definite* sort of thing, process, or substance. But we may, in terms of our present-day chemistry, plus the not inconsiderable mass of historical materials, trace a development of notions, conceptions, or even concepts that shows certain internal relations such that it is natural to say that they are all notions of acid. The idea that there is an accumulation of knowledge of acids, as defined today, through several hundreds of years, is untenable. We may, however, assert that there is an accumulation of knowledge of $\text{acid}_1, \text{acid}_2, \dots, \text{acid}_n$, where these are different notions in the past, and a stagnation in the accumulation, or even decrease, of knowledge relative to $\text{acid}_{n+1}, \text{acid}_{n+2}, \dots$, which are notions with no relation to research today. Decreases are possible because the term *knowledge* refers to relatively stable convictions within fairly uniform communities, not to verified hypotheses or theories. It would take an unusual amount of pessimism to believe that there has not been a *definitive* accumulation (“not-to-be-lost knowledge”) concerning acids in some interpretations of the old term *acid*. But in principle, the model of growth, progress, or accumulation is such that there is no end to, and also an accumulating probability of, changes of notions and practice that result in incomparability.⁸ The model envisages revolutions, perhaps relatively often moderately deep and pervasive, though rarely extreme. But an extreme revolution breaks the previous accumulative period. (If the model

is subordinated under a certain model of social development, conclusions may be different: if it is envisaged that social conditions will ultimately be completely standardized and uniform, one definite kind of conceptual framework will be adopted and never change anymore. There will be a final revolution and unending accumulation afterward. Scientific revolutions presuppose social change.)

Refutation Seen in Historical Perspective

The following concluding remarks go no further than stating what seems clear from recent historical studies.

All that observations have furnished, however numerous and diversified, have been *instances* of disconfirmation, or *instances* of confirmation (corroboration) or support. To each instance there have normally been attached specific initial conditions or, more generally, relevant observational reports because properly trained researchers (according to the standards within a certain community at a certain epoch) are simply assumed to make reasonable evaluations of this sort. But the weight and relevance of the implicit premises for the conclusions of the scientist are on a par with premises explicitly mentioned by him.

In the light of this, I prefer in what follows to speak of “instances of disconfirmation of a theory” or “disconfirmatory instances” rather than of “disconfirmation of a theory.” Among other things, this way of speaking makes it more difficult to identify disconfirmation with falsification or refutation. One can hardly speak of there being *instances* of falsification or refutation of a theory. The “instance” of falsification makes it a false theory, and the “instance” of refutation refutes it. There is no place for, let us say, three falsifications (or refutations) and five nonfalsifications (or non-refutations) among the results of a test. But there may well be three disconfirmatory and five confirmatory instances. The “evidence” may be, in part, negative, in part positive. According to Paul Feyerabend (1969), we can dispense with confirmatory (positive) instances in science. We *can*, but why should we? Feyerabend claims that by doing so, we get rid of the paradox of confirmation in science because it refers only to positive instances. But in any case, the paradox does not concern methodology because the assessment of confirmatory instances is not (and need not be) of a kind suggested by a calculus of propositions or functions.

For example, let us examine the expression $(x) (Ax \supset Bx)$, for all x , if x has the property A , then x also has the property B . If this expression is forced on us as an *adequate* symbolic expression of a hypothesis (which it clearly is not), and if certain other dubious applications of logical calculi to nonformal languages are accepted, we will *deduce* that the expressions $Ax_1 \& Bx_1$ and $\sim Ax_2 \& \sim Bx_2$ refer to equally weighty positive instances. There is nothing in formal logic to prevent us from taking them equally seriously as confirmations of $(x) (Ax \supset Bx)$. But what is the relevance of this exercise in formal logic here?

What is more important is to test whether a sentence of the form $Ax \& Bx$ has the same relation to those of the form "if Ax then Bx " as sentences of the form $\sim Ax \& \sim Bx$. (A test would soon furnish negative instances if the assumption can be said to be at all testable.)

Let us take an instance from research activity at the level of Gay-Lussac. From the general hypothesis that two liters of hydrogen and one liter of oxygen combine chemically to form two (not three) liters of water (as gas), we predict that the two liters of hydrogen in container C_1 will combine with one liter of oxygen in container C_2 to fill another container C_3 with two liters of water (as gas, if temperature and pressure are held constant). Suppose now that we get a confirmatory instance, as did Joseph-Louis Gay-Lussac in similar tests. Suppose further that we can limit the probable experimental error to within the range of ± 0.001 liters, or one cubic centimeter. Now why is this a *formidable* positive instance? Because previous to our hypothesis we had no grounds for supposing that the result should *not* have been, for example, three liters of water gas. Measured relative to the set of possible experimental results, the one obtained is highly special: the prediction rules out (*prohibits*) a large number of results of the operation. It would be *methodologically* nonsensical to combine two liters of water and one liter of milk and say that their combination as three liters of rather thin milk is an equally weighty positive instance (corresponding to $\sim Ax \& \sim Bx$). (If the universe consisted of only about a dozen things, however, the cases of $\sim Ax \& \sim Bx$ might be interesting.)

Our conclusion supports that of Feyerabend but dispenses with any need to deny that the presence of positive instances should not sometimes be taken as a decisive argument for continuing work with, or for adopting, a definite hypothesis (competing with other hypotheses). John Dalton fur-

nished some negative instances, but by sloppy handling of the gases. Contemporaries (unhappily) sided with Dalton, the luminary, against the relatively obscure Gay-Lussac.

Even at the methodologically uninteresting level of Baconian hypotheses like "all ravens are black," the use of negative *and* positive evidence in any assessment of credibility is clear. Suppose *A* wishes to test the hypothesis, whereas *B* wishes to work with the hypothesis "some ravens are not black," and they hire an ornithologist to observe as great a number of ravens as possible. Suppose he reports only black ones. The result is significant for *both A* and *B*, not only for *B*. If it is significant for the affirmation of *A*'s and *B*'s hypotheses, how could it not be significant for the negations of each of them?

My conclusion concerning the whole discussion on refutation is not "Theories are never falsified or refuted," but rather "Theories that have long, dramatic histories may be said at any given time to have a certain status in relation to efforts of verification, refutation, confirmation, etc." Viewed in the wide perspective of the historian of science, the changes of status are on the whole smaller than they appear to the practicing scientist. His judgments are not *sub specie aeternitatis*, but partly of local research strategy. What, to the personally involved scientist, seems to be a perfect instance of an *experimentum crucis* is to the historian but one interesting test in a potentially long series. The result, if negative, may completely undermine the status of the theory within the scientific community for even as long a period as a generation or two, but a revival is not forever out of the question. The scientist often speaks as if what was *decisive* when changing a research strategy in a given situation was a clear, impersonal proof or disproof, verification or falsification, in a strict cognitive sense. A modern historian may justifiably apply a more "voluntarist" or "personalist" terminology, perhaps one of which the scientist as well as the logician of science would vehemently disapprove.

The historian cannot distinguish consistently between (for instance) "*said* unanimously within the community of scientists to be disproved (refuted, shown to be false, etc.)" and "disproved (refuted, shown to be false, etc.)," at least not without some arbitrariness. Nevertheless the distinction is one he cannot avoid. It requires of the historian that he be explicit in *his* choice of criteria of disproof, refutation, and of other expressions of the status of a theory.

On the other hand, if one considers situations of refutation, the perspective of individual scientists may show great variation. The modern historian does try, therefore, to reconstruct *all* that “was in the air” at the moment in *all* research communities, whether expressed in documents or not. Within a particular scientific community—usually a fairly small group—there may be many things in the air: a more or less generally accepted theory formulated in a fairly definite way (at that moment); the report—interpreted in slightly different ways and involving some explicit auxiliary hypotheses—of a case of observational disconfirmation; several attempted modifications of the theory that seem to change the chains of derivations in such a way that the awkward prediction cannot be made; and several attempted modifications of auxiliary hypotheses or assumptions “disconnecting” the (unaltered) theory from the unfavorable observational report. Whatever the momentary decision of each member of the group, the multiplicity of factors will *continue* to “be in the air.” Decisions not yet published are easily revoked—a long sequence of happenings may take place before anything is published and matters are stabilized. Sometimes interest will contract and subside before satisfactory results can be reported to the community at large: the disconfirmation has been duly recorded and will be remembered, but little can be done about it. Scientists just continue applying and working with the theory—if there is no alternative at the time.

It is a consequence of the foregoing that there are no crucial experiments in the sense of experiments not only furnishing a set of negative instances, but automatically furnishing decisive and definite refutation in the strong sense of furnishing rational justification for rejection.

However, “a new look at crucial experiments” has convinced Imre Lakatos that crucial experiments can “overthrow research programs.” But it is clear that it is not the cognitive basis of this (causally defined) overthrow, the isolated negative instances, that are crucial, but rather the whole development following on the experiments. If there is no “come-back, after sustained effort . . . the war is lost and the experiment proved, with hindsight, ‘crucial.’”⁹ This is a sound description, especially when hindights are dated, so that it does not look as if there had been a definite *last* hindight. But it does not touch on rejection or definitive refutation as a cognitive, nonvolitional notion.

II

Experimental Setup, Rank Dimensions, and Pluralism

Decisive Relevance of Experimental Setup to Testability, Field of Test, and Cognitive Content of a Theory

Contemplating the history of genetics, Bentley Glass poses a question of relevance far outside his own speciality:

The important thing to note in this connection is the effect of the overwhelming well-nigh universal acceptance of a conceptual model that explains satisfactorily all—or nearly all—of the scientific data now known. One must recall that the doctrine of the “billiard-ball gene,” the conception of the “chromosome as a string of beads,” likewise had a tremendous weight of experimental evidence in its favor and little to render it invalid or even slightly dubious, until evidence of a new kind began to accumulate in the past twenty years. Today, graduate students and young geneticists, like those of a generation ago, accept without question the *current dogma*, mould their thinking round it, and most importantly, plan their experiments in accordance with it. Is it not conceivable that, for all its virtues, the blind acceptance of a conceptual model may once again, as so repeatedly in the past, hinder the advancement of science towards a deeper appreciation of genetic relationships?
(Glass 1963: 539)

The abdication of the old model or theory in the case described by Glass was preceded by an accumulation of *evidence of a new kind*. The area of relevant, practicable tests is in general near zero in extension compared with the field of relevant tests. If there is prolonged, diverse, and successful testing within the practicable field, however small, a theory tends to be unanimously accepted. But only among those tending toward dogmatism is it considered to be *true*. The transition from acceptance of a theory while working with it—that is, when using, applying, elaborating it—to accep-

tance of it *as true* has no function in research activity. Among dogmatists it diminishes production of ideas incompatible with the theory. A certain kind of tyranny is established where dogmatists are in charge of research policy. The “life expectancy” of a theory largely depends on the rate of production of “promising” incompatible ideas and the energy with which they are elaborated and tested *independently* from the generally accepted theory.

Let us inspect in more detail the process of testing a theory, having in view the picture suggested by Pierre Duhem. It is my contention that we should make a model that takes into account all presumably relevant traits of the experimental situation (presumptions that may all be more or less misleading!). Each kind of test involves a *unique kind of experimental setup*. The descriptions of kinds of experimental setups are not part of the *exposition* of the theory, but as soon as there is a question of testing or applying it, they act on a par with genuine parts of the theory. At any given time there is a field of practicable experimental testing. The fate of the theory is in part dependent on what happens within that field. And the field is only describable in terms of families of experimental setups or designs. The clear understanding of the relevance of the differences of test operations to the cognitive content of the theory was an important factor leading Percy Bridgman toward his operationalism. Only by more or less arbitrary postulates can we be said to test “the same” things, for example, temperature or pressure when using vastly different test operations suitable for vastly different temperatures or pressures. The postulates are the bridges between molecular or quantum theory and kinds of experimental setups. A small modification of the postulates may alter the conclusion or decision; a presumably “heavily disconfirmatory instance!” may become a “confirmatory instance!”

If some of the relevant properties of the experimental conditions in given cases of testing are unknown, or if some of the relevant properties are not considered, the test is a partial or total failure. Neither confirmation nor disconfirmation ensues. In the life history of theories, this is the way that vast collections of “observations,” “data,” or “facts” drop out. Certain relevant traits of the experimental setups are no longer considered to be known, or they are considered to be known but to show lack of control of relevant variables (temperature, pressure, gravity, etc.). The criteria of relevance change, thus making whole libraries obsolete, as in the field of experimental psychology. A *critique* of the ways a theory

has been *tested* may leave the theory “naked”—that is, neither the reported confirming nor the reported disconfirming instances are accepted as relevant.

If, in this painful situation, new or improved experimental designs are not constructed, the theory is either abandoned without being “refuted”—a frequent event in the history of science—or it is heavily modified. This happened, for instance, to theories about various “experimentally confirmed” sensations in rats in the first two decades of this century. Another example is the rejection of vast collections of published data on degrees and kinds of *attention* when it became clear from more recent studies of suggestibility that the positive results of tests to a large extent reflected suggestibility on the part of the subjects. But the theories to be tested were theories of attention, not suggestibility! (Cf. Charles Spearman 1937, chapter on attention.) By considerable change of wording, the experiments *might* be taken to furnish positive instances of tests of theories on suggestibility. The weakness of this example for our purposes is that the “theories” were somewhat crude, more like empirical laws. A simpler example is the discarding of data on temperatures in the history of physics because thermometers used before certain inventions had been made were influenced by differences in air pressure.

In the case of quantum theory, a subclass of statements on experimental setups acquires a special status, as has been made abundantly clear by Niels Bohr.

[N]o result of an experiment concerning a phenomenon which, in principle, lies outside the range of classical physics, can be interpreted as giving information about independent properties of the objects, but is inherently connected with a definite situation, in the description of which the measuring instruments interacting with the objects also enter essentially. This last fact gives the straightforward explanation of the apparent contradictions which appear when results about atomic objects, obtained by different experimental arrangements, are tentatively combined into a self-contained picture of the object.

(Quoted in Müller-Markus 1966: 78)

Narrow Contextual Testing of Hypotheses

It was the contention of Duhem (1962: 188 ff.) that one cannot test an *isolated* hypothesis experimentally because in order to arrive at a definite

conclusion, one must assume the truth of other hypotheses, namely those making up the entire theory of which the hypothesis is a part.

Let H_1 be a hypothesis we intend to test, and H_2, \dots, H_n hypotheses assumed true or valid when testing H_1 , whether forming part of the same theory or not. Let the letter O symbolize a directly testable observational consequence ("methodological prediction").¹

We can then illustrate verification and falsification of the hypothesis H_1 as follows, if S stands for H_2, \dots, H_n :

- (1) $[(H_1 \ \& \ S) \supset O] \ \& \ O$ (confirmation)
- (2) $[(H_1 \ \& \ S) \supset O] \ \& \ \sim O$ (disconfirmation)

By (1) we shall say that hypothesis H_1 gets *confirmed*. But this does not, of course, rule out that H_1 is false. And the confirmation is—if we ignore the intention of the experiments—not only of H_1 , but of a group of hypotheses. In its actual use among researchers, the term *confirmation* and similar, more idiomatic words are used in relation to H_1 even if the users themselves are perfectly aware of the role of some or all of H_2, \dots, H_n . The intention is built into the use, and only by neglecting this can one agree with Duhem.

As researchers, we *trust* H_1 if there are many and varied previous confirmations, but we may accept H_1 as a working hypothesis even if no confirmations, or only disconfirmations, are available, or if neither confirming nor disconfirming instances exist.

The formula (2) illustrates an instance of disconfirmation. According to current usage among researchers, it is a disconfirmation of H_1 , not of $H_1 \ \& \ \dots \ \& \ H_n$. That is, the definiteness of intention relative to the use is not sufficient to declare that the users clearly would reject (or accept) the suggestion that the disconfirmation is of *all* the hypotheses H_1, \dots, H_n . Cautiously I shall say that the instance of disconfirmation is of H_1 . Until further notice, I leave out of consideration the relation of the disconfirmatory process to the hypotheses H_2, \dots, H_n , since this is a separate issue.

From (2) we may conclude that H_1 and S cannot *both* be true—that is, that not *all* H_1, \dots, H_n are true:

- (3) $\sim(H_1 \ \& \ \dots \ \& \ H_n)$

This in turn is equivalent to:

- (4) $\sim H_1 \vee \sim H_2 \vee \dots \vee \sim H_n$

Whether it is H_1 or any other H that is not true remains unclarified.

In short, hypotheses are not verifiable or falsifiable separately, but only contextually, if at all. The expressions “instance of observational confirmation of H_1 ” and “instance of observational disconfirmation of H_1 ” do not refer to verification or falsification, but to the intended test of H_1 as part of a group of hypotheses from which (methodological) prediction is made. Confirmation does not preclude falsity, nor disconfirmation truth.

In the light of the above consideration, we may reformulate the Duhemian contextual principle of testing as a principle of falsification in this way: a hypothesis can only be falsified contextually, not in isolation. If we have an instance of observational disconfirmation but decide to continue to work with the hypothesis, we can justify our decision (in part) by making the ad hoc assumption:

$$(5) \quad \sim H_2 \vee \sim H_3 \vee \dots \vee \sim H_n$$

The relative strength of assertion of (5)—considered as a formula—is very little when n is big, and this suggests that the assumption is very weak.² But this nevertheless makes the case of disconfirmation irrelevant for H_1 . We can thus blame the wrong observational consequence on the rest of the test hypotheses, without necessarily specifying which of the H 's, H_2, \dots, H_n , we suppose to be untenable.

Implicit in the current usage of the term *disconfirmation* is, as already suggested, a reference to an intention: every single, definite experimental design is adjusted to *one* hypothesis, or to a group that is a (genuine) subgroup of the total class of hypotheses relevant to the test. One cannot have a design such that all relevant hypotheses are tested at once. If this were practicable, confirmation (or disconfirmation) would be confirmation (or disconfirmation) of all components taken separately.

The question of the methodological justification for (5)—that is, for blaming the unwanted happening on “the others”—can only be decided, if at all, on the basis of a study of the total research situation. What is the strategy at the research frontier at the moment? What are the priorities? Which are the interesting alternative hypotheses, if any? What can be neglected until further notice, considering the order of priorities? Making move (5) does not imply any definite *belief* that some of the other hypotheses are wrong. Position (5) may be posited as an ad hoc assumption in order to explore what might follow.

A Broad Thesis on Contextuality of Testing

I shall now propose a wider thesis of contextuality of testing than that of Duhem. His thesis is limited to the context of *other hypotheses* in the capacity of being other parts of the systematic exposition of a science. The context I shall refer to includes that of “initial conditions.” At least in part the sentences expressing such conditions do not express hypotheses belonging to the systematic exposition. The relevance of such a context has often been pointed out, and I shall only try to clarify some details.

The mass, position, and velocity of a planet at a given time must be specified before an observational prediction can be made by means of the Newtonian theory of gravitation. Any error here spoils a test of the theory. That is, if ten sentences express the initial state, the truths of all of them are presumed when the verdict of “confirmed” or “disconfirmed” is proclaimed. The formula $PV/T = r$ (P = pressure, V = volume, T = temperature, r = a constant) immediately prescribes three assertions to be tested, one stating the pressure, the other the volume, the third an indication of temperature. Considered as a hypothesis, $PV/T = r$ by its very formulation *prescribes* (at least) three initial-condition assertions.

This is all elementary. But a vastly greater number of factors are relevant to the conclusiveness of a test. In its textbook formulation, a law need not contain reference to gravitation or air pressure. But it is implicitly assumed, perhaps, that the law only holds true outside strong gravitational fields or in a near-perfect vacuum. If an observational report were to make all the relevant, more or less doubtful assumptions explicit, there would have to be a sentence reporting air pressure during the test and the local force of gravitation. Sentences of this kind are sometimes said to express the *Randbedingungen* (boundary conditions) of the test. They then include as a subgroup the specifications of the so-called *initial state* of a system, the initial-condition sentences.

Many famous tests have been inconclusive because of the uncertainty or vagueness or incorrectness of the initial-condition sentences. The researcher has had, for instance, to use very bad measurements of the size of the earth or the distance to the moon in testing gravitational hypotheses. Later some nonecholalic historians would say that the hypothesis was “really” correct but the experimental setup was imperfect, or the “constants” used were wrong. One is reminded of Newton’s temporary aban-

donment of his universal law of gravitation: he had assumed that a degree of the earth's circumference was only sixty miles in length. The measurement appears as one of the constants assumed to be correct in the description of the case tested. In other cases, a hypothesis *is not* given up. Thus, the existence of the neutron was persistently assumed in spite of strong negative experimental results. Referring to certain test situations, the discoverer of the neutron, James Chadwick (1932: 692), says: "The failure of these early experiments was partly due to the weakness of the available source of polonium, and partly to the experimental arrangement, which, as it now appears, was not very suitable." That is, if somebody *now* set up an experimental arrangement of this early kind and announced a disconfirmatory instance in relation to the existence of a neutron, one would point to certain unfulfilled requirements specified as *Randbedingungen* (boundary conditions) in later experiments. The hypothesis no longer covers experimental test situations of the old kind.

There are no rules such that one can or must pick out a definite set of traits of the experimental setup and express them in *Randbedingung* sentences. What is certainly required and what may certainly be left out depend on characteristics of the problem situation at the frontier of research, and only insiders can develop such an awareness. The novice betrays his crudeness in counting trivialities and leaving out essentials. If, say, $s = \frac{1}{2}gt^2$ is to be tested, one must of course specify in initial-condition sentences g and t , s and t , or s and g . But which special features of the mechanical setup should be mentioned? Scientists are apt to refer only to those features that they think or guess members of the research community would find particularly relevant when asking about the *quality* of the experiment. But the identity of the experiment is only in part determined by those features. The choice of factors considered relevant and worth mentioning is always in part the result of sociological guesswork.

I shall distinguish, in what follows, narrow or central initial-condition sentences from peripheral initial-condition sentences. The adjective *central* I shall reserve for the assertions related to constants prescribed by the very formulation of the hypothesis. The *peripheral* initial conditions have two important properties in common: (1) They do not belong to the hypothesis intended to be tested nor to any other hypothesis derivable from the same theory, nor do they belong to the narrow or central set of initial sentences on conditions. (2) If a categorical verdict of "confirmed" or "discon-

firmed" is made, the truth or correctness of these assertions is assumed. If any of them are false, the test is irrelevant; it has, strictly speaking, no weight in relation to the question raised (until it is perhaps calculated *how much* the state of affairs expressed by the negation of the assertions could influence the experimental conditions).³

By "initial conditions" I shall refer to the total class of central and peripheral assertions.

In different philosophies of science, opinions concerning the scope and variety of (relevant) peripheral conditions must necessarily show variation. The new historiography must be expected to accept a very broad concept. The longer the periods of development studied, and the more weight laid on periods of revolution, the more kinds of conditions will have to be taken into account. But this holds good only insofar as the new historiography aims at covering *different* philosophies of science. It calls for a neutrality that is difficult to reach, even if strongly coveted.

Referring to the conflict between cosmologies at the time of Galileo, Feyerabend seems to insist on ideas or theories of cognition being considered part of the initial conditions (in my terminology):

The Copernican view conflicts with the evidence that has been established in accordance with the older ideas of cognition and is directly compared with it. This is not a relevant test. Relevant tests must interpolate meteorological, dynamical, and physiological disturbances between the basic laws and the perceptions of the observer. (Feyerabend 1970c: 294)

I certainly agree but might add that parts of theories of cognition that are relevant to the testing of any hypothesis whatsoever, and affect all in a similar way, might not be counted as initial conditions.

Taking central initial-condition sentences (*I*), into consideration, we now write for purposes of illustration:

- (6) $[(H_1 \ \& \ S \ \& \ I) \supset O] \ \& \ O \ \& \ I$ (confirmation)
- (7) $[(H_1 \ \& \ S \ \& \ I) \supset O] \ \& \ \sim O \ \& \ I$ (disconfirmation)

In the case of disconfirmation, our suspicion may center on any hypotheses contained in *S* or on some of the many sentences, *I*, specifying initial conditions and, of course, on the professional level of the observation itself or on the complicated transition symbolized by \supset .

We also need a formula reminding us of the importance of the truth of the description of the experimental setup. Perhaps there was no vacuum? Perhaps the light patch was due to an imperfection in the lenses?

In what follows, the letter *E* stands for the conjunction of the peripheral initial conditions. The truth of *I* and *E* is a necessary and sufficient condition for the relevance of the maximum of the experiment.

The explicit derivation of *O* is usually made without stating anything about the experimental setup. This is presumably the main reason why it is customarily omitted from presentations of “logic of science.” The sentences expressing the setup appear as an addition to the others before the decisions “confirmed!,” “disconfirmed!,” or “undecided!” are taken. We may now write:

- (8) $[(H_1 \ \& \ S \ \& \ I) \supset O] \ \& \ O \ \& \ I \ \& \ E$ (confirmation)
(9) $[(H_1 \ \& \ S \ \& \ I) \supset O] \ \& \ \sim O \ \& \ I \ \& \ E$ (disconfirmation)

Any change in truth-value of any sentence expressing the test situation (the *E*’s) will result, according to (8) and (9), in there being neither confirmation nor disconfirmation. We get an “undecided!”

Theory of cognition must include theory of observation. Since the time of Hermann von Helmholtz’s discussion of the nature and limitations of scientific observation, more or less precise and careful theories of observation have been developed. But situated as they are at the crossroads between physics, chemistry, biology, physiology, and psychology (and other social sciences), these theories have not been easy to compare and evaluate.

Because of the inevitable proliferation of theories of observation, the class of observations of a phenomenon is only a (roughly) definite class (at a definite stage of the movement of the research frontier) if related to a definite theory. The classes related to different theories sometimes have, sometimes do not have, common elements. Those who subscribe to a theory of pure sense-data will tend to say that if scientists note down in their observational journals *exactly* what they observe (that is, sense-data) and then count as observed phenomena only phenomena *defined* by the observations in the journals, then there will be an invariance in the class of observations in relation to competing theories. But in spite of very advanced work in defense of theories of sense-data and in spite of related

theories of “the immediately given,” no such theories have been established. In fact they are even losing ground. So we must, I think, admit that as things stand now, there are, strictly speaking, extensive variations in the elements of classes said to constitute “the observational basis” of a theory.

Only rarely is there such a large-scale agreement as in the case of “the red shift.” Here what we *observe* are displacements of spectrum lines—that is, differences in measurable locations of lines (usually on a photograph), but because of the large number of theoretically important kinds of complications that arise when interpreting the differences, there is a general and pronounced modesty about what is said to be observed. Usually it is the displacements that are said to be observed, not, for instance, the velocity of bodies receding from us. That is taken to be an *interpretation* of a displacement.

Suppose we have a theory, T , which says that under specified conditions of the kinds $A(x)$, $B(y)$, . . . we should observe certain values of the two parameters (variables) $F(z)$ and $G(t)$. If, for instance, x and y are 6 and 8, we derive from T that z and t should be 3 and 2. In addition to x and y , there will be an indefinite number of other specified conditions—“indefinite” because of the indefinite number of elements of the class “relevant traits of the experimental (or more generally, observational) setup.” Only a few of them will be quantitative, as $A(x)$ and $B(x)$.

The recognition, identification, and acceptance of F and G as *phenomena observed* (that is, that something is such and such) depends on views, mostly unexplicated, about observation. When made explicit and precise, the views may take different forms. Let us say T_1 is the adopted form. It will be our *theory of observation*. The choice of A , B , . . . as explicit parameters of conditions depends on a set of auxiliary hypotheses, assumptions, and postulates.

Thesis: Given (1) a set of observations said to be observations of a definite phenomenon, (2) a theory said to be a theory covering that phenomenon, and (3) a set of auxiliaries used as intermediaries between the theory and the observations, then if T_1 is a theory of observation that, together with a set of auxiliaries, justifies (or permits the derivation of) the conclusion that the phenomenon *said* to be observed is the phenomenon observed, one may always formulate a workable observation theory, T_1 , incompatible with, or even incomparable to T_1 , such that it, together

with a set of auxiliaries, justifies (or permits the derivation of) the conclusion that the same observations (in the form of symbols in an observational journal) are of a different phenomenon.

In short, *what we identify as the phenomenon observed* depends on our theory, or rather metatheory, of the process of observation. Given the potential many-one relation of workable theories to phenomena said to be observed, there will never, as long as active research in the field continues, be one phenomenon uniquely pointed out by any theory.

Whether this thesis is judged tenable or not seems largely to depend on which field of research one habitually works in. For workers in psychophysics, perception, and many other humanistic disciplines, the thesis is considered true, but more or less trivial. “Tough” natural scientists tend to ignore or reject it.

Much of the importance of the thesis is due to its sobering effect: whereas the Mach-Duhem-Poincaré principle (see p. 37) stresses the many-one relation between theories and observation to be covered, the above thesis stresses the many-many relation between metatheory of observation and phenomenon said to be observed.⁴ For a given theory T' , the relation may be represented thus:

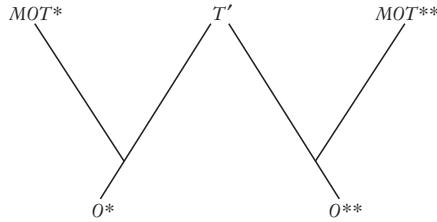


Figure 1. Illustration showing the many-many relation between metatheories of observation and their corresponding observed phenomena for a single theory.

Theory T' , combined with metatheory (of observation) MOT^* , results in O^* being taken as the observation under a specific set of conditions. Combined with metatheory MOT^{**} , the same theory under the same conditions results in O^{**} being taken as the observation. The term “observation” is here used to express something that includes the identification of a phenomenon observed (for instance, “2 °Fahrenheit,” not just “2”).

The corresponding many-many relation may, for *two competing theories* T' and T'' , be illustrated as follows:

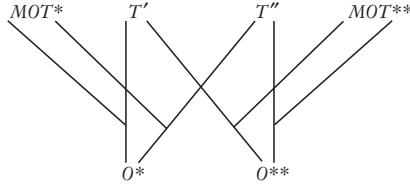


Figure 2. Illustration showing the many-many relation between metatheories of observation and their corresponding observed phenomena for two competing theories.

Here, from two mutually inconsistent theories T' and T'' , the same observation is derived. But close inspection suggests that a metatheory MOT^* is presumed. So the observation is symbolized by O^* rather than simply O . If a different metatheory, MOT^{**} , is assumed to be valid, the “same” observation must be given the form (and content) O^{**} .

It remains to illustrate the application of a metatheoretic principle of the Mach-Duhem-Poincaré type to these theories of observation. It implies that, for instance, MOT^* would not be the only metatheory that together with T' yields O^* :

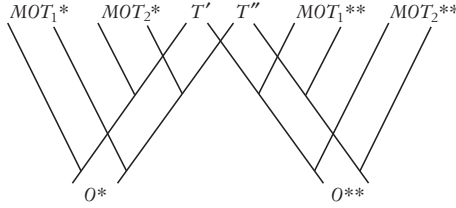


Figure 3. Illustration showing the many-many relation between multiple metatheories of observation and their corresponding observed phenomena for two competing theories. Metatheories whose names have the same number of asterisks but different indices, have the observations under consideration in common but are in other respects different. Metatheories named with a different number of asterisks cover different sets of observations.

The multiplicity of metatheories of observation precludes that we can, strictly speaking, identify a set of observable phenomena independently of a metatheory of observation. As long as all scientific observers within a field have a similar cultural background, the implicit views on observation may be expected to show small variation. Explication is often unnecessary. However, an interplanetary research team, including some strange beings from α Centauri, might pose some tough problems of metatheory.

According to the foregoing, an instance of confirmation or disconfirmation of a hypothesis by observation is only definable relative to a fairly complex conceptual structure. The relata are the other hypotheses of the theory to which the hypothesis belongs, S , the central and peripheral initial conditions, I , the sentences that guarantee the relevance of the experiment, E , and the observation theory MOT^* .

We may illustrate the above by offering the following schemata for such instances:

- (10) $[(H_1 \ \& \ S \ \& \ I) \supset O^*] \ \& \ MOT^*(O^*) \ \& \ E \ \& \ O^*$ (confirmation)
- (11) $[(H_1 \ \& \ S \ \& \ I) \supset O^*] \ \& \ MOT^*(O^*) \ \& \ E \ \& \ \sim O^*$ (disconfirmation)

Philosophers of science of so-called empirical affiliations have something to learn from the *praxis* theoreticians, for the latter do not identify the empirical with the observational. There is ample historical justification for a broader interpretation of “empirical”: the Greek word may well be translated as “experiential.” My experience as a human being includes my professional experience and all I have of experience outside my profession. I am more “experienced” in relation to certain tasks than in relation to others, and the difference reflects both personal and social conditions. Observation also contributes to my experience but cannot be severed from the rest as more basic or more certain.

“It is my experience that . . .” This locution is convenient for reference both to observations and to informal, crude generalizations and interpretations of what one has “seen and heard,” as well as to *tentative expressions of insights* that integrate both.

When more instances of observation are unlikely to overthrow a theory, more experience may well do the job. Conditioned reflex theories covering learning of mazes were undermined by broad, relaxed, critical reflections and observations on rats running in mazes. Environmental factors and the experimental designs were tentatively changed. Some theo-

rists were able to assess more clearly the narrowness of the standard conditioned reflex experimental setup. They had *enlarged their experience*. Radical criticism of the dominant conditioned reflex theories was made possible.

The expression “observation falsified or weakened the theory” can, thanks to the eminent metatheoretical work of a large number of philosophers of science, be given very precise and interesting reformulations. The expression “experience falsified or weakened the theory,” taking “experience” in broader meanings than “observation,” has not been worked on with similar admirable results. I shall not try here to change the situation. Suffice it to say that the situation calls for an integration of the empirical and *praxis* models of cognition, using the wide connotations of experience as a point of departure.

Plurality of Functions and Rank Dimensions of a Theory

What are the prospects of fulfilling the purpose, the intended functions, after a negative result of the test of a theory? What are the alternatives? Are other theories available? If not, what is the prospect of creating alternatives?

Perhaps the scientist, if he is of an unusually articulate subspecies, can list, say, ten functions relevant for him and make up his mind about the change in the prospect of the theory's fulfilling those functions after the setback. If philosophers of science were interested in honest, down-to-earth investigations of reasons why individual scientists abandon or continue to work with a theory or a hypothesis, they would have to start with such reasons mentioned by the scientists themselves and then start trying to classify, simplify, and criticize.

Incidentally, a methodologist of social science, Johan Galtung (1967: 459), lists ten dimensions of theories, all of them “rank dimensions in the sense that they can be used to evaluate theories”: (1) generality, (2) range, (3) status of individual hypotheses, (4) formalization, (5) axiomatization, (6) relations to other theories, (7) predictability, (8) communicability, (9) reproducibility, and (10) fruitfulness.

Some readers might look for “testability” as a dimension. It is indirectly taken into account by (3). The rank dimensions of hypotheses are said to be: generality, complexity, specificity, determinacy, falsifiability, testability, communicability, reproducibility, predictability, and tenability.

It should not be necessary here to discuss the number and kinds of rank dimensions of competing theories. Suffice it to remind ourselves that the intricacy and many-sidedness of the functions of a theory necessitate *a whole range of questions being asked* when assessing the status of a theory; insofar as these questions allow for improvement in interpersonal preciseness, they are best thought of as part of a comprehensive questionnaire. The pretesting of the questionnaire would probably result in eliminating such predicates as “works well,” or “is simple.” Very different abilities are placed together under the heading “works well,” and “simplicity” has recently been used in too many senses.

With ten rank dimensions, not necessarily those suggested by Galtung, ten competing theories may all score differently, each scoring satisfactorily in relation to some dimensions and unsatisfactorily in relation to others. In lively research communities, the comparative status of a set of competing ideas will show variation over the years. The action to be taken on the part of the active researchers in the face of this complex situation cannot easily be one of simple acceptance or rejection but must be considered within a long-range research strategy, with distant moves dependent on feedback from previous moves.

Today scientists work mostly in groups that belong to larger communities. Whether one drops a theory depends very much on what others do.

Why is it necessary to remind ourselves of the bewildering multiplicity of rank dimensions of a theory? First of all, because in this century ingenious but simple models on the metalevel have been worked out and then mistaken for reality—or at least have been discussed as if they pretended somehow to cover the actual function of theory choice. One cannot doubt the central importance in metascientific queries of simple models and rational reconstructions in general, but mistakes as to their operation (and limitation) may prove fatal to metaresearch. “Hypothetico-deductive method” is a name for models that have been mistaken for single descriptions.

A very weak disconfirmatory instance may, under certain circumstances, be a *conclusive or decisive* factor in rejecting a theory. The scientist decides to *stop working with it*. In the background of the simple words “He rejected the theory,” or “The theory was now abandoned,” there is, of course, a complicated *praxis*. Sometimes these words just mean that *for the moment*—that is, within a definite subsection of a research program a sci-

entist finds the prospect of work on the theory unpromising—or they may mean that a competing theory, which has already been worked out to some extent, seems much more promising to him in that particular work at that particular time. But at the same time others may find applications for the abandoned theory, acknowledging that it yields excellent predictions within many fields and may still be a potent source of inspiration when contemplating possibilities of new theories. (Cf. Bohr's "Correspondence Principle" as a guide for the imagination.)

Scientific research is a search for truth, but this does not imply that the range of properties of rank dimensions of scientific theories is intuitive and uncontroversial. A drop in strength of assertion increases, or at least cannot decrease, chances of truth. But in spite of this, superior strength of assertion is taken as an argument *for* a theory, not against it. It figures prominently in rank dimensions. "Simplicity" is controversial: some take it to be part of an adequate set of rank dimensions; others do not (the disagreement is somewhat less than it seems because of semantical confusion). The dimensions must "have to do with truth," but must also "have to do with search!"

It is of course an important goal to arrive by rational discussion at one single set of rank dimensions as the supremely adequate one in a particular historical research situation within a particular field.

But the prospect of this noble goal should not prevent us from admitting that we are very far from realizing it and that the best thing to do at present is to clarify discussion on criteria, connecting the disconnected strings of argumentation, to make key terms ("simplicity," "observation," "fact") more precise in various directions, to refine the network of distinctions, and to deepen the range of explicit assumptions all the way down to fundamental ontological and other philosophical positions.

In judging the relative validity of proposed priority lists for rank dimensions of a theory, one has to take into account the typology of the intended main functions of theories. After all, most original and important innovators in science have their own way of conceiving the function of their theorizing.

A survey of broad theories, or rather systematizations, in the field of (theoretical) psychology in the 1930s suggests that they may be ranked according to a variety of traits deliberately fostered by the theoreticians. In the table on page 31 the systematizations are compared and ranked in

an intuitive way. The numbering of the following items corresponds to those of the several columns of the table.

1. High level of (verbal) explicitness and unambiguity as regards assumptions, and clarity as regards implications, whether genuine or not. (Conventional criteria—include, for example, prestigious physiological, physical, or mathematical terms and an abundance of “if . . . then . . .” and “according to that . . . this . . .” statements.)
2. Extent of the field (of possible predictions) *claimed* to be covered by the theory. (For example, certain experimental results, learning, and adaptive behavior.)
3. Unambiguousness in *practice*—that is, for use by independent observers: consistency of estimated implications by observers with different terminological or general ideological idiosyncrasies.
“Cooperational efficiency”: Suppose an observational report obtained under specified conditions is given. The greater the number of independent users who arrive at the same conclusion as regards the question of whether the report (1) is irrelevant to, (2) strengthens, or (3) weakens the theory, the greater the cooperational efficiency.
4. Extent to which we assume cooperational efficiency to have been due to factors other than positive transference among members of an ingroup. (We assume that in view of the absence of extreme degrees of clarity and rigor in any existing theories, consistency of implications is achieved by direct adaptation and “*Gleichschaltung*” of speech habits between members of the group.) If members of an ingroup are bound together closely by common slogans and habits of exposition and inference, the expansion of the group will make the theories cooperationally more clear because of the greater percentage of psychologists tracing out the implications in the same way. Thus, a minus in column (4) but a plus in column (3) means that the relatively high degree of cooperational efficiency is to a small extent reached by factors other than transference among members.
5. Ease with which an outgroup (a group of competing theorists or people with some training in psychology who do not belong to a definite theoretical group) can distinguish programmatic state-

ments and slogans from other statements, and the ease with which it is possible to separate programmatic (anticipatory, propagandistic) components of the meaning of a statement from other components. If reports of experiments are couched in a terminology toward which certain groups are hostile, this will make it difficult for members of these groups to see anything other than the odious terminology. The way to an understanding of the experiments and the results is thus made more difficult than would seem necessary. Another example is where propaganda by means of general slogans and broad methodological declarations is mixed so perfectly with specified descriptions and interpretations that outsiders do not know what is "serious" and what is "show."

6. Extent to which predictions have been made from the theory in a cooperatively efficient manner. Same unit of measurement as under (2).
7. Preponderance of successes (verified predictions) among the predictions mentioned under (4). *Post-festum* predictions ("This result is just what I would have predicted from my theory if you had asked me") are included if backed by those other than the author of the theory.
8. Author's preference for concepts taken from the realm of "low-level" (*l*) behavior or "high-level" (*h*) behavior. (Examples: physiological behavior, stimulus mediation—low level; level of descriptions of behavior in the vernacular—high level.)
9. Type of methodological rationalization in cases of asterisks in column (8): *P* \equiv salvation of psychology as a science by low-level approach by "reduction" of high-level descriptions to low-level, by looking for possibilities of extension of physiological (ultimately physical) laws in the realm of high-level behavior. *C* \equiv salvation of psychology by finding laws (at least high statistical correlations, as in chemistry) that hold for high-level behavior, by adaptation of conceptual framework to the exploration for such "invariants." Belief in ultimate reduction to low-level as explained under (7). *A* \equiv extreme antiphysiological attitude by stress upon the necessity of using sociological (or very high-level) descriptions of behavior in every case of specification of test conditions for a theory, whether the theory is conventionally called physical, physiological, or psy-

chological. Disbelief in reduction of high-level laws to low-level laws. Integration of science "from above."

10. Emphasis in actually accomplished research on:

$L \equiv$ rigor. Theorem producers.

$F \equiv$ extent of field covered, imaginative (exploratory) fruitfulness of system. Hunch producers.

$S \equiv$ significance for social problems and reforms (rather than for "academic" problems). Social-argument producers.

The table below shows my scores of psychological systematizations from various points of view. A score of $-$ means a small amount of the trait scored, $+$ means a fair amount, and $++$ means a large amount. An b means the system starts from "higher-level," or "molar," concepts; l means the system starts from "lower-level," or "molecular," concepts. An $*$ means that the b or the l is especially stressed by the author of the system.

Types of Theories Compared

Brunswik	Theories on thing constancy
Freud ₁	Theories on psychoneuroses
Freud ₂	Theory according to which interruption in verbal reaction in the psychoanalytic situation is due to suppression rather than to lack of associations
Hull ₁	Gradient of reinforcement (goal gradient hypothesis)
Hull ₂	Revised mathematico-deductive theory of rote-learning (1939)
Lewin	Theory of psychological forces (1938)
Piaget	Studies on the cognitive development of children
Skinner	"Average" theory of <i>The Behavior of Organisms</i>
Thurstone	"Average" rational equations, theory of primary factors
Tolman ₁	Scheme of operational psychology (in his presidential address)
Tolman ₂	Sowbug theory of VTE (1939)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Brunswik	-	++	-	+	-	-	+	b^*	S	F
Freud ₁	-	++	+	-	-	-	+	b		S
Freud ₂	+	-	+	-	+	-	+			
Hull ₁	+	++	+	-	+	-	+	l^*	P	L
Hull ₂	++	-	++	++	++	-	+			
Lewin	++	++	+	-	-	-	+	b		F
Piaget	-	+	+	+	+	-	+	b		F
Skinner	+	++	+	-	+	-	+	b		F
Thurstone	++	++	+	+	++	-	+	b		L
Tolman ₁	-	++	+	-	-	-	+	b^*	S	F
Tolman ₂	+	-	++	+	+	-	+			

As seen from the table, none of the theories considered has a + (or –) in every rank dimension. All of them have some advantages and some disadvantages. This suggests the complexity of choosing what theory is most promising. It also emphasizes the importance of establishing explicit value priorities in order to rationally reconstruct a policy choice.

But the creative scientist need not, of course, experience anything comparable to a cold *pro et contra* deliberation centering around a number of rank dimensions. This might happen, but usually the decisions for or against particular possibilities to theoretical solutions of a problem are experienced as violent “true or false” intuitions. Relevant criteria are closely connected with the special topic at hand, and not experienced as abstract principles applied to a concrete case.

Theories help us “solve problems.” This means first of all that we obtain understandable, integrated answers that address sets of burning questions—it is primarily the *kind* of answers we obtain that we use to judge the efficacy of the theory. How “certain” the solution is, is of course also a matter of consideration, but if the theory is not completely without zones of testability (cf. Einstein’s criterion) it is acceptable. When it is said that Schroedinger’s wave equation “completely solved” *the problem* of atomic hydrogen, what is meant is that questions covering the spectacular behavior of atomic hydrogen in electric and magnetic fields, questions relating to its spectral lines, and practically all other questions raised *at a certain time*, were answered by using Schroedinger’s equation. Where experimental checks were possible, they were fairly successful, but if some serious discrepancies had been found, they would most likely have stimulated theoretical interest rather than made physicists “reject” the theory (of which the equation was a part).

The expression “*x* completely solved the problem *y*” might suggest that if *x* is a theory it must show a high level of observational confirmation or rank very high on all dimensions. This need not be the case. Schroedinger’s equation was “just what one was looking for,” but looking as active researchers, not as bureaucrats of so-called knowledge. It answered a definite series of “why’s” that had been asked about the hydrogen atom and “addressed” most of the relevant observational data.

To try to construct metatheories of verification and falsification or confirmation and disconfirmation that would justify a decision to reject or adopt a theory implies a neglect of heuristics as an integral part of the

dynamics of theories. There is no separate cognitive or epistemological dimension such that if it is not reached, the theory should be rejected in research, whatever its merits on heuristic dimensions. And in speaking of the scientific enterprise, it is research, not knowledge, that is crucial.

Karl Popper (1963: 228) and others have worked toward the establishment of a criterion that would enable (genuine) science to be demarcated from pseudoscience.

Psychoanalysis is considered by Popper to be a pseudoscience. But at the Institute of Human Science (Yale), psychoanalysts and behaviorists, inspired by C. L. Hull and others, successfully collaborated in translating some of the ideas of psychoanalysis into a more testable form. There has never been anything definitely “wrong” scientifically with psychoanalysis as a whole, perhaps only some persistent weaknesses. For an excellent survey of the questions, see B. Christiansen (1964). If one wished in all seriousness to disqualify psychoanalysis, one would have to criticize definite published psychoanalytic hypotheses defended in quotable papers, as Popper always does when commenting on physicists’ hypotheses. There must be a fair trial. It is perhaps unnecessary to add that if such a confrontation is made, any clear-cut demarcation formula is apt to break down.⁵

From the point of view of social development and the free society, genuine science seems, through its influence, to be more of a threat than the hundreds of sects that firmly believe in what Popper or Lakatos would call pseudosciences. The sects counteract uniformity and other-directedness and have no chance of establishing thought monopolies. Small-scale irrationality is of little importance compared with the global irrationality created by dominant “scientific” worldviews fostered by “genuine” science. There is a “competitive-selective aspect of a methodology” (Spinner: 1968), but to win is to win in relation to a worldview, not to *any* worldview.

The insistence, in the abstract, of the fallibility of science has increased the prestige of, and confidence in, science, not diminished them. But more than anything else, perhaps, we need courageous proclamations of values that are different from those said to follow from a rational, or even scientific, world outlook.

The enemies of a free society are to be found today in quite other directions than in the 1930s.

According to Popper (1968: 91), “science often errs and . . . pseudo-science may happen to stumble on the truth.” If this happened more and more frequently according to those who were applying Popper’s criterion of pseudoscience, would it never be wise to move the line of demarcation? Surely it would, and the effect of a few moves would be to lower the pretensions of the demarcation so that, for instance, it was used only as a basis for issuing yearly bulletins about what looks most scientific at the moment in relation to theories adopted within specified communities of researchers.

Lakatos continues Popper’s program of elaborating “objective and critical standards of growth” of natural science, especially physics. The aim is in part to advise the scientist and to give him a measure of what is good and what is bad, or at least of what were the good and what were the bad decisions in past research work. If the critical standards are followed, a high rate of growth is to be expected, and if not followed, one can expect a low rate or stagnation. The details of Lakatos’s proposals can only be understood by careful analysis of the “Popperian” approaches within the “logic” of research since 1934.

Here I shall only state some conclusions from the study of Lakatos’s recent papers (Lakatos 1968a, 1970):

1. The criteria of whether a theory *or* a research program or a hypothesis *should* be abandoned (until further notice or finally, in part or in whole, by all or by some) are local in character, both in space and time, and highly tentative. There are no general rules or standards (except very trivial ones).
2. Study of past decisions (concerning abandonment) reveals astonishing, unpredictable revivals (rehabilitations) of theories that were universally abandoned for very good reasons in terms of Lakatos’s own standards.
3. Great scientists have tended to avoid shifts or changes in theories that had little prospect of “producing” new and astonishing facts—sometimes, however, one had to wait very long for such happenings.
4. Proliferation of theories, etc., serves scientific growth, but one should note (temporary) successes and setbacks and abandon them neither too early nor too late.

5. If carefully reconstructed historically, decisions to abandon theories, etc., are on the whole rational. I take Lakatos to mean that there are grounds and reasons for the decisions that we find intellectually understandable and correct.

These conclusions formulated with the use of a minimum of technical terms are important in supporting our pluralist and possibilist views. It should be noted, however, that the aim of Lakatos is not to support or attack such views, but to solve demarcation problems.

The very complexity and tentativeness of Lakatos's normative and descriptive demarcating formula suggest that research policy might be fruitfully studied in a wider frame than that which he adopts.

Research programs in natural science are today perhaps more than ever bound up with technical questions such as: what kind of machinery is available and at what cost? Program *A* is technically so costly that the competing programs *B* and *C* must be given up if *A* is chosen. Or, theory *A* suggests rich possibilities of tests and new facts but is very costly, whereas theory *B* requires practically no apparatus, but (highly tentative) estimates of what will be found are somewhat less optimistic. Further, decisions on research policy affect human values through pollution, degrading use of persons in experiments, etc.

These and other factors strongly influence the *rationality* of research policy. Standards of rationality depend on value priorities, individual and collective. There is no autonomous "scientific" policy.

The intellectual interest of two researchers or two research communities may be systematically different: for the first, high technical and logical achievement is a paramount value; for the second, there is broad interest in the objects of research (for example, climates, fossil animals, and particular classes of numbers). It is *rational* to pursue different research policies in the two communities.

Research as an activity cannot be rationally independent of the rationality of individuals, groups, robots, or communities pursuing research. One may construct an ideal of research inhabiting "the third world" of Popper and Lakatos such that research follows a set of rules—for example, maximizing the rate of growth of scientific results of certain kinds. The *effectiveness* of actual research may then be measured relative to the ideal. But I think one should be careful not to identify rationality of research

with any ideal that takes research as a completely autonomous activity and postulates one particular measure—for example, rate of growth of certain kinds of results. Rationality should be measured, if at all, in relation to maximally wide systems—that is, systematic philosophies.

Research policy may be rational even if “maximization of scientific rate of growth” has a very low priority. Lakatos sees a threat to rationality in Feyerabend’s hedonistic proclamation “Do as a researcher what you wish and have a pleasant time” (Feyerabend 1970a)⁶; but *if made explicit*, requirements of joy in research need not lower rationality, only effectiveness. Continued tests of theory *A* may be found excessively tedious and abandoned mainly for that reason. The shift to other theories may in this case be perfectly rational but it is clearly against an effectiveness-promoting rate of scientific growth. Scientific enterprise may show continuous rationality under variation of effectiveness or continuous effectiveness under variations of rationality.

As described by Lakatos, a researcher, in order to be rational, seems to need unusual intellectual brilliance combined with narrowness of vision and interest. Why can’t a rational researcher be of moderate intellectual ability and pursue his research in the spirit of an amateur or of a man *primarily* devoted to social reform, not to science?

Human interests, passions, and idiosyncracies do not necessarily interfere with the rationality of research. Rationality means primarily rationality of decisions. If a researcher consciously and deliberately chooses a theory in part because testing it necessitates a long-desired journey to the Antarctic, it may reduce effectiveness (for example, speed of growth of research, number of new astonishing facts), but his research as a human activity is more rational than if he chooses the same theory under the illusion that it necessitates a trip to Africa.

At this point it might be clarifying to examine a well-known pronouncement of Willard Quine (1953: 43): “Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system.”

As a slogan, a jubilant expression of freedom in theorizing, this sentence has great value even without accompanying explanations. From the point of view of scientific research, however, something should be added: “holding true” does not imply, in research, entertaining a belief as true, but treating it as if true, acting or deciding in a way that would not be dif-

ferent *if* one considered the theory true. Further, it might be added that in research we only make drastic adjustments *for a limited purpose*, and they are all *revocable*. And sometimes we need not bother making adjustments, because “Is it *true*?” is a rare question in evaluating theories.

Looking back at the history of such famous theories as those on acids in chemistry or on light in physics, perhaps much faster progress could have been made if some scientists had not tended either to accept them *as true* or reject them *as false*. A more differentiated strategy, applying “drastic adjustments” in the face of major setbacks from observational disconfirmations, would have kept alive—that is, kept up to date—more *theoretical alternatives*. Such a strategy could have made it much easier to compare the virtues of different theoretical approaches.

Suppose that from a well-established theory we derive the statement that a certain kind of event is physically impossible (for example, that, according to Einstein’s theory of relativity, it is impossible to increase the speed of a material particle from a level under the velocity of light to a level at or above that limit). From a wider perspective we may nevertheless accept it as physically possible—that is, from theories constructed by modifying at least one sentence in an exposition of the original theory without changing the level of observational confirmation and positive relations to other (established) theories. That we can always do this is part of what is asserted by a metatheorem we shall call the Mach-Duhem-Poincaré principle, which is briefly formulated as follows:

Given a set of observational sentences, a set of theoretical sentences incapable of direct observational test, and sets of initial sentences such that the theoretical sentences joined with appropriate parts of the initial sentences permit derivation of the observational sentences, there are indefinitely many other, mutually incompatible, sets of theoretical sentences such that the given set of observational sentences can be derived from them. The expression “there are” may be replaced with “if n sets are available, it is possible in practice to construct a set $(n + 1)$ fulfilling the conditions.” “Incompatible” is used in a wide sense: there is incompatibility if there is at least one observational sentence derivable from the one set that contradicts an observational sentence derivable from the other.

But, it might be objected, there is no *guarantee* that such a variety of *nontrivial* theories can be found. And even if they are, the modified forms may not be acceptable to researchers as ones they could *work with*. These

objections do not hit the mark, because the *possibilities from wider perspectives* do not pretend to be as, or even more, acceptable to the active researcher within his specialized sphere of theoretical activity. The widening of perspective has an importance independent of any such pretension.

Pragmatic-Heuristic Component of Conceptions of Refutation

The picture of the test process given in chapter 2, pages 20–21, seems to indicate that there can be no refutation without the assumption that each member of the set of initial-condition assertions, *I*, can be conclusively *verified* as part of the test procedure. Many of the members normally have the character of conjectures, perhaps refutable, but scarcely verifiable. And what is worse, it is to a large extent arbitrary how many assertions are included in *I*.

The absence of refutations does not harm the development of research, because rejection or acceptance (of working theories and hypotheses) does not depend on refutation. The *justification* of a decision to reject or continue work with a hypothesis is given by answering a long series of questions on its status. The situation as regards instances of observational disconfirmation is only one factor.

The position that there can be no conclusive refutation, however, is misleading. The formulae have been adapted to the contemporary two-valued logic of truth and falsity. But as used by scientists and historians of science, the terms “verify,” “falsify,” “refute,” “establish,” and other terms closely related to the process of testing *theories* have not been narrowly tied down to the true/false distinction of formal logic. Only compelling reasons should make us depart from this kind of usage. The instrumental aspect, the functioning of a theory as a tool in research, has always been a consideration when assessing the weight of a confirmation or disconfirmation, by observation or by theory. Thus the borderline between acceptability and unacceptability, between the status of being established and being not quite established, between conclusive and not conclusive confirmation or disconfirmation, between tenability and untenability, has no definite relation to the distinction between “established as true” or “not established as true.” Here I am tempted to quote Ernest Nagel extensively and approvingly:

The use of theories is one patent factor in reflective inquiry. . . . Reflective inquiry is instituted for the sake of settling a *specific* problem, whether it be practical or theoretical, and inquiry terminates when a resolution of the problem is obtained. The various procedures distinguishable in inquiry (such as observation, operation on subject matter including the manipulation of instrumental symbolic representation of prophetics of subject matter, symbolic transformation and calculation, etc.) are to be viewed as instrumental to its end product. . . . Accordingly, in their actual use in science, theories serve as *instruments* in specific contexts, and in this capacity are to be characterized as good or bad, effective or ineffective, rather than true or false or probable. Those who stress the instrumental function of theories are not necessarily committed to identifying truth with effectiveness and falsity with uselessness. Their major insight does not consist of denying the meaningfulness of certain types of inquiries into the truth of theories but in calling attention to the way theories function and to the safeguards and conditions of their effectiveness. (Nagel 1960: 264–65)

These quotations, taken from Nagel's "final remarks" in his paper "Probability and Degree of Confirmation," point to a restrained instrumentalist view that does not commit its advocates to identify "falsity with uselessness" and, I would add, that does not identify rejection of a theory with rejection of a truth or confirmation claim.⁷ Having in mind how theories function within the tremendous complexity of the total research process, it is clear that the introduction of ad hoc auxiliary hypotheses, the ad hoc changing of assumed truth values of some hypotheses (some H_i 's), or of some statements of initial conditions or of experimental setup (some I_i 's and E_i 's), may very well be part of an excellent research policy; such actions need not reflect any diminished attention to intellectual honesty or love of truth. The passage quoted from Nagel becomes misleadingly instrumental only if we ignore his careful wording: "[T]heories serve as *instruments* in specific contexts, and in this capacity . . ." He does not say (or imply) that theories do not serve in other capacities.

Feyerabend has argued convincingly how certain hypotheses that are false and inconsistent—which the scientific community, for good reasons, accepts—may nevertheless function as the nucleus of condensation for new kinds of evidence and the construction of new sets of interrelated hypotheses. Perhaps only after several generations, these hypotheses become corroborated and consistent to a degree comparable to the hypotheses that they supersede.⁸

There is nothing paradoxical about new insights or the building up of a new (or old) theory sufficiently remote in content from the dominant one, starting from a basis that may seem ludicrously slight from the point of view of the dominant theory. The scientist who goes against the dominant trend must *trust* or, if of a cooler temperament, at least “presume ad hoc” the emergence of new kinds of evidence and interrelations. There can be no rational basis for this kind of trust or presumption as judged from internal standards of research. The scientist develops and elaborates a “mere” *possibility*. The rationality, in a narrow sense, of his choices can only be argued in retrospect, if at all. But, as we shall argue later, criteria of rationality may be introduced relative to wide philosophical systems.

It is perhaps the greatest merit of the new historiography that it encourages *both* stubbornness in face of near-universal, rationally argued resistance within the scientific community and the legitimacy of an “orthodoxy” in the sense of continued work with a definite “traditional” theory and practice as long as no alternatives are in the making and in spite of disconfirmations and paradoxes.

The foregoing arguments in favor of stubbornness concern new theories. They should be generalized, however, because in a very vigorous research climate, all theories have the character that we have attributed above only to new theories. When there are no “resting places” for theories because research is carried out in all directions at once, the shortcomings of any theory are revealed more easily and more frequently. There is a continuous stream of evidence and counterevidence. Theories that from a superficial or narrow point of view are “refuted” are, under favorable circumstances, kept up to date, thus making it difficult or impossible for any definitive version of any theory to dominate or enslave the imagination.

Let me once more mention the theory-observation relation in a “competitive market”—that is, in environments with several competing theories. Let $\{O+\}$ symbolize the set of those observational sentences (predictions) derived from theory S that have been tested and given a positive result. (“What was predicted was also found to happen.”) Let $\{O-\}$ symbolize the corresponding set that has given negative results. Let $\{O+\}$ and $\{O-\}$ be the corresponding sets related to a second theory S' . Some of the more important relations are as follows:

$$(1) \quad \begin{cases} \{O+\} \subset \{O'+\} \\ \{O'-\} \subset \{O'-\} \end{cases}$$

In this case S and S' are eminently comparable as regards evidential, observational support (corroboration), and S' is unambiguously the better theory, the superior theory *in this respect*.

Nearly all other kinds of cases lack the unambiguity exhibited in (1):

$$(2) \quad \begin{cases} \{O+\} \subset \{O'+\} \\ \{O'-\} \subset \{O'-\} \end{cases}$$

In this case S' is superior in relation to “strengthening instances,” whereas S is superior in relation to “weakening instances.” Most other cases are even less clear-cut—for example, the cases in which some fields in which the theories have been tested are more or less disparate.

Test of Isolated Hypotheses Practicable

The assessment of the test must take into account the details of the use the scientist makes, or rather proposes to make, of the theory, within his research program (with its policy declaration) in its relation to the situation at the frontier.

Duhem argued that when we say that we test one definite hypothesis, we are really testing a whole theory involving that hypothesis. We might as well add that we then also test *all* auxiliary statements implicitly assumed to be true when making the experiment that is said to test the (isolated) hypothesis. But this may seem rather strange. Do we really test things that we do not know about and assumptions we have never *intended* to test?

There are two distinct problems involved: that of how to test a given (isolated) hypothesis by planned manipulation of certain variables, and that of assessing the possible influence of other variables on the results of the test. If a hypothesis is tested by determining the position of a star image through the use of a telescope, vast fields of optics, physical and physiological, are involved.⁹ The outcome and interpretation of the test *depends on*, and is *influenced by*, all these theories, hypotheses, observations, assumptions, postulates, and so forth. The astronomer must ask: Is the

result of the measurement of the position *unduly* influenced by an imperfection in the telescope—an imperfection perhaps due to a mistake in the fundamental theories of optics used as the basis? Is there something suspicious about the position of the telescope and the vast number of other details of the experimental setup specific for testing this definite hypothesis? Or are things exactly as they used to be? But little is gained by conceiving the astronomer's situation as one in which *he really tests more than he says he tests*. I therefore conclude that a test with an experimental design adequately adapted to the test of an isolated hypothesis *does* test this very hypothesis and nothing else. It does not *test* the other hypotheses of the theory nor the auxiliary hypotheses. But the test may have *repercussions* on the status of these other statements. And the assessment of the quality of the test may change from year to year because of changes in the status of some theories, hypotheses, and so forth, that were relevant to the test.

Incomparability Due to Differences in Conceptual Framework

Let us refer again to Duhem. The agreement of a theory with established laws is, according to Duhem, mainly or wholly a question of derivability. This means that it depends mainly on inventions in mathematics and logic. Revolutionary progress in physics therefore cannot be expected to come from disagreement between a dominant theory and physical laws.

If a theory asserts a definite relation R_1 to hold between a set of variables—for example, mass, core charge, velocity, and so forth—disconfirmations suggest different relations R_2, R_3, \dots between *the same* variables. New laws, or strong disconfirmations of some of the old, may be said *logically to necessitate* some kind of modification of a theory in order to ensure coherence. But they cannot necessitate a change of *theoretical concepts*. Let A, B, C, D be the theoretical concepts in terms of which “established” theoretical laws are formulated. Any theoretical laws *contradicting* the established ones must be *in terms* of the established ones—how could there otherwise be contradiction? Adoption of new theoretical concepts is never forced on us by a negative trend in results of observational tests.

If, now, Duhem's view is taken seriously, changes in theory will only or mainly be dependent on changes in opinion about which theoretical laws are correct. Given certain laws, maximum changes will therefore

remain inside the realm of formulae using the old set of theoretical concepts. But revolutionary change involves new concepts and new ideas, which are *not* definable in terms of the old. It seems therefore that the Duhemian approach does not account for radically new theories.

The basic ideas or conceptual framework of a theory cannot be affected by sets of observations either, since both confirmations *and* *disconfirmations* are *in terms of* that framework. With appropriate changes—that is, modifications of the theory—disconfirmatory instances are changed into confirmatory or irrelevant data—but the conceptual framework is retained intact.

We may even form a kind of principle of *perpetuum immobile*: No theoretical or practical work aiming at testing certain theoretical laws or a definite theory, taken in isolation, can bring forth evidence against the basic conceptual structure of the theory. The worst that can happen to a molecular theory is that it seems that molecules behave very differently from what the theory says—that *they* have quite different properties. But this does not refute the existence of molecules or invalidate the concept “molecule” in the basic structure of the theory. What happened in the case of the instinct theories in psychology before their abandonment was not the refutation of the existence of instincts, but, on the contrary, the excessive proliferation of instincts. In his survey, Luther Bernard (1924) listed 842. (The example is not perfect because most “theories” of instincts do not fulfill reasonable or even minimal conceptual requirements of a theory such as those mentioned in chapter 3 here.)

If two theories are mutually inconsistent, I repeat, they may nevertheless both be confirmed or both disconfirmed by exactly the same set of established theoretical laws. This is due to the circumstance that a theory has more consequences than *just* the set of current accepted laws. But if a new theory has a different conceptual framework from the old, the theoretical laws derivable from the old theory cannot be derived from the new. From a theory in terms of concepts *A, B, C, D*, only laws in terms of *A, B, C, D* can possibly be derived. A law in terms of the concept *E* cannot be derived except when *E* is reducible to *A, B, C, D* or definable by these concepts.

What happens in actual practice, of course, is that the terms of the old theoretical laws are slightly or extensively redefined—mostly without any corresponding change in symbols. Stability in the linguistic apparatus provides an appearance of continuity. The letter *m* and the four letters *mass*

will be retained as long as there are mnemotechnical reasons for retaining them, and those reasons have little to do with meaning- or use-constancy.

Turning now to empirical laws, the same conclusion holds, but perhaps it calls for additional commentary. “Metals expand when heated” contains three key terms used to refer to everyday observables and none of these terms are specific to any competing theories. Two mutually inconsistent or theoretically incomparable theories may therefore both permit the quasi-derivation of “Metals expand when heated.”

I say “quasi” because one needs, in addition to the theories themselves, different sets of “rules of correspondence,” or “*Zuordnungsdefinitionen*” (Reichenbach), that connect the observables with vast theoretical, conceptual frameworks. Because of the one-many relations between the terms of the empirical laws and the theoretical terms, the confirmation of one theory implies neither a disconfirmation of theories contradicting the first nor a disconfirmation of theories with incomparable conceptual frameworks.

Suppose that in the time interval $t_2 - t_1$ a number of natural laws are said to be established and that an accepted theory T_1 (at time t_2) is found to be inconsistent with one or more of the laws. Some would take T_1 to be refuted, others not. The room for genuine disagreements is seen to be considerable since, first, it is not establishable a priori whether T_1 need be in disharmony with any single *observation*, and second, even if it is in disagreement with such an observation, it is not specified whether there are other theories, T_2, T_3, \dots that can take over the functions of T_1 more satisfactorily. If one takes refutation to justify abandonment, both conditions must apply in order to classify the theory as “refuted.”

Let us use an illustration from the calculus of propositions: that q implies r , and p is inconsistent with q , does not exclude that p implies r . The conjunction

$$(q \supset r) \ \& \ \sim (p \ \& \ q) \ \& \ (p \supset r)$$

has the relative strength $s = \frac{4}{8} = 0.500$ —that is, less than the strength of a conjunction of two variables, and far from $s = 1.000$, the relative strength of a contradiction. From the laws (corresponding to a part of q , the rest being *Randbedingung* statements), the observations (in our illustration: r) are derivable, but this does not exclude their also being derivable

from the theory T_1 (in our illustration: p) in spite of the inconsistency of T , with the laws (in our illustration: the inconsistency of p with q). This is essentially the pluralism of derivation on which the principle of Mach-Duhem-Poincaré rests (see p. 37).

Thus, any reported refutation of a theory by natural laws, or by any other theoretical statement “established” by observation, is relative to the outcome of attempts to link the theory to observations *through channels other than the refuting laws*. Now, to relate theories to observations is a crucial task for researchers, and imperfect solutions mean imperfect decidability of the weight of a “refutation.”

In the light of these considerations, it might be deemed surprising that *new* theories are often in vehement conflict with some well-established laws. But if it were expected of a new theory that even in its first version it should not contradict any established law, there would be so few proposals that the theoretical development of science would practically halt. Furthermore, an established law can be deeply modified without losing its admirable relation to observation. Such modification may subsequently bring the law into harmony with the theory.

This may justify a short discussion of a point made by Duhem. He concludes his treatment of testing theories with the dictum that the “only experimental check on a physical theory that is not illogical consists in comparing the *entire system of the physical theory* with the *whole group of experimental laws*, and in judging whether the latter is represented by the former in a satisfactory manner” (Duhem 1962: 200). This might be so if we write “alogical” instead of “illogical.” If laws already formulated in terms of a definite theory are badly represented (I would say badly “accounted for,” “addressed,” or “covered”) by it, it certainly speaks against the theory, but of more decisive importance are comparisons of competing theories in relation to carefully selected experiments. The comparison is alogical in the sense that the conceptual framework by means of which the experiment gets its theoretical import is different for the different theories. When concepts are thus different and mutually untranslatable in practice, the comparison cannot be “logical” insofar as the theories seem to speak of different things. They are not confronted with the same experiment as judged from the sentences expressing the experiment’s theoretical import.

To follow the letter of Duhem’s dictum would be unnaturally to restrict work with theories; designing and testing a theory would become

an exercise in logic and mathematics. What Duhem calls an “experimental” check differs little, if at all, from a logical mathematical check. The relation between law and theory is one of derivability. But if testing a theory is testing this relation, why make experiments? The comparison of two competing theories, and assessment of tests applied to both, call for an assessment of the relation of the theories *to observations*, not just to laws. A theory may be victorious because of its relation to certain crucially important observations—in spite of conflicts with “established laws.” Ernest Rutherford once said retrospectively: “I was perfectly aware when I put forward the theory of the nuclear atom that according to classical theory the electron ought to fall into the nucleus . . .” (Holton and Roller 1958: 624). The conflict was not only with the only competing theory then at hand, but with laws derivable from the theory and based on an immense number of confirmatory instances. Nevertheless, Rutherford’s theory did not conflict with any observations! Nobody had observed whether electrons tend to fall into the nucleus. If laws, as Mach proposed, should be *denkökonomische* condensations of observational data, conflict with laws would be very much more serious than it is. But the relation between the number of observationally established instances of a law and the total number of instances covered by it is as that between a finite and an infinite, or at least indefinitely large, number.

Proliferation of Concepts of Refutation: Pluralism

The historical sources of cases of refutation are texts of various kinds and recorded discussions among scientists. These sometimes end with the rejection of a theory or hypothesis. We are interested in the reasons for rejection, however, and whether they amount to refutations (in various senses) or not. The rejection and abandonment of a theory by a scientist or a community is also more easily inferred from the sources than a refutation. No more use, perhaps, is made of the theory, and the historian asks, “What happened?” He is sure that the theory was rejected and abandoned, but to give the exact reasons is a task of speculation and reconstruction, however rich the sources.

The new historiography has amplified our knowledge of a number of reasons for rejection and consequent abandonment. They are scarcely less

complicated than reasons for social rejection and ostracism. Just as answers like “We don’t like him” or “He’s undesirable” to the question “Why ostracism?” will not satisfy a logician of social relations, the answer “It is falsified or strongly disconfirmed” will not satisfy a logician of science. Logical reconstructions that have any value to research must systematize the reasons, for instance in sets of necessary and sufficient conditions. Such sets will, of course, only approximately, and within limited domains, “cover satisfactorily” the historical sources, but this holds well for any logical construction with descriptive aims. In what follows we have natural science in mind. It is unlikely that a logical reconstruction of processes of theory- (or even hypothesis-) rejection can be disentangled from the special topic (chemical, physiological, sociological, political) to such a degree that it satisfactorily fits *any* theory or *any* hypothesis. At least preliminarily one might have to speak of “rejection in physics since Galileo,” “rejection in experimental psychology,” etc.

The historical sources of the new historiography are overwhelmingly rich and detailed compared with what was available thirty years ago, but there is still a long way to go from these to fruitful conceptual reconstructions of the historical processes. The very richness of the sources makes it psychologically more difficult to make pronouncements on the level of complete generality. And there is even further to go if one wishes to propose methodological norms of rejection.

Our main concern in this essay is not to decide for or against particular conceptions of refutation and refutability, but to point to the room for a *diversity of conceptions*. The procurement of this room is secured by systematic elaboration of the *differences of descriptions by scientists and by historians of particular cases* of falsification, refutation, or rejection. Case histories show great variation and provide the best antidote against prejudices as to what a refutation *ought to be*.

There is a substantial basis for similar pluralism in regard to conceptions of other crucial phenomena relevant to scientific development. This opinion will be discussed in some detail in chapter 4.

From the “economy of thought” approach of Mach, Avenarius, and contemporary pragmatic philosophers of science, rejection is ultimately a question of economics. When it “does not pay” to expend more effort, make more auxiliary hypotheses, or more complications, the theory is rejected.

From the “falsificationist” and “testability” approach of Popper, rejections are conceived more in terms of the true/false distinction and the question of frequency and severity of tests.

But these two examples represent only one major class of dimensions of rejection. We have also the dimensions of consistency, generality, and severity of rejection. A theory may be rejected within one of several domains covered or in all domains. It may be rejected only when very special requirements of exactness of prediction are laid down or, on the contrary, rejected only when a low degree of exactness is required. Further, the theory may be rejected by simple neglect—interest shifting to others until it is so out of date that it is found impracticable to revise it.

Very rarely are the deaths of theories heralded by explicit, careful refutations with well-formulated premises. A theory may simply be talked of less and less, and the predicates used to characterize it become more negative, harsher. Nobody concerned may ever have produced or seen a formal refutation. Thus we have to consider still another dimension: the variety of requirements (or lack of such) concerning *explicitness*. If a theory simply is not heard of anymore (historical sources are silent), is it rejected? What, in short, are the criteria that characterize an *act* of rejection?

It is not our aim here to go into detail, but only to suggest that rational reconstruction of conceptions of rejection and refutation can only proceed fruitfully on a pluralist basis.

Conclusion: The rules and considerations leading up to a verdict, or rather to a decision of “refuted!” or “not refuted!” cannot be isolated from purely heuristic ones. Criteria of truth or falsity and criteria of the goodness of theories have never been as clearly distinguished in practice as they have in discussions of the “logic” of the practice.

The complex relations of actual and potential fields of testing preclude clear-cut decisions as to rejection or acceptance on the basis of observation.

If we are interested in the *logic* of refutation, what kind of symbolic structures should we use? In order to do justice to some of the complexities inherent in the life and death of theories, I think the “logic of indices” might fruitfully be applied. Economists and other social scientists have, for the purpose of condensed comparison, developed indices of, for example, the standard of living, overcrowding, popularity (for example, of

politicians), air pollution, degree of democracy, and many other magnitudes. A great number of factors have to be investigated in order to calculate such mostly quantitative indices, one major concern being the *careful, explicit* formulation of each item or factor relevant to the result. Each researcher might of course propose his own index, but this would defeat the main purpose: to have standard ways of condensed comparison within the research community or within even larger groups. The status of theories in regard to refutation and corroboration may, at some time in the future, be recorded by research teams issuing monthly bulletins that end with index tabulations—neither taken too seriously nor completely neglected by research communities.

III

Theory and Theoretical Idea

Theories: Variety of Notions

The vigor and confusion of the debate on the nature of theories is in no small measure due to the same factor: the vagueness and ambiguity of the term “theory.”

Sometimes the term is used for propositions of any level of preciseness that are not expressions of observations. Thus, Popper (1962: 194) speaks about idealism as the theory that “the world is my dream.” It is, according to him, a false but irrefutable “theory.”

Any assertion of hypothetical character, however vague or trivial, seems to be called a “theory” by Popper and many Popperians (for example, Lakatos). Still others use the term in a very broad way but seem sometimes to require that it contain at least one universal quantifier. If this is the only requirement, one is forced to include the ill-famed “all ravens are black” among our theories and also other primitive generalizations.

Carnap’s concepts of empirical law, theoretical law, and theory provide a better frame of reference. Here the inevitable “all ravens are black” occurs as an empirical law, not as a theory. Empirical laws of temperature are derived from theoretical laws (parts of theories?) by a correspondence rule. “The temperature of the gas corresponds to the mean kinetic energy of molecules” (Carnap 1966: 241). New theories must permit derivation of new empirical laws; those that do not are logically equivalent to the old theories.

Then there are conceptualizations of “theory” that are too narrow for our purposes—for example, those requiring a detailed explicit exposition in the form of a hypothetico-deductive system. The terminology of Duhem is very special, defining physical “theory” as a set of mathematical propositions from which laws can be derived: “What is a physical theory? A group of mathematical propositions whose consequences are to repre-

sent the data of experiment; the validity of a theory is measured by the number of experimental laws it represents and by the degree of precision with which it represents them" (Duhem 1962: 288).

In the last two chapters I deal with complicated sets of fairly precise propositions of various theoretical and observational levels. I need a concept of theory adapted to this purpose. In what follows I therefore take as paradigms of theories in my terminology the wave- and particle-theories of light; the kinetic theory of gases; in chemistry, the theories of acids; and in experimental psychology, Hull's theory of learning, Tolman's sowbug theory of vicarious trial and error (VTE), and Homan's theory of social behavior as exchange.

These complicated structures certainly contain, or *may* be formulated so as to contain, general statements, but there are other features of a theory deserving more notice: (1) *The merely indirect relation to observation*. No observation statement is part of the theory. (2) *The multiplicity of levels*: levels of fundamentals or, better, principles, levels of hypotheses, and laws. (Level of consequences directly testable by observation is not included. It would have to include statements about initial conditions.) (3) *The derivability or quasi-derivability of consequences* and, among them, predictions—observation sentences from fundamental propositions of the theory (when initial statements and so forth are added). (4) *The multiple problem-solving function* (not inherent in "all ravens are black"!), suggesting vast research programs. (5) *The presence of a fairly simple unifying idea* (conception, not only concept) behind the professional (scientific, precise) formulation of the fundamental features.

I have stressed the characteristics most important for the problems dealt with in this book.

I find this general structure, especially point (2), more helpful when studying theories than the one proposed by the many philosophers of science who are mainly inspired by logical analysis. One cannot, without great arbitrariness, take an abstract calculus as one of the three main components of (the formal structure of) a theory. Consequently, it is awkward, too, to make into another component "a set of rules that in effect assign an empirical content to the abstract calculus" (Nagel 1961: 90). This means that we must be doubtful of the significance of the important concepts of *Zuordnungsdefinitionen* and *rules of correspondence* carefully worked out by Carnap, Reichenbach, and others in the 1930s.¹ The main point is that if a

theory is a physical theory, all three levels are physical. There is no “uninterpreted” level. The phrase “a theory may have different interpretations” (Feyerabend and others) is avoided because it suggests that there exist uninterpreted theories or theories not yet interpreted.

Similarly, if a theory is biological, all its levels are biological; none of them are a province of logical or mathematical calculus. Recent work in the philosophy of quantum physics indicates that no exception need be made because of the supposed nonphysical, purely mathematical, instrumental character of the ψ -function and other esoteric symbols.

However important it is for the logician of science to introduce distinctions “so that attention is directed exclusively to the logical relations,” the dynamics of scientific test procedures require very different conceptualizations. The three levels of statements proposed above only provide a model of the formal or linguistic structure of a theory. One must be careful not to *identify* the theory with that structure. If such an identification is made, scientists seem to jump from one theory to another, falsely pretending they are “the same” theories. Or they seem not to recognize clear cases of “refutations.”

The above five characteristics relate mainly to the anatomy or statics of theories. We take our lead from the excellent rule practiced in some countries for defining patents: never give a patent for the *function* of an invention. In defining a lamp for a patent, we must specify its structure (anatomy) and what it is made of, but not how well it lights a room (physiology) or how nice its color is (psychology). But a definition of essentials (*definitio essentiae*) must include functions. In the case of theories, these include the properties brought to light by study of their uses and jobs, of the interaction of one theory with others, and of their place in research activity, programs, policy, strategy, tactics, and in applications that have no research pretensions. All this is relevant to any adequate *essential definition* of a theory, just as the essential definition of an organ of a living animal has to include references not only to anatomy, but also to physiology and acquired abilities. Therefore, I do not take the listed five characteristics to constitute an essential definition.

In order to avoid too much laxity in distinguishing logical, and more generally cognitive, aspects of a theory from the genetic, psychological, sociological, and political aspects, it is convenient to distinguish theorizing from theory. Theorizing is an activity manifested by a theorizer, and it

includes the construction of theories. But the predicates applicable to theorizing are not always predicates applicable to theories. I shall have to go into detail on this theme later when assessing the large-scale attack by continental philosophers of science on what is called *the illusion of pure theory*. Two central questions need clarification: To what extent is this an attack on pure theorizing rather than on pure theory? How “deep” into a conceptual framework within general systems does the distinction between activity and content go?

Whereas the use of the term “theory” and its relations to that of the terms “law,” “hypothesis,” “principle,” and “conceptual framework” show a tendency toward stabilization within the fields of philosophy of physics, in the humanistic disciplines all is still in flux. Reginald Robson (1968: 349) has helpfully distinguished four uses of “theory” in sociology: “theory” as a general name for social ideas and theoretical thinking; theory as social commentary or analysis of social phenomena; theories as taxonomic systems—more or less systematic or comprehensive sets of concepts; and a theory as “a set of interrelated, general propositions that seek to explain empirical uniformities and from which one can deduce further hypotheses for empirical tests.”

Talcott Parsons’s use of “theory” in his influential *Toward a General Theory of Social Action* neatly fits the third of these uses. “His contribution amounts to a ‘conceptual framework’ or ‘frame of reference’ which provides a set of concepts which can be used in describing a vast array of social behaviour” (Robson 1968: 355). Robson formulates conclusions of a team of sociologists evaluating the work of Parsons. I find Robson’s terminology well chosen; the only thing I object to is his tendency to expect fruitful theories to be rather general in scope. Within the vast field of the psychology of learning there are a number of well-defined specific theories, but the general theories are either tautologous or too vague to be of any use—except as conceptual frameworks. Hans Zetterberg’s view of the size of fruitful theories is more realistic (Zetterberg 1965).

Whereas the reading of the great classics of physics will largely be a reading of theories, the reading of the classics in sociology, it seems to me, will largely be a reading of theorizing—especially the wise or acute analysis of social phenomena. Think of Tocqueville, Znaniecki, and others! There are scarcely any theories to pinpoint in the works of these theorists.

Conclusion: The importance of theories for the development of social science is not as great as in natural science. To say “is not as yet as great” would be to reveal some of one’s tendencies in philosophy of science, perhaps a misleading submission of ideals of social science to those of nomothetic natural science.

One more comment on the use of the term *theory* in sociology should be mentioned. Zetterberg writes:

First, there is a habit of designating all the better sociological writings of older vintage as “social theory.” . . . An alternative and better term would be “sociological classics.” . . . A second conception of “social theory” . . . equates it with a commentary on sociological writing, usually from an historical perspective. . . . “Theory” here means essentially “sociological criticism.”

(Zetterberg 1965: 6–8)

Third, there is “theory” in the sense of “taxonomy.” Zetterberg, like Robson, uses Parsons’s frame of conceptual reference as a paradigm. Fourth, there are “systematically organized, lawlike propositions about society that can be supported by evidence” (*ibid.*, p. 22).

Of the many important reasons for reporting these tentative classifications of senses of “theory” in sociology, there is one that has to do with the current interaction between continental and American sociology (and political science). Representatives of the continental trend sometimes take it for granted that the limitation of the term *theory* to both Robson’s and Zetterberg’s fourth sense implies a devaluation or criticism of works falling under the first three classes, or a program of future elimination of those kinds of work. But this elimination is a suicidal approach and is hardly recommended by any contemporary metatheorist of sociology. Both Robson’s and Zetterberg’s fourth sense differ from that of the term *theory* adopted in this work, especially by leaving out the theoretical idea. A set of interrelated propositions without a unifying idea cannot be a theory in my sense, but then the aims of those writers are different from mine. There is no “disagreement.”

The intricate relation between theoretical idea, theory formulation, and observational test can be studied by comparing the achievements of Julius Robert Mayer, James Prescott Joule, and Hermann von Helmholtz in developing the principle of conservation of energy. Mayer “found the new idea,” and the “fame of discovery,” therefore, belongs largely to

him—according to what Helmholtz himself made clear. But *exactly* what idea? Mayer starts from the postulate *causa aequat effectum* and the dictum that energies are causes. He deduces, in the best of metaphysical traditions, that energies are “*indestructible convertible entities*” (Holton and Roller 1962: 351–57)—like all other kinds of causes. *This* is the idea. . . .

It is clear that there is a long way to go from the contemplation and formulation of this idea to a professional statement of a theory in physics. Helmholtz did it—and most elegantly. But the next hundred years of development of conservation principles show that neither Helmholtz’s *formulation*, nor any other, could take over the role of the idea. Every modification and elaboration had to get its inspiration from, and take its cue from, “the” idea—“the” in quotes because there is of course no *definite* idea, but a family of ideas with vague outlines.

To James Joule goes the honor of having invented a suitable experimental setup for testing the theoretical statements. His and other experimental designs are interwoven with ad hoc theoretical assumptions. Just like Mayer, his conviction that the energies *are* conserved had a metaphysical background—and content. He thought it would be “manifestly absurd to suppose that the powers with which God has endowed matter can be destroyed” (ibid.).

The idea of theories mirroring reality, being somehow isomorphic with the structure of how things really are, still plays a considerable role in sociology and political science. The theories of one-dimensional society (Marcuse 1964), of the structure of dominance in highly industrialized Western societies, and of the breakdown of communication (Jürgen Habermas) may be taken either as ideal-typical constructs, which are social science’s functional equivalents of models in natural science, or as mirrors of reality. If the latter, it is easier simply to take them *to be true*, to act on them, and to form political strategies based on them. If we take a social theory to be a construct or a model, however, the way from theory to political practice is more intricate. This is perhaps one of the reasons why the mirroring conception of theory is still not only alive, but growing in importance socially with increasing political activism.

The great historical theories are in a sense more or less permanent crystallizations of certain ideas that represent a quite unusually happy combination of hunches—of fluid, fragmentary ideas constantly cropping up in the intensively active minds of great researchers.

Both the fluidity and the diversity of these fragments can be judged from a recent contribution to the materials of psychology and sociology of discovery—the autobiographical account by James Watson (1968) of the discovery of the three-dimensional structure of DNA. What he and his collaborators were looking for was not a theory, but a “correct hypothesis.” Nevertheless, theoretical ideas in great numbers—most of them, of course, borrowed from others—were cropping up in their minds. Tests were made, mostly with accepted theories and hypotheses, sometimes by confrontation with observational journals, and ideas were dropped—as wrong, misleading, insufficient, etc. The account is excellent as an illustration of *theorizing* and of the complex relations between loose, fragmentary ideas, relatively well-formulated ideas with some clearly worked out consequences, and elaborate, systematic ideas on the way toward adequate expression as a precisely formulated theory.

The differentiation of theories from related phenomena requires a short treatment of the trilogy of theory, working theory, and theoretical research program.

The term *working hypothesis* has long been used for a tentatively, deliberately adopted-until-further-notice hypothesis *during* a research activity. What one does with such a hypothesis is, among other things, to derive its consequences; to compare them with those of other (incompatible) hypotheses; to test predictions (singular statements testable by observation); to investigate which already accepted theories might have the hypothesis as a consequence and which not; and further, to look for experimental setups that might increase testability. These and other activities are normally expected to furnish premises for decisions on whether to rely more heavily on the hypothesis as part of a wider research program or to drop it.

The term *working theory* is used less but can be given a strictly parallel use, except that working with a theory is still more complicated and has more aspects.

The notion of a “working theory” leads naturally to the consideration of research programs and their relation to theories. It is quite customary, as we have already mentioned, to formulate decisive parts of a program in the shape of a theory. “Complexes are really attitudes”: this formula was used by academic behaviorist psychologists to express a program, namely that of systematically reformulating psychoanalytic hypotheses about

complexes in terms of attitudes—that is, behavioristically into patterns of reactions. If successful, the program leads to the establishing of a theory, but at least at the initial stages the theorylike formulations are not expressive of any theory. It is a theory *in spe*, not even a “budding” theory.

The very close relation between the performatory use of theory formulations, namely as expressions of a program, and the more stiffly cognitive use makes it important to map out the differences between the conditions of programs and those of theories. The decision to start, continue, or close a program may be more or less wise, rational, clever, well-founded, or realistic. The refusal to give it up may be more or less stubborn, tenacious, etc., but not dogmatic as in the case of a theory. The designations “true,” “false,” “tenable,” “corroborated,” “confirmed,” “refuted,” etc. do not apply to programs (some of them are misplaced even in the case of theories).

The notion of a research program is used by Lakatos (cf. 1970) to express what I would call “programs of maximal size and of philosophical import.” He uses as a paradigm “the cartesian metaphysics C”: “in all natural processes *there is* a clockwork mechanism regulated by (*a priori*) animating principles” (Lakatos 1968a: 179). Lakatos argues convincingly that it serves the growth of scientific knowledge to stick to such a program even if it is temporarily untestable by basic statements, in Popper’s sense.

Theories: Names, Expositions, Versions, and Modifications

Let me begin with an example. The term “Bohr’s theory of the hydrogen atom” does not, and never did, refer to a definite set of propositions. There is a conceptual scheme, an idea, that in the hands of a genius affected a formidable conceptual breakthrough. The scheme does not permit exact delimitation but is perhaps most easily approached through the theoretical postulates or assumptions by which Bohr was able to derive a vast number of empirical laws summarized by the formula:

$$\frac{I}{\lambda} = R \left(\frac{1}{m^2} - \frac{1}{n^2} \right)$$

Essential aspects of the scheme are the following: The electron can move only along certain orbits, the “permissible” ones. Circulating there, the atom does not radiate anything. This is a famous *ad hoc* assumption, or, rather, postulate. Testability when accepted: zero. The electron may

drop spontaneously from an orbit of higher energy to one of lower energy. The energy liberated will appear as radiation of a definite frequency. Collisions or radiation directed against the atom make it absorb energy, and its electron *or electrons* are jerked up to an orbit of higher energy. (These are qualitative, not quantitative, conceptions. The success of Bohr's *ideas* was, of course, dependent on the quantitative specifications added to the qualitative conceptions.)

The expression "or electrons" is italicized because it indicates a generalization beyond the realm of the hydrogen atom. The ideas "behind" the professionally formulated theory of the hydrogen atom are such that generalization is psychologically inevitable. In the mind of Bohr, they were probably never narrowly associated with just the hydrogen atom. Such sentences as "the theory was only moderately successful applied to heavier atoms" attest to the broader interpretations: strictly speaking, the theory, as it was professionally formulated, could not possibly, as a pure hydrogen theory, be applied successfully or unsuccessfully to heavier atoms. Among the essential parts of the theory were specific propositions about the *hydrogen* atoms, and to its one and only electron. It would not occur to anyone to simply "apply" these propositions to other atoms. (But they certainly could be applied to ions—for example, the helium atom robbed of one of its electrons.)

Contemplating the high speed of electrons, Sommerfeld theorized in 1915 that relativistic, not classical, mechanics should be used in calculating the orbits. The hydrogen spectra were therefore investigated more accurately to detect deviations. "The results appeared to corroborate Sommerfeld's theory; and at the time, these corroborations were viewed as lending powerful support to Bohr's theory . . ." (D'Abro 1951: 514).

This is instructive because obviously the new spectroscopic findings represented a massive series of disconfirming instances if Bohr's theory was identified with a *definite* version, or even with a definite exposition of a version. That is, the theory should properly have been regarded as "conclusively falsified" if conceived narrowly as identical with any one of the definite professional versions. But in the minds of physicists, the theory was never conceived without its idea or ideas. And the fruitfulness of the ideas was only enhanced by the *modifications of the professional version* necessitated by the consideration of the enormous speed of electrons. In the above quotation there is reference to Sommerfeld's "theory," but in the

history of physics it is plainly a new version of Bohr's theory, in spite of the logical incompatibility of Sommerfeld's formulae with those of Bohr.

After the modification was carried through, deeply affecting the cognitive content of the professional versions of the theory, the new formulations were, of course, still considered to express the old theory, "Bohr's theory of the hydrogen atom." There is a symmetry to be seen here between thing identity and theory identity. As a thinglike entity, the theory is modifiable, has indefinitely many aspects, and is subjected to Leibniz's principle of the inexhaustibility of actuals. But it is the physiology, not the anatomy—the dynamics, not the statics—of the theory that reveals its thing character.

The series of modifications are an open-ended series, as are the forms of experimental setup, which are essentially bound to the theory. New discoveries, by definition nonanticipatable, add new dimensions of possible modifications. Who could anticipate in 1913 the ideas of Sommerfeld, and who could anticipate in 1915 the later idea of the spinning electron? The latter idea introduced what could still be viewed as a modification of "the" Bohr theory, but of course the modifications were now on a scale that made it natural to start talking about an abandonment of "the" Bohr theory in favor of new ones.

Sometimes a name is retained even after the most drastic of changes and expansions. "The quantum theory" was used originally by Max Planck to designate the idea that the emission and the absorption processes in atoms occur discontinuously. In the professional expression of his theoretical idea he specified the quanta as quanta of action. They each have a definite value: $h \approx 6.6 \times 10^{-27}$ erg sec. The professional expression of the theoretical idea was a hypothesis, the quantum hypothesis, in our terminology. It is quite common to look at present-day quantum theory as only a modification and extension of the old "theory"—perhaps similar to the relation between New York City today and New York City in 1800. Example: "The quantum theory has been extended and modified repeatedly since its original formulation by Planck, and even today it is still in a state of rapid change" (D'Abro 1951: 48). Planck himself did not quite recognize or accept the "developments," but he could retain the name "the quantum theory" for what he had himself put forth in 1900.

Our conclusion is only this: if we wish to employ a concept of "theory" that does even minimal justice to the use of the term in present-day

scientific literature, a theory must have a thing character, which both permits different versions of *the same* theory and includes its *idea* as part of the total phenomenon, “the” theory.

If the theoretical ideas behind two theories are clearly different, it is unlikely that the one can embrace the other as a special case, or that the one is “more comprehensive than” the other, taking this relation to be an explicit relation between professionally adequate versions. *Prima facie* there is evidence that those people are right who insist that classical mechanics cannot be shown to be a special case within relativistic mechanics, or quantum physics to be more comprehensive than classical.² One would expect, instead, a relation of incomparability due to basic differences of conceptual structure.

A theory must be capable of being presented by a finite series of fairly precise sentences. But this does not mean that there must be one definite authoritative formulation, “the” (formulation of the) theory. More important, the set of adequate formulations cannot be expected to be logically equivalent—to have identical cognitive content. There must be some similarities, but the extent of the similarity is difficult to quantify.

So far, there have not been two independent researchers who have attributed exactly the same content to Newton’s mechanics. The more important varieties are (logically considered) mutually inconsistent, if not incomparable as regards consistency (due to ambiguities). This presumably holds for the presentations of the Bernoullis, d’Alembert, Lagrange, Hamilton, Jacobi, and Hertz.³

The shortest argument for the indefinite plurality of versions of Newton’s dynamics is perhaps best illustrated by referring to the multiplicity of interpretations of a small part—namely, the laws of motion. Generating differences at this fundamental level means generating different systems of dynamics—systems with different cognitive content.⁴

The formidable complexity of details in Copernicus’s own version of the Copernican theory was not assumed by other versions. “The” theory was always in flux. If by “the Copernican theory” we mean *exactly* the original version, very few ever read or understood it, and it was never believed in, not even by its author. If Newton’s own text of *Philosophiae naturalis principia mathematica* were to be taken as *the* expression of certain Newton theories, they would be eternally infested with dozens of plain errors, mathematical and otherwise.

Referring to the situation of Galileo when defending the heliocentric system, Feyerabend argues:

The advice to *test* his theories would have been quite useless for Galileo, who at any rate was faced by an embarrassing amount of *prima facie* refuting instances, who was unable to *explain* them, for he lacked the necessary knowledge (though not at all the necessary intuitions) and who had therefore to *explain them away* in order to save a valuable hypothesis from premature extinction. (Feyerabend 1970b: 303)

As reported by Feyerabend, the conflict between theory and observation seems more direct than it would seem when taking the theoretical idea as a genuine part of the theory. Galileo could always move from professional versions of the idea to its qualitative, intuitive core: that the planets move around the sun rather than around the earth. If a theory is completely identified with definite professional versions, as sometimes *seems* to be the case in the accounts by Feyerabend and others, the survival of new theories appears near miraculous and their defenders as heroic and desperate as Vikings fighting on a ship that is sinking or already nearly submerged. I stress “seems” because custom permits us to talk *as if* a theory could be identified with its favorite version, or some of its favorite versions. This custom is reasonable in many ways, but slurs over the not inconsiderable question of how to pick out the approximate boundaries of a theory in relation to a field of ever-growing modifications.

Eddington humorously proposed that since there was so much trouble with entities partly having and partly not having wave or particle properties, a new entity, the wavicle, should be introduced, having just the right combination of both kinds of properties. Why not accept this generous offer? Because whereas the concepts of wave and particle constitute important, very simple commonsense ideas, the wavicle would have to remain a feature of the professional and precise *expositions*. Therefore there are wave theories and particle theories but no wavicle theories! The latter would lack an important ingredient: the theoretical idea.

Among the factors responsible for the semantical diffuseness or vagueness of what a name of a theory expresses, some are very simple, such as the fluid condition of what is taken to be the borderline between the theory as such and a consequence of the theory. When the chain of derivations between fundamentals, or principles, of the theory and partic-

ular laws or hypotheses derived from it is a long one, it is more or less arbitrary which link is taken to be the last of the theory itself and which to be the first within the realm of its consequences.

Further, there are versions with different degrees of generality: "Is it a theory of the hydrogen atom or of hydrogen-like atoms or ions?" There are, of course, no standard answers to what constitutes a modification of an old theory and what would be a new theory. How extensively may an Italian Renaissance chair be repaired and still be a Renaissance chair?

From the point of view of formal logic, the slightest change in cognitive content must be said to give rise to a new "theory." And from the same point of view, there may be different expositions but not different versions of one and the same theory.

From the point of view of a historian specializing in long trends, one may speak of "the" wave theory of light as if centuries of theories with different contents are all modifications of one "basic" theory, "the" wave theory. This tends, however, to result in the identification of theory with theoretical idea, a pernicious step in the philosophy of physics. Even in biology it has had awkward results, for example, in untenable assertions on the "anticipations" of Darwin's theory of selection by a host of philosophers. It was Darwin's theoretical idea, not his theory, that was anticipated.

A theory (in our terminology) must be adequately formulatable so as to make derivations practicable. An idea is normally *suggestive* of consequences, but not adapted to rigorous derivations. The formulation and expression of a theoretical idea, while not in itself constituting a theory, is nonetheless a critical prerequisite for theory development.

Behind the seemingly trivial semantical decision on what to designate by "theory" there are nonterminological questions having to do with the "idea" of a theory. Scientists are mostly willing to put their theories into a nutshell, using terms from everyday life and often some pictorial or otherwise metaphorical presentations: "light is waves," "light is particles," "electricity is a fluid," "electricity is an effluvium," "the sun, not the earth, is the center of the universe." Such utterances are sometimes frowned upon professionally but are more apt to reveal important theoretical ideas than are long careful expositions. To consider them "unscientific" may easily result in a kind of castration of the imagination. The following extract from the diary of Darwin illustrates the importance, but also the simplicity of a theoretical idea:

THEORY AND THEORETICAL IDEA

In October 1838 I happened to read for amusement Malthus on Population, and being well prepared to appreciate the struggle for existence which everywhere goes on from long continued observation of the habits of animals and plants, it at once struck me that under these circumstances favourable variations would tend to be preserved, and unfavourable ones to be destroyed. The result of this would be the formation of new species. Here then I had a theory on which to work. (Darwin 1986)

Or in my terminology: “Here then I had a *theoretical idea* by which to work.” And he certainly worked! In contrast to philosophers of previous generations, he worked out a theory and applied it with immense success.

Concluding, then, I propose a separation of the following entities:

1. *A theoretical idea*—vague, suggestive, preferably from the realm of everyday life, intuitive.
2. *Formulations expressing the theoretical idea*. These are vague, ambiguous, “crude,” but superbly fitted for being made more precise, more specific in various directions.⁵
3. *Exposition of a theory*. A definite string of sentences, in professional style, using technical terminology.
4. *Version of a theory*. Several expositions may be logically, mathematically equivalent. They all express one version, one professional *explication or development* of the theoretical idea.
5. *Inequivalent expositions expressing different versions of a theory*. Usual designations: “modifications,” “developments,” “refinements,” “forms.”
6. *The theory itself*—the idea *and* the class of versions.

There is not much to be gained, of course, by adopting this terminology, but it may occasionally clarify issues, especially the relations between logic of science and “science of science”—the many-sided study of the scientific enterprise.

Theory construction and application are part of this enterprise. To understand under what circumstances theories tend to be adopted or rejected requires studying the many aspects of scientific activity—including the climate at the frontier, policies, and research programs.

Some of these points will be elaborated below.

Value and Function of Indefiniteness and Unsurveyability

The understandable longing among social scientists for a somewhat higher degree of clarity and preciseness sometimes drives them toward persuasive definitions of “theory” that we should be happy not to see followed. For example:

A theory is a set of propositions complying, ideally, with the following conditions: (1) the propositions must be couched in terms of exactly defined concepts; (2) they must be consistent with one another; (3) they must be such that from them the existing generalizations could be deductively derived; and (4) they must be fruitful—show the way to further observations and generalizations that increase the scope of knowledge (Timasheff 1957: 9).

Brilliant formulations of historically important theories and of the intricate structure of derivations of laws and hypotheses are, of course, the highly valued rewards of lifelong study by philosophers of science, but their results may well create the impression that theories are to be *identified* with hypothetico-deductive systems. Here Michael Polanyi’s warning, well transcribed by Raymond Aron, is apposite:

Only the method of inquiry, of research or of demonstration in fact practised by the creative scientist can reveal the nature of scientific knowledge: its subsequent arrangement in hypothetico-deductive form, whatever its utility, disguises the intention of the knower and consequently the essence of his knowledge. (Aron 1961: 104)

The successful arrangement within a hypothetico-deductive framework is a great and positive achievement, but the essence of knowledge in the sense of the research situation “at the research frontier”—the status of competing theories, of relevant hypotheses and masses of observational journals—is not revealed by a lucid hypothetico-deductive system.

Considering the misunderstanding that when a precise version has been created, the vague but suggestive formulations of the idea are obsolete, archaic, superseded, and therefore scientifically objectionable, it is understandable that scientists sometimes stress the imprecise and undefinable character of important and fruitful “concepts,” meaning what I have here called “theoretical” ideas. Thus, Hendrik Kramers:

THEORY AND THEORETICAL IDEA

My own pet notion is that in the world of human thought generally, and in physical science particularly, the most important and most fruitful concepts are those to which it is impossible to attach a well-defined meaning.⁶

In our terminology: no precise version of a theory exhibits all the potentialities of its idea.

To a theory there belongs an idea. But this does not mean that the *conjunction* of the formulation of the idea and a professional exposition expresses the complete theory. The essential and important vagueness and ambiguity of the formulation of the idea make possible different *directions* in which it can be made precise and testable. The conjunction of the idea-formulation and *one* of the expositions would serve no purpose, and the conjunction of the idea and several elaborations would be inconsistent.

Simple expressions of an idea function as points of departure for strings of sentences with increasing levels of preciseness. Each such string of sentences represents an elaboration in a particular direction.

The importance and also the limitations of "point-of-departure formulations" are clearly seen in the case of the three sentences: "the universe has about the same properties all over except for local irregularities," "the night is dark," and "stars are very bright." From plausible assumptions we can derive the claim that it "ought to be" very bright at night—even infinitely bright. Why is this not so? One suggestion is that it is due to loss in energy of electromagnetic waves originating from very distant nebulae—the Doppler effect.⁷ As soon as we depart from the everyday formulations and survey precisizations in terms of theoretical concepts, the multiplicity of frameworks makes comparison of theories problematic. The observational statements are, just as all others, reformulated into technical jargon. When theories are still considered to be competing theories, theories "about the same subject matter," and their observational relevance and the status of their agreement with observation are assessed comparatively, it is because the connection with such crude formulations as "the night is dark," etc. is maintained. Only in this way can there be a common ground and therefore room for agreement and disagreement.

The indefiniteness of content of any interesting theory (due to a multiplicity of versions) makes it impracticable, once and for all, to delimit the class of its consequences or to give any definite rule such that derivable propositions can be separated from nonderivable ones. This immediately rules out the possibility of *surveying* the potentialities of severe tests, falsi-

fication, refutation, and all the other interesting properties. Sampling, not surveying, is a basic need in research.

The usual kind of statement, "Hypothesis H is (strictly) derivable from theory T ," is substantiated by giving an exposition of (part of) the theory T . If no definite exposition is explicitly or implicitly mentioned, the derivability claim is gratuitous. There is a maximum claim of derivability, "Whatever the exposition and version, H is derivable," and a minimum claim, "There is at least one exposition of at least one version such that H is derivable." Considering the diffuse "borders" of the class of versions, the latter claim is rather indefinite. A derivation counts as strict when suitable additions to the premises taken from the theory itself are known or, rather, supposed to be known.

Historically, defenders of competing theories do not agree over what is derivable. An instructive example is the systematic and persistent disagreement between the camps of E. C. Tolman and C. L. Hull during the 1930s over the consequences of each other's theories of learning. However, it might be argued—with some but not, in my opinion, sufficient justice—that Tolman only had a theoretical idea not a theory, and—with less justice—that Hull lacked a theoretical idea, and therefore also lacked a complete psychological theory. In the case of Lavoisier and the defenders of the phlogiston theories, the old generation did not catch up with the young—they did not have time, energy, and talent enough to answer the criticisms of the Lavoisier crowd. Thus, it came to be said that this and that "cannot be accounted for" by phlogiston, that certain experiments contradicted the theory, etc. This was quite convincing, as criticism is apt to be when derivations from a theory are made not by its users and defenders, but by its fiercest critics. Retrospectively, one must admit, however, that a revival of the phlogiston theory after 1785 would have necessitated the relinquishing of what is arguably the theoretical idea itself, retaining only those basic assumptions that did not imply that something is added to an oxide in processes by which metals are obtained from oxides.

"The supreme value of a new theory is its power to predict new empirical laws" (Carnap 1966: 231).⁸ If we stress the second, and not the first, occurrence of the term *new*—and I think we are justified in doing so—the evaluation of a theory cannot be made once and for all: the derivation of a new law is a *discovery*, with all the unpredictable and astonishing properties of discoveries. Further, successful derivations of new, potential laws

may occur after strong disconfirmations, because derivations often do not depend on the total theory as necessary premises. The weak parts, those responsible for the disconfirmations, need not have been used. Or, even if they have, the new baby is there, whether or not the mother is delinquent.

A theory may revive, blossom, and outstrip another long after it has been declared dead. There is no way of excluding the possibility of a revival—and why should one try? To determine what is beyond the reach of a theory is beyond the reach of a metatheory. This does not imply that it is unjustifiable *to state* that this or that is beyond the reach, say, of classical physics. But care has to be taken not to overestimate in one's assessment of probable life span, evidential or logical support, or importance for research policy. What is said one day to be utterly beyond the reach of a definite theory, may, on the next day, be considered well within its reach. Such changes can be studied anywhere, in any science. A noteworthy example is the existence of what, in cosmology, is called "the red shift."

[It] was first predicted by the earlier studies in the 1920s of the consequences of Einstein's theory of gravitation applied to the universe as a whole. Its discovery was therefore viewed as the most spectacular of the triumphs of that theory, which dominated a couple of decades of cosmological thought. But it was shown very beautifully in 1932 by Milne, McCrea, and McVittre that the "expanding universe" is contained in any effort to describe the cosmological problem in gravitational terms, even using pure Newtonian theory.

(Morrison 1965: 135)

The red shift, if the above is correct, might have been predicted from Newton's theory.

This example illustrates well the inexhaustible character of the class of consequences of a theory. Thanks to the complicated character of derivation, a complete survey is impossible. The most surprising derivations may crop up at any time.

The value of indefiniteness, or the impossibility of attaching a well-defined meaning (Kramers), and vagueness, is not absolute, but is connected with the one-many relation between a theory and versions of a theory. Rigidity destroys the elasticity and rich suggestiveness on which a theory thrives. Moreover, even when crystallized into very precise versions, the class of a theory's consequences is unsurveyable and in constant change because of changes in its auxiliaries.

Value and Function of Indefiniteness and Unsurveyability

The *main* reason for the foregoing reflections concerning notions of theory is not to press any definite terminology on anyone, but to suggest that the variety of opinions marketed as opinions on *theories* are maximally informative only when we take into account the variety of usages of “theory.”

IV

The Unimpressiveness of Impossibilities

Possibility of the Impossible: “Anything Is Possible”

The life of a theory may be long and dramatic: we may speak of its conception, fetal existence, birth, and infantile development; of triumph and defeat; of alternating phases of rejection and acceptance, domination and humiliation, stagnation and senility; and of astonishing rejuvenation and revival. Dark periods of neglect may alternate with stages of excessive popularity. (Seen in this perspective, it is not astonishing that the history of scientific theories now entices so many brilliant students of history!)

Among the well-known revivals is that of Ludwig Boltzmann’s theory of the atom. Boltzmann was regarded as the last pillar of defense of a lost cause, the atomistic-kinetic theory.

Very often scientific progress has been made by a scientist who has revived a theory against all odds, against all plausible and reasonable arguments, in the face of utterly conclusive evidence as judged by legitimate scientific standards.

This is the background of the excellent requirement of Edwin Boring that originality not be measured by absolute newness but by newness in relation to what is generally accepted in the scientific community. Revivals may thus call for more originality than constructing a new theory along the direction of what is already accepted as the most promising one.

There is a fearful contrast between the realistic picture of the exuberant, unpredictable, awe-inspiring life of scientific theorizing and that of a considerable section of the scientific community and the “unenlightened” public. There is a picture reminiscent of the slogan “Grün ist das Leben, grau ist alle Theorie.” There is a tendency to confuse rationality and

understandability with certainty and predictability. "Something *must* be considered impossible, we must know for certain that certain things not only will *never* happen but *cannot* happen." Science is used by the cultured to lift a warning finger to those who let their imagination play with mere possibilities or with what is generally considered to be implausible. But this is a striving for security and results in sleepy conformism. If it is typical of "realism in life," we are tempted to say "Grün ist die Theorie, grau ist das Leben."

Today there is surely no less dogmatism in our scientific communities than at any other time. Or, let us rather pose a question: Who is in a position to judge whether there is more or less dogmatism today?¹ Guessing that there is still some, I feel justified in offering the following slogan: "Whatever the proofs put forth for the impossibility of a kind of happening or process or thing, do not always reject an invitation to inspect an argument for its existence: It *may* exist. *Anything is possible!*"

The word *always* has been inserted because there are, in some fields, so many invitations to inspect ideas that time does not permit serious consideration. In calling "Anything is possible!" a slogan, I do not mean that it is *nothing* but a slogan. It may be taken as an assertion in ontology and made more precise in various directions. Some of them lead to paradoxes or to positions we need not make the effort to defend. Therefore, I find it convenient at this juncture to call "Anything is possible!" simply a slogan.

It has been argued that most scientists, most of the time, would not have worked so hard, and would therefore not have pushed research as far as they have done, unless they had been convinced about the eternal truth and validity of their assumptions, "facts," and postulates, and the unsoundness of any pattern of research different from their own. With certain reservations, this historical generalization seems tenable.

The most important qualification is the following: When a scientist, in his actual work, takes something for granted, without any reservations whatsoever, this does not imply that he holds certain assumptions, principles, or laws to be true or highly confirmed. He is at the moment not concerned with the truth or certainty of *those* propositions. Research practice requires limitation of perspective during each piece of work. This again implies a strong limitation of the philosophical definiteness of intention in application of terms like *true*, *false*, *fact*, *impossible*, *necessary*, or *certain*.²

The above *possibilistic slogan* is not so much directed at the individual scientist engulfed in his extremely limited research program as at the "consumers of science," especially the philosophers and the "educated public," who take their cues from science and are attracted by theories said to be scientific and repelled by those said to be unscientific or not up to date.

To take something for granted—absolutely, and beyond any question—differs vastly when it is done outside a research-problem situation from when it is done inside. The producer of science has a research program in mind; the consumer may have anything in mind *except* just that kind of situation.

The value of the admonition "Anything is possible!" in the face of arguments (in a research situation) for impossibility stems in part from the ambiguous character of the verdict "*x* is unshakeably established." There are so many nonscientific roots of formidable increases in the status of a theory or hypothesis (especially "laws") that it requires exceptional open-mindedness and intellectual power to review the evidence and the chains of derivation dispassionately and with methodological scepticism. The possibilistic slogan requires us to change the question "Is this impossible?" to "Under what kind of (perhaps unlikely) conditions is it likely to happen?"

In daily life and in nonphilosophical writings, there is usually only a difference of stylistic emphasis between "is not" and "cannot be." The extensive use of the latter among natural scientists may have contributed to the belief among philosophers of science that experiments can "show" impossibilities. The eminent scientist John D. Bernal (1967: 25) furnishes us with an example of the philosophically lighthearted use of "could not" and "impossible": "[T]he success of Pasteur's demonstration that spontaneous generation was experimentally impossible. Pasteur had shown that life could not originate except from germs of life already present in the air." Less misleadingly formulated: "In Pasteur's experiments, life 'originated' from germs in the air, and when the air was cleaned of germs, life did not originate. Pasteur rashly concluded: life *cannot* originate except from life."

Some philosophers of science, being great admirers of scientific achievements, are awed by "scientific" proofs of impossibilities. And of course, so-called "postulates of impotency" have had an important beneficial function in certain phases of scientific development. Notable

instances are the “impossibility of transmuting matter chemically” and the “impossibility of obtaining net mechanical energy from a medium by cooling it below the temperature of its surroundings,” and the “impossibility of mass destruction.”

But from the perspective of a historiography covering centuries, the changes in dominant opinion, from “possible” to “utterly impossible” and back, are too frequent and too complex in motivation and justification to be taken as indicative of conclusive discoveries.

In a scientific community—and especially among the young—there are normally traces of hero worship, or at least such a high estimation of certain researchers that when they express a doubt, others are led to doubt, and then to actually find concrete counterarguments. It is very largely a question of authority as to whether a pronouncement of “possible!” or “impossible!” is taken seriously.

Of greater theoretical importance, however, is the scrutiny of *what* is deemed impossible in fruitful postulates of impotency. Let us inspect the postulate “it is impossible to destroy energy.” The principle of constancy of energy has, in the course of a century, revealed its character as a rule (how to calculate) or postulate rather than an assertion. In order to maintain certain invariabilities, specific ways of structuring laws and new “energies” have been put into the equations. That is, there has not been one single theory with a definite energy-constancy theorem as a genuine part. There has been a succession of mutually inconsistent theories that have all been in accordance with a *rule* of energy constancy. Behind the “impossibility” we find a heuristic rule. The impossible is what is *ruled* out. No physical process is declared impossible by the principle of constancy of energy.

The slogan “Anything is possible!” has been related to negations of possibility derived (logically, or otherwise in case of impossibility of phenomena) from theories. But as can be expected from a good slogan, it cannot be rendered in plain informative language without loss. This was already hinted at earlier. If we do so, we get into trouble at once. In analogy with the shift of consideration from “*x* is impossible” to “‘*x* is impossible’ is (only) asserted,” there must be a shift of consideration from “*x* is possible” to “‘*x* is possible’ is (only) asserted.” From there on, we may, once more inspired by astonishing events in the history of science, feel inclined to introduce the slogan “all eventualities except one are impossible,”

stressing that there is only one unrepeatable course of events, or better, "anything may be impossible." After all, there have been completely unforeseen, conceptually confusing instances of phenomena declared to exist or at least to be possible that have later been taken to be impossible—even "proved" to be impossible. So, there is no guarantee that anything that is *defined within a definite conceptual framework* will persist in being taken to exist or even to be possible. But such an "impossibilism" has at the present time—as far as I can judge—no mission. What is lacking in many environments is the clear realization of how little weight a proof of impossibility has if our perspective covers several decades and encompasses the premises and rules of inference used in the proof.

The relation of a pronouncement of "possible!" to definite conceptual frameworks is essential because if such pronouncements are made in a conceptually uncommitted way, they acquire a function different from that within research. Thus, if in an everyday setting I seriously think it *possible* that my horse will win the derby next year running backwards, I am simply wrong. On the other hand, some strange feats of locomotion may well be declared possible in a biological discussion in which the consequences of some new theories are being considered.³

It has been remarked by David Hume and many distinguished contemporaries that a speculative man may hold sceptical doctrines in the abstract that he does not even try to follow in real life. But the converse possibility also needs remarking, that speculative minds may hold dogmatic views in the abstract that do not correspond to their more sceptical ways in real life. An example may make this clearer.

"No physicist would dream of trying to settle experimentally someone's claim to have constructed a *perpetuum mobile*." This I suppose is *wrong*, but wrong as sociology of knowledge and as psychology. That absolutely no such claim would induce a physicist to inspect the construction of a *perpetuum mobile* shows a *theoretical* inclination toward just the same lack of research-mindedness as was shown by those who once refused an invitation to settle experimentally whether there could be movement in heaven or not. But experience shows that at least some, if not the majority, of physicists *are* sometimes willing to join forces in experimentation based on a negation of generally accepted "impossibility." They can be persuaded to do this in spite of the nagging question "But isn't this completely absurd?" It was once agreed that movement (in a certain sense) in

heaven was *impossible*, and how could an impossibility in principle be overruled by experience? The answer is that anyone who is free in his abstract theorizing can conceive an “impossibility in principle” from the outside—as an *asserted* impossibility, a claim, and therefore perhaps merely an error of judgment. With even a little stretch of the imagination, definite sources of error can be conceived, and with still less stretching, definite *kinds* of sources. The situation is then seen in a new perspective. Change of perspective saves us from many dogmas.

One might object that a case of *perpetuum mobile* cannot be settled *purely* experimentally: there is a successful theory from which the impossibility follows. A rival theory would have first to be created from which either the impossibility does not *follow*, or from which the possibility follows (that is, by being clearly compatible). This objection, however, has in part to do with credibility or acceptability as part of a working program. Together with a (seemingly competent) theory at hand, it still makes sense to inspect an experiment that is said to “show” something impossible (according to the only well-elaborated theory available).

But granted that one might be willing to inspect such an experiment rationally without having been confronted with a theory, the objection could still be made that as the *perpetuum mobile* would have to go on forever, no human experiment could convince a man who believes in mortality that the matter could be *settled*. This, however, would be a clear case of interpreting the settling of “empirical” research questions in the sense of definitive verification. The conditions required for justifying the verdict of “settled!” are not of this kind. They contain a pragmatic component and involve a *decision* to stop further inquiry when prospects for obtaining evidence that could unsettle a tentative, ad hoc conclusion are dim.

Today we shake our heads. How could people believe in the conclusiveness of the proofs of immobility in the heavens! We laugh at their expense. And we add, perhaps, that of course *any proof of impossibility starts from premises and uses rules at least one of which is nonevident*. What is easily forgotten, however, is that this mood of openness is apt to vanish as soon as the historical perspective is replaced by the systematical and contemporary.

The term *physical necessity* is often used in regard to well-established physical laws. Any process inconsistent with what is well established is accordingly called physically impossible. The necessity

talked about here is not logical necessity, but deducibility from a dominant theory. However persuasive the theory may be, human creative imagination conceives other possibilities, and sometimes they materialize. That is, after some time a new theory, inconsistent with the old, well-established one, becomes in its turn well established. The old "laws of nature" are reformulated in the new jargon, or sometimes given up. Thus, only by equivocation are so-called physical impossibilities taken to be permanent obstacles. If a creative chemist—to take an interesting example, Linus Pauling—publishes a statement that flatly contradicts extremely well-established laws, the possibility of discarding or modifying those laws is seriously contemplated. I refer to the account by J. D. Watson, who competed with Pauling in constructing a model of DNA. A manuscript by the latter, outlining a solution, fell into the hands of the former, but it contained some highly "unorthodox" chemistry. Says Watson (Watson 1968: 161), "We could not but initially worry whether Linus' model followed from a revolutionary re-evaluation of the acid-base properties of very large molecules. The tone of the manuscript, however, argued against any such advance in chemical theory." (Watson's ultimate conclusion: Pauling had committed a mistake that would have stamped a student as "unfit to benefit from Cal Tech's chemistry faculty.") Either revolutionary advance or crass blunder! These are the alternatives often considered. Of "impossibility," little is said in actual research.

According to Popper (1961: 430), "there may be structurally different worlds—worlds with different natural laws." His view is fully consistent with "possibilism": "Natural necessity or impossibility"—a somewhat wider concept than physical necessity or impossibility—"is like musical necessity or impossibility," imposing "structural principles." It is like the impossibility "of a four-beat rhythm in a classical minuet, or the impossibility of ending it on a diminished seventh or some other dissonance." The main point in my argumentation is that it is still, and is likely to remain, an open question whether "the world," if considered as a musical creation, is a classical minuet, or a minuet of any kind whatsoever. Natural necessity or impossibility imposes structural principles "upon the world" (ibid.), but leaves "a great deal of freedom to the more contingent singular facts—the initial conditions" (ibid.). But hypotheses of the kind " x is a case of natural impossibility (or necessity)" have no function in research except as ad hoc rules and do not restrict freedom in any way

whatsoever. In the terminology of Wittgenstein: science is a game in which we change the rules as we go along.

According to Norwood Hanson (Hanson 1958: 117), “perpetual motion machines and velocities greater than light are not psychologically inconceivable: they are impossible in principle.” It is because of this impossibility that no physicist, according to Hanson, would even remotely consider undertaking an experimental settlement of a claim to have constructed a *perpetuum mobile*.

Here I think a fundamental attitude toward research is involved. From certain exceptionally well-established theories in thermodynamics, one can derive, perhaps even deduce in a fairly strict sense, certain sentences that imply the impossibility of perpetual motions of certain kinds. But what follows? That a theory is ever so well established does not mean that it is established as true. Acceptance of a theory (to work with) does not imply acceptance of it as true or even probable. Therefore, if the negation of a sentence is derived by logic from the theory, this does not mean that the negation is true, or even probable. And if the negation can be interpreted to imply the impossibility of a phenomenon *P*, the sentence “*P* is impossible” does not mean the same as “it is true that *P* is impossible” but rather “it is true that *P* is impossible if the theory is true.” Since the theory is not asserted *as true*, it cannot be derived that *P* truly is impossible.

As John Austin and Ludwig Wittgenstein (and others before them) have tried to teach us, there are many ways of using language, and I take it that the use of language to formulate theories is only one, and a different use from formulating particular truths.⁴ Theories are formed as “normal” indicative sentences, but if *S* is such a sentence, there is, I am glad to say, no exclusive use such that *S* must be taken in this simple affirmative use.⁵ Saying *S* assertively is only one way of saying *S*. But even if the indicative mood were uniformly used in such a way that *S* expresses that *S* is simply true, historiography tends to make us attribute only provisional truth-values to the postulation of impossibilities derived from theories.

Let us inspect an assertion that seems to imply a limitation of possibilities (that is, increase in strength of assertion): “A law sentence expresses an a priori proposition when its user maintains it in the face of all experience” (Hanson 1958: 114). “Confrontation with *all* experience” is

an interesting formula, but how is it applied? Can it be used as a criterion? A physicist may be said to use a law sentence at times t_1, t_2, \dots, t_n . Then he leaves physics, enjoying his pension or simply dying. There is a limit to the number of occurrences our theories about *his* use of the sentence can cover. Suppose we have used modern equipment with tape recorders, television, neurological devices, and interviewers in order to get the richest possible observational basis for our theories. One hypothesis might be, "Whatever this man will experience in his life as a researcher, he will maintain this law sentence." It is clearly an interesting biographical hypothesis, and, if made more precise, only understandable in terms of a comprehensive conceptual system. The conceptions of x *maintains* y are many, and the relation of observations of x and y to the conclusion x *maintains* y is a very complicated one. There are also interesting differences in conceptions of experience and of "sentence." Some would say that we must be fairly clear about whether we are doing research on a formula or on the law that the scientist holds to be expressed by the formula. In any case, a metascientific hypothesis such as " x maintains y in the face of all experience" is unable to furnish any *definite* criterion of "a priori." For example, it can be interpreted as expressing obviously too weak a criterion: "A sentence S is a priori in relation to experiences of class E if and only if S is left unchanged in the face of each element of E ." This criterion is too weak because S might be a priori in relation to most small classes and a posteriori to many rich classes. And again, an obviously inapplicable criterion is obtained if the class is open and therefore potentially infinite. "A sentence S is a priori if left unchanged by users of S in the face of any experience in the past, present, and future." If, after some time, there are no more users of S , a stipulation must be made as to how to conceive the sentence.

It is a valuable feature of Hanson's account of $F = ma$ that he stresses the multiplicity of its functions or uses, and also that he would criticize any concepts of meaning that made use irrelevant to meaning, but I do not see the full realization of the methodological issues involved. If $F = ma$ has at least five main uses, a sentence such as " $F = ma$ " has at least five main uses" should not be expected to have less. Nor should we expect that two presumably competent observers and metascientific theorists would class a given instance of the use of $F = ma$ in the same category nor that they would agree on a definite classification of uses. Work done in lexi-

cography and in empirical semantics strongly suggests that there will be a multiplicity of interpretations and different, normally incomparable conceptual frames of reference. What I suggest is that the valuable insistence on plurality of uses must not be limited to *physical* research. It is just as pertinent in lexicographical and empirical semantical research. This has the consequence that argumentation in favor of pluralism is as relevant in those “soft” fields as in the “hard” field of physics.

“The logical point is that some laws *are* maintained in the face of all experience.” Considering scientific development as a process in time and the scientists as actors, the new historiography cannot admit “facts” of this kind. Experience is inexhaustible and no scientists face all experience. If the scientist says, “It is my deadly serious intention to maintain the law to the very last breath of my life,” this is of course an interesting (and astonishingly “unscientific”) statement, but not a sufficient basis for asserting the truth of the claim that the law *is* maintained in the face of all experience. Suppose the scientist adds, “You see, potentially falsifying evidence would be inconsistent with a conservation principle, and, furthermore, the idea of potentially falsifying evidence is psychologically inconceivable.” According to Hanson, he may maintain the law *because of* such considerations (= such hypotheses?). But what if, in the face of some experiences, he appeals to opposite considerations? Hanson himself reveals a potential complication:

Popes die and Commissars are liquidated, the successors may be of opposite opinion: A man might hold to a law against all counter-experience because, for instance, a Pope or a Commissar instructed him to do so. We deplore his reasons, but the logical issue remains unaffected: he uses the law sentence concerned in an *a priori* manner. (Hanson 1958: 115)

So, if the pious scientist outlives n popes who all give different instructions concerning the acceptability of propositions p, q, r, \dots does he nevertheless “use p, q, r, \dots in an *a priori* manner” at every turn of events? For our present discussion, it suffices to conclude that if the actual (factual) obedience to an instruction is made relevant for the question whether a formula expresses an *a priori* proposition or not, there will be room for many conceptions of “*a priori* proposition” and its relation to “use of a sentence in an *a priori* manner.” They will be dependent on contemporary conceptual frameworks in social psychology.

Investigation of the occurrences of certain terms and sentences, applying the available methods and techniques of linguists and social scientists (including historians), discloses the complicated and indirect relations between observational journals covering the occurrences on the one hand and hypotheses about use, function, meaning, import of the terms, and sentences on the other. If investigations could be carried out today as adequately as analogous investigations of occurrences of falling bodies and electric sparks, my guess is that one would find both the relation of theories to hypotheses and the relation of theories to observation to be just as indirect as in mechanics and theory of electricity. *There is no less room for pluralism in metascience than in science.* Metascience is in this, as in so many other respects, nothing more than a part of science.

The difference between acceptance and acceptance as true is apt to influence behavior. A physicist may accept the theory of relativity and accept as logically correct the derivation of sentences implying the impossibility of velocities (of matter) greater than that of light, but if there is a rumor of theories being worked out that do not deny supervelocities, or of experiments that might even show the existence of such velocities—well, wonderful! Nothing makes him more curious than the possibility of “seeing” something violate his fundamental scientific assumptions. There is joy in transcendental stupefaction! One must be prepared for what cannot be anticipated!

This point has to do with the strangeness of the relation between theory and practice. Think of a professor of physics just completing, before an audience of graduate students, the proof of the impossibility of velocities greater than light. A colleague and friend rushes in and tells him about an experimental setup just being tried out that indicates the presence of particles with a speed greater than that of light. “Incredible! Impossible! I don’t believe it!” the professor shouts, but adds *without changing his opinion on anything*: “Let me have a look! Perhaps it is *not* a hoax! The class is dismissed! Let us see what is happening at the other end of campus!” And so they rush off to expose themselves to an impetus that *may*—though it is statistically unlikely—change their attitude. (More likely, of course, the rumor of supervelocities would stem from the announcement of a *new theory* with new consequences concerning velocities.)

Any sociological or psychological model of the rationality of scientists that would rule out this open-mindedness of a physicist toward the

"impossible," is not only wrong but pernicious. "Anything may happen!" Whatever the articulations in the way of proofs, semantical, mathematical, or logical, they cannot rationally determine their own weight.

Man is capable of reflection, and of reflection about reflection. This makes it always possible for him to go from the n th to the $(n + 1)$ th level. When he has just completed a proof and accepted its conclusion as inevitable, an incident may make him ask "Does it hold?" And even if it holds, has it any bearing on any phenomena? Or, if he persists in being completely convinced, he can make his state of mind an object of research; he can move from living *in* the certainty to living in the reflection *about* the certainty, thus making way for the thought that there is a difference between the provenness of the theorem and the state of mind of being completely convinced of its provenness. This again makes a dialogue possible with one who rejects the provenness and makes it possible to work with the assumption that it is not proven. The awareness of this makes it natural to utter "anything is possible," *angelikos*, in the manner of the Pyrrhonian sceptics.

It is not only basic views of rationality (read: pedantry) that are used to oppose open-minded recognition of possibilities, but also some of irrationality: Scientists, it is said, *belong* to traditions, societies, communities, and ways of life inside which certain possibilities simply *cannot* be seen. People *must* believe in certain presuppositions essential to their tradition. But however fruitful the constructs "tradition," "society," "community," and "ways of life" may be, they should not be taken too seriously. Geyl, Sorokin, Popper, Gellner, and others have successfully opposed the social determinism of Spengler and contemporary philosophers. Within certain sections of the populace, definite views may *dominate* for shorter or longer periods of time, but where one has had the opportunity to make detailed studies, instances of contrary views are found, and no psychological or social determinism has been able to show that those following the mainstream of opinion are completely impregnable to views that go against the dominant ones.

In short, the acceptance at time t_1 of this or that as impossible does not exclude rational openness toward an argumentation for the existence of the so-called impossible at some future time, t_n .

The acceptance of the possibilist slogan may often have little influence on behavior, or more generally, on *praxis*. Comparing the pairs of con-

clusions: "x is necessary" / "x is extremely likely to be the case" and "x is extremely unlikely to be the case" / "x is impossible," there are no consistent differences of weight within the pairs when it comes to questions of research policy. Accepting the slogan means primarily that, under special circumstances, one *uses* as an assumption in actual research, *works out* consequences of, *plans* tests for, and *presumes* the existence of what is generally taken to be nonexistent and incapable of existence.

"But *ultimately* don't we strive as researchers for *knowledge*?" Why this high evaluation of knowledge? There is no clear argumentation that brings us from statements like "according to the now generally accepted theories, x is the case" to "it is known that x." On the whole the stress on knowledge has degenerated into a stress on certainty, permanence, dogmatism, and pedantry.⁶ That research is ultimately motivated by a need for certainty is a dogma that has recently been undermined successfully (for example, by James, Popper, Polanyi, and Feyerabend). This cannot but affect the status of knowledge as a kind of resting place, which is contrary to the whole idea of unending, expanding research.

Research may be motivated by a need to systematically explore intensely exciting and illuminating possibilities whatever the chances of finding evidence that will eliminate the speculative character of the hypotheses. Let us say a researcher works with propositions with four very different levels of "certainty" or of "plausibility." There is nothing in research methodology or a "psychology and logic of research" that indicates that working with only the highest level must be the most satisfactory from some or all points of view.

An example: From time to time rumors reached the Copenhagen circle (Bohr and associates) in the 1920s that a really devastating argument had been found somewhere against quantum mechanics. Bohr and his many outstandingly talented collaborators would then hastily get together and in an atmosphere of almost diabolical fervor try out the strongest possible versions of the argument suggested by the rumor. There was the kind of rush to refute their own theory that would have delighted, perhaps even astonished, Popper.

There was not the slightest doubt, however, that if the rumor were true, and some really fundamental, but until now *completely unsuspected*, weakness were found, this would only temporarily halt the progress of physics. There was also probably little doubt that they themselves, the very creators of quantum mechanics, would be able to participate in new

theory construction; there was probably a pervasive belief that *they* would *be able* to reconsider *everything*. In short, the intensive work that went on on the basis of an acceptance of quantum mechanics did not undermine the disposition to listen to and to study any qualified suggestion implying the rejection of the most basic assumptions underlying their work. The most impossible might turn out to be possible, and the unimaginable imaginable. The open recognition of this was made easy by the well-founded self-respect and the realistic assessment of the capacities of the group's members. Where there is a feeling of impotency, such openness is, of course, more difficult. But in no case is it necessary to accept an ideology of the finality of results.

A study of history suggests that, statistically, this openness is exceptional, but the statistics of attitudes is not the only relevant discipline in assessing what acceptance of a theory means to a researcher in times of the maximum employment of his talents.

Feyerabend stresses that the elaboration of a new cosmology requires a step back from the available evidence—the new cosmology may well start with zero or negative *observational* support. One must therefore not ask of an idea—the nucleus of the emerging new cosmology—that it show its credentials *before* being elaborated in various directions. When an established “impossibility” is rejected, it may still be much too early to ask for *how* it is possible. A theory may tentatively be accepted that makes room for a *perpetuum mobile* without any idea of *what* a *perpetuum mobile* would look like, or *how* it would be possible.

So-Called Completeness and Maturity as Signs of Abandonment

Philosophers who argued against Einstein in the 1920s used a kind of “ordinary imagination” argument to block efforts to overcome the limitations of one's theoretical imagination, treating it as a static factor and not a capacity like that of calculation, which can, and sometimes must, be developed far beyond its temporary limits. There are no *preliminary* checks to an idea; ideas in research are themselves preliminary. Therefore the competition among ideas is always completely open. If a scientist is capable of elaborating an obscure but simple idea in such a way as to elevate it to a scientific level of preciseness and testability, so much the better—the

more independent ideas there are, the more starting points for renewal and decisive progress. The bottleneck is, of course, our limited capacities and eagerness for concentrated tenacious effort.

"Any idea can become plausible and receive partial support," according to Feyerabend. When an idea becomes "the starting point of concentrated effort," it is connected with preexisting observational journals and expressions of natural laws. *Using* the idea includes reformulating the law formulations and interpreting the observation sentences in terms of the idea. Obviously, these reformulations can hardly avoid giving the idea substantial evidential support. It is a birthday gift to any clever idea. But only future elaboration will show whether the effort of restructuring is worthwhile. The "good" idea flourishes, the "bad" one is given up, perhaps only after considerable efforts that, unfortunately, are left unmentioned in the historical accounts of scientific development. More should be done by editors of journals to elicit reports on laborious research that, despite adequate equipment, has been unsuccessful.

Considering the chances of a theory's revival, one must bear in mind that its idea is an already "elaborate" one, and that the competing definite precise formulations along the axes or directions of precization are different in content and that the conjunction of the professional versions is therefore contradictory and, of course, not expressive of the idea "itself." Suppose we were today to revive "the" phlogiston theory, a most "unthinkable" and thankless job. If we took the professional versions from 1785 and later years as a starting point, we would have to make more and stranger ad hoc assumptions than if we took up the theory at an earlier stage, before it had been elaborated in detail. Revivals are revivals of ideas, and fragments of ideas, not of definite hypotheses expressed in an "ossified" technical jargon.

Feyerabend's statement that "any idea can become plausible" is closely akin to our more general "Anything is possible!" It has an important relation to our wide conception of auxiliary propositions:

Not being prophets we must also admit that *any* inadequate view, however often refuted and however implausible, is capable of becoming the crystallization point of a new world picture which, taken in conjunction with a new (and perhaps utopian) theory of knowledge, might in the end prove more effective than the most attractive and most highly corroborated element of the *status quo* (no method can guarantee an approach to "truth").

(Feyerabend 1970a)

A good slogan can be overworked, and this holds true for “Anything is possible!” Suppose we agree that an event that is impossible according to generally accepted contemporary physical theory is nevertheless possible, or even probable or necessary, according to theories that negate some of the untested or incompletely tested assumptions of these theories. And suppose further that we take it as likely that within the time interval of a few generations, such theories, or theories more widely different from our present ones, will be generally accepted. This perspective is rarely, if ever, relevant in the research work of the vast majority of theorists today. As already mentioned, there is a bottleneck: our limited capacities. A super-human intelligence might well point to certain data, laws, or already known events that foreshadow the next major change in physical theory, but what relevance has this for us? Ernst Mach’s lonely opposition to what was generally accepted could be used by Einstein to create something revolutionarily new, and Einstein himself thought that Mach might have come up with the theory of general relativity if external circumstances had been somewhat different. But where are the Einsteins today, and how can they know they are Einsteins? Questions and considerations such as these strongly suggest that if a researcher is tempted to work from premises, which, in part, negate generally accepted physical theories, the slogan “Anything is possible!” should *not* be used to cheer him up: it is *unlikely* that he will get anywhere. Statistically, the chances of eliciting a new “revolution” are small, and the users of the possibilistic slogan ought not to take upon themselves the responsibility of backing up unrealistic revolutionaries just for the fun of it.

Even if we discard certain crude ideas about scientific development as the accumulation of facts, or tested approximations to truth, we may acknowledge an accumulation of documents and other artifacts of scientific activity. This implies a steadily more comprehensive job of obtaining coherent pictures, and noncontradictory, content-rich theories covering the various loosely connected “($n + \chi$)-dimensional” fields of research. The n stands for uncontroversial dimensions acknowledged in the research communities at a definite time, the χ stands for the new dimensions cropping up in contemporary research not yet digested by, or not yet discussed extensively in, the communities.

In rejecting the image of broad theories being abandoned by simple falsification, or new ones being adopted by more complete verification or

confirmation, we are on the verge of adopting a coherence view of scientific “truth” or “acceptability.” The theory that can “account” for more than any other available theory is to be adopted, “accounting for” meaning “bringing into coherent, at least noncontradictory, intelligible connection.”

But even to require no known contradictions as a necessary condition for acceptability is too strong a requirement in view of the dynamics of theory change. For new theories under construction may, of course, contain contradictions—that is, internal inconsistencies—and nevertheless be perfectly *acceptable*—that is, more worthwhile to work with than any other available candidate. Internal contradictions falsify but do not make unacceptable. Contradictions may be present at any level and between any levels—within the basic formulations (the principles), between the basic formulations and some laws or hypotheses considered to be derivable, between hypotheses, and between hypotheses and observation. Further, there may be inconsistency between the auxiliary assumptions and the science from which they are borrowed. Physicists have often worked with clearly inadequate, sometimes squarely false or absurd, mathematical theorems; assumptions concerning observability have been in conflict with psychophysical experimental evidence and so forth. In general, the worker in one science tends to use obsolete data and hypotheses from other fields and often acknowledges his inability to keep up to date. He *should* be up to date, but he often does well enough without being so.

Conclusion: Even if “coherence” is taken as an important *desideratum*, absence of it does not imply unacceptability of a new or an old theory. “Acceptable” in research is not synonymous with “acceptance as true.”

As regards theories that are no longer under construction—*mature theories, completed theories*—they do not “live” under the most favorable research conditions. The belief in completeness reveals loss of interest in expansion and development, both of idea and of professional versions, or reduction of the use of the theory to mere applications. Thus, the “abnormal” character of theory change is due to the perhaps inescapable situation that most theories, most of the time, are applied, not *accepted* in the sense of “accepted as a set of working hypotheses in efforts at expansion.”

The question of definitive completeness raises the recently discussed question of “permanent revolution in science.” Let us make some distinctions: A change from theory *A* to theory *B* may be called revolutionary

only if there is a conceptual incomparability between A and B —due to profound changes in conceptual framework or practice. As long as A can be translated into B , and vice versa, there is a continuity that one might consider evolutionary, not revolutionary.

Using this concept of revolution, it is difficult to see on what grounds any revolution should be considered to be a final revolution, however long human beings occupy themselves with science. The main obstacle to finality might be revolutions in society at large. These are likely to affect the practice of science in subtle ways that are resistant to articulation (even if in principle expressible). With sufficient changes, old theories will cease to be understood or be understood in ways too different to make it reasonable to maintain that the old theories are still accepted.

The supposed infinite complexity of nature (and man) does not itself preclude a halt to scientific revolutions. Here I agree with William Kneale (1968: 38). But as regards the possibility of a future, conclusive, complete, and comprehensive theory, I do not see what could reasonably be meant by that. Or, for that matter, by completeness of a science, even if defined soberly as follows: “A science is complete when it gives as much descriptive detail as is desired for the domain of the science and when the theoretical structure of the science satisfactorily explains all the facts of the science” (Schlegel 1967: 46). If, in a new science, there are only two facts and only one researcher with weak desires, completeness seems easily reached! Desires are fragile criteria.

A theory might be termed complete in terms of a definite frame of reference and definite aims of inquiry. Thus, there is a “complete” theory of crystals in the sense that there is a generally accepted theory of all the “possible” 230 forms of crystals. But frames change, as do judgments about possibilities, and there are indefinitely many properties of crystals, not all of them covered by any theory. And it is a wide step in generalization to move from a kind of temporal, aim-related, and therefore highly relative assessment of the completeness of a theory to that of a whole science. I see no point in introducing concepts to characterize the completeness of a science.

The Inexhaustiveness of Ideas: A Semantical Model

The relation between a vague and ambiguous but suggestive theoretical idea and a precise exposition may be schematically presented semantically as a movement from a vague and ambiguous but suggestive *formulation*

("light is waves," "electricity is a fluid") to less and less vague and ambiguous formulations along a definite direction, a direction of increased preciseness (a direction of precization) or greater specification. A resultant fairly precise and specific "version" of the theory is tested in various ways, including observation, but only as a stage in a larger movement. Whatever the success of a particular version, the scientist returns to the vague initial formulation ("the T_0 -formulation"), develops new hunches, and proceeds along a new direction of increased preciseness or of greater specification. If T_{11} and T_{12} are formulations at the systematic second step of "making more precise," they are (by definition) heteronymous, clearly separated in cognitive content (see p. 129). The conjunction of two sentences that are more precise than a given one, say T_{11} and T_{12} in relation to T_0 , may or may not be logically inconsistent with each other, whereas conjunctions of competing versions of a theory will normally be mutually inconsistent.

If, over the course of time, various precise formulations have been accepted and then later rejected, and a new theory begins to gain a foothold, there is sometimes no energy left to consider another movement from the original idea. This situation is well expressed by saying that "the theory is dead." But unlike humans, theories do not decompose after death; revival is easier for them than for us. After having been dead for a very long time (in scientific communities), Goethe's theory of color has experienced a strange revival, but (of course) not as a theory competing with that of Newton (Holtsmark 1969). Cognitive competition requires cognitive comparability.

Possibilism and Permissiveness: Crazy Ideas and Connectability

In a notorious article in *Nature*, it was "shown" that the time of gestation of animals as different as cows and rabbits is a multiple of the number π . "Scientists have rightly refused to pay any attention to this evidence," says Polanyi (Crombie 1963: 376), and he adds, *inter alia*, that "if scientists did not suppress the offerings of cranks, science would be swamped by trash." I would rather say: if scientists and philosophers did not brush aside manuscripts that on the basis of very superficial reading seem cranky, their time would be totally consumed reading such papers.

Does the possibilistic slogan applied to theoretical impossibilities tend to encourage excessive tolerance in dealing with implausible, crazy, or irreparably vague conjectures? I do not think so.⁷

Let us consider one of the many factors determining which manuscript (among the formidable cascades of paper) to read carefully. Suppose I am asked to read a manuscript and start reading sentence p on page 20. It seems utterly crazy to me, but why? Because I think that p implies q , and q is obviously false. On page 21, I see that the author offers a very good reason for the view that, strictly speaking, p does not imply q but, rather, r . This interesting little triumph of the author makes me willing to examine reasons he may have for accepting r , a rather unlikely proposition in my opinion. If the author in a precise and convincing way supports r , the initial craziness of p has now abated a little. It may, however, require hundreds of additional tests to prevent me from concluding that among the innumerable seemingly crazy papers, this manuscript does not deserve the very high priority it needs to warrant close inspection. In this connection I would recall the notion of "connectability," profitably used by R. von Mises and others, which is essential to most forms of conventionalism and to coherence theories of truth. If we connect pairs, triples, and greater classes of intuitively unlikely propositions, we shall rarely attribute higher degrees of likeliness to the classes of conjunctions. If confronted with a proposition that seems crazy, but is possibly true in the wide sense of "possible," we normally require knowing how it connects with a large number of other propositions. In some cases, the connectability with a large class of propositions is such that the class "retroactively" strengthens the likeliness of the "crazy idea." The idea "coheres" within a larger, consistent whole. This, for instance, is the case with certain ideas in Velikovsky's *Worlds in Collision* and his published defense of them. But some people nevertheless ask for larger areas of connectability in order to change their verdict of "crazy!" Ultimately, the tribunal is that of philosophical systems giving definite meanings to propositions in "scientific" encyclopedias. The ideas of *Worlds in Collision* are such that, even if they are coherent with the vast body of historical sources appealed to (among others, the Bible), they still have to be confronted with modern astronomy and with general methodology and epistemology, the latter because of the author's (perhaps) greater reliance on historical sources than on those of natural science.

Suppose researchers *P* and *Q* agree that *if* set *A* of rank dimensions is adopted, then theory *T* is superior to theory *U*; and *if* set *B*, then *U* is superior to *T*. We shall say they agree that the sets of propositions *A* & *T* and *B* & *U* connect, whereas *B* & *T* and *A* & *U* do not. But as metaresearchers we may ask: Are *P* and *Q* *factually* right? Is it the case, is it true, is it really so that *A* & *T* connects? The direct answer is not an answer that *A* & *T* connects (or does not connect). We shall assess the nature of the theorem "*A* & *T* connects" and, if things are complicated, the kind of research that is relevant in order to find the truth. On the basis of this investigation we shall adopt criteria of verification and falsification of the theorem. If metaresearchers *C* and *D* arrive at different sets of criteria—for example, after a rational discussion of epistemological issues—they might agree on the conclusion that if set *C* of criteria is adopted, "*A* & *T* connects & *C*' connects" is true, whereas if criteria *O* are accepted, "*A* & *T* connects & *D*' does not connect" is true. The conclusion is in terms of connectability.

But *C* and *D* may, of course, start a factual discussion about whether their conclusion is correct: do the connectabilities in fact hold? Thus at no level are there *only* questions of connectability. Questions of facts, of "agreement with how it really is" can be or must be posed at all metalevels. This argument is valid not only for the concept of connectability, but for that of coherence as well.

If a definition of truth in terms of agreement with fact is compared with a definition of truth in terms of coherence, there are strong reasons for rejecting the latter rather than the former. But if a criterion in terms of agreement with fact is compared with a coherence criterion, no definite conclusion is warranted before one knows approximately how tests of agreement with facts (a-tests) are compared with tests of coherence (c-tests).

Popper's argument in rejection of coherence (Popper 1963: 226) is weak because he does not seem to distinguish definition from criterion and coherence from logical consistency.

The important question is not whether something is possible, but whether it is worth studying and working with. Research programs are not, and should not be, easily changed. A simple declaration that something we firmly believe to be impossible may well turn out to be the case is still not a sufficient basis for committing ourselves to research that might furnish fresh evidence for or against.

Then there is the difference between “wild” *theories* and metaphysical utterances of the lowest level of (research) preciseness. By the latter I mean what Popper sometimes calls “metaphysical” theories (“The world is my idea” and “Perhaps all is a dream”). They are not covered by what I have said in the foregoing discussion of possibilities because they do not in any way satisfy the requirements of a theory set down in chapter 3, p. 52.

If a scientist invites us to *work* with an unestablished theory that explicitly assumes the possibility of something that, on the basis of a justly admired, well-established, “complete” theory, is “proved impossible,” this implies that the new theory has already been worked out in sufficient detail for it to come into clear conflict with other theories. What scientists are apt to call “the philosophies of the schools” have no chance at all of coming into conflict with a theory, either with the kinetic theory of gases or with any other theory with scientific pretensions. The very notion of working with a theoretical idea implies a movement toward preciseness and specification. The slogan “Anything is possible!” is introduced for and *intended to operate* in research situations, and nowhere else. It has a built-in reference to “work.”

Working with Many Theories, in Many Ways: Theory Proliferation and Diversity of Praxis

As regards implausibility or “craziness,” it should be noticed that the possibilistic slogan only points to or suggests possibilities. It is not an injunction to work with *any* conjecture whatsoever. In addition to a so-called proof of impossibility (based on a well-established theory or “law of nature”), there may be *decisive* arguments against working with a particular hypothesis. Normally there are innumerable possibilities in a problem situation and a decision to pick out one approach for detailed consideration presupposes (needless to say?) additional qualification. Possibility is not enough for action. A decisive argument against working with a hypothesis is rarely a decisive argument against the hypothesis considered as a proposition. Work has to do with here and now, with persons, priorities, propensities, and time schedules. Work requires *someone* to work, whereas no one is called on to do anything about an abstract—however emphatic—assessment of truth, probability, likelihood, or possibility.

Therefore a decisive argument for accepting or rejecting something as a working hypothesis must be distinguished from accepting or rejecting something in the abstract as probable or improbable, true or false, likely or unlikely.

The possibilistic slogan tends to counteract the prevailing tendency to view a decisive argument against the *adoption* of a theory at a definite moment by a definite group as a decisive argument against the theory itself. It does not tend to encourage the adoption of any definite theory. That forces a reduction in the free inspection of alternative theories. The possibilistic slogan encourages serious consideration of alternative theories in the atmosphere of pluralism. It encourages this in defiance of the many so-called impossibilities. These are perhaps no more than expressions of current dogmatism, thoughtlessness, and narrowness of perspective. "I believe that, in the pursuit of physical science, the imagination should be taught to present the subject investigated in all possible, and even in impossible views . . ." (Michael Faraday).⁸

In view of the very limited time and capacity of a definite individual or a definite team, the vigorous pluralist approach in science can only be realized in large communities of researchers.

The above does not imply any agnosticism or any "*ignorabimus*" in relation to reality. "Through science and philosophy we get to know reality." There is no good reason to reject that slogan. But we cannot seriously impose any definite structure on reality to the exclusion of others with *correspondingly* high coherence, or with an *incomparable* degree and area of coherence due to divergence of criteria of coherence and relevance. Rather than supporting agnostic attitudes, the possibilistic slogan supports the Copernican view of science, against the medieval or positivist, in the terminology of Polanyi.

About Copernicus, Polanyi says:

But he was irresistibly compelled by the appearance of his own system to claim that this particular feature of the celestial order, though derived essentially from experience, was true and real. Thus did he make for the first time the metaphysical claim that science can discover new knowledge about fundamental reality and thus did this claim eventually triumph in the Copernican revolution. Such is the claim of science to know reality, that positivism disowned in our time; and it is this same metaphysical claim, now widely discredited, that I want to re-establish today.⁹

The claim of Copernicus as revealed in his theories is that of showing how the solar system really is. But there is no claim of irrefutability, of impossibility of retraction, in his work. Individual scientists indulge in pleasant fantasies concerning the importance and permanence of their contributions. The phrase “thus it is, and not otherwise” does not in itself indicate any absolutism when confronted with the large perspectives of scientific development through the centuries. Copernicus rejected the notion that his theory was only a mathematical tool; he stressed its physical character, but this does not directly relate to the questions of pluralism and possibilism.

The new historiographical trend shows the importance of the theoretical *idea* rather than any definite *exposition* of a theory. There is, however, another feature of a theory that is stressed: the conception of the *function* (rather than propositional or cognitive content) of the theory and its methodology within the total structure of scientific *activity*. The revolution that began with Boyle concerned *how to do chemistry*; it was not limited to a search for tenable or, even more narrowly, “true” hypotheses and theories. It therefore also has to do with what to expect from a theory, the motives of theorizing. One phrase dominates most of the answers: the aim of a theory is *to explain*. But it seems that a fairly comprehensive description of scientific theorizing cannot be in terms of explanation—if we do not wish to take the term in an excessively wide meaning. A more suitable term, because it is more general, is *to cover* or *to account for*; a theory aims at covering, and, if successful, *does cover* certain classes of phenomena, facts, laws, other theories, and it *solves* certain problems. As soon as one asks “What does coverage imply, and what constitutes a solution?” one sees that very different answers have been given, some of them using *explain* as a key term, but others are in explicit opposition to this idea or family of ideas (cf. the ideals of description, not explanation, in Mach, Hertz, Poincaré).

If a description of scientific theorizing tries to do justice to very different ways of doing biology, chemistry, physics, and astronomy, then this must affect the very essence of how “scientific theorizing” is conceived in its complete generality. A very broad metatheoretical concept is needed to help frame and place the plurality of kinds of theories and their methodologies. A pluralist approach is needed, otherwise one particular way of doing things is explained as an unsuccessful attempt at doing it another way.

Pluralism of Methodologies: Incomparability

The great Indian mathematician Ramanujan exasperated some of his down-to-earth Western colleagues by refusing to concentrate on *exactly* how he arrived at his astonishing theorems. He tended to revert to his assurance that the goddess of Namakkal inspired him with formulae in his dreams. Even if this methodological perspective opened up by Ramanujan seems rather exotic, its importance is unquestionable: it reminds us of the nonclosed character of the search for methodologies. Anything can happen to any methodology, however well established. But a new methodology must somehow impart to the defenders of the old methodology the impression of both success and universalizability. Ramanujan was perhaps the favorite of the goddess of Namakkal; she might inspire Westerners with wrong formulae.

Insofar as ideas on the function and methodology ("how to proceed") of a theory are "built in" and assumed valid when the theory is advocated and used, it is cognitively (or at least logically) incommensurable with competing theories of the same comprehensive sort: If you adopt methodology M_1 , theory T_1 is confirmed and T_2 disconfirmed; if you adopt M_2 , T_2 is confirmed and T_1 disconfirmed. Or T_2 appears untestable from the point of view of M_1 , and T_1 appears untestable if M_2 is adopted.

The choice of a methodology is a choice of a conception or idea made on a wider basis than knowledge of the field of phenomena covered by the competing theories. It is an intrusion from the outside.

We have already mentioned the frequency of incommensurabilities and the resulting conceptual incomparability relation between "old" and "new." The *phenomena* or *facts* said to be covered by a theory, when defined implicitly by the theory and the associated practice of research, cannot be independent of the theory itself. A very different theory that in superficial parlance covers the same phenomena, *cannot* cover the same phenomena—there are no theory-independent phenomena to be covered. (If the total classes of phenomena of theories T_1 and T_2 are C_1 and C_2 , and C_1 is identical with C_2 , there must be a set of semantical rules such that the conceptual structure of T_1 can be defined by that of T_2 and vice versa. But semantical incomparability is one of the necessary conditions of a characterization of theories as "very different." With semantical incomparability at the theoretical conceptual level, semantical comparability at the obser-

vational level [by means of more or less arbitrary *Zuordnungsdefinitionen*] can only be ad hoc and not important for the life and death of the theories T_1 and T_2 .) In such cases, the new theory cannot “take care of” the phenomena or facts covered successfully by the old one. They are not defined and identified in relation to the new theory. Some designations may, however, survive the transfer. For example, “acid” and “vitriol” survived several methodological revolutions in chemistry. But, we must emphasize that in our use of the term *designations* here we are not merely speaking about words.

If there is a kind of isomorphy between the kinds of phenomena in the new and the old, the adherents of the new theory must be able to answer the “Where do you talk about such and such phenomenon?”—questions of the adherents of the old—with “To the phenomenon P in the old theory corresponds phenomenon P^* in the new.” The new theory does not say that P does not exist, nor does the old explicitly exclude phenomenon P^* —how could it?—but there are conceptual differences. P cannot be “named” in the new, and P^* cannot be “named” in the old.

The progress of the new over the old may now (in part) be said to mean that there are isomorphic relations of a P^* to any P in the old, and in addition, the new theory accounts for some P^* for which there are no analogues in the old, and which the old *cannot* accommodate.¹⁰

This way of seeing things also counters the tendency to view confirmation or disconfirmation as being of decisive importance. The superior quality, the “goodness” of the new theory, cannot be expressed in terms of falsification of the old, and the phenomena P^* , which are completely beyond the old theory, could never have been envisaged by the adherents of the old. However eagerly the adherents of the old theory would have looked for refutations, they could not have found any relating to those phenomena.

But such a conclusion may not be the last; there is no reason for not modifying certain well-developed concepts of confirmation, taking account of the relation of isomorphy between old and new “phenomena.”

Feyerabend (1970b) seems to argue along similar lines. The usual way to compare two successive theories is by an examination of consequence classes (Feyerabend 1970b: 219–20). When a theory T is superseded by T' , the accepted scheme of this comparison explains why T fails where it does, why T has been at least partly successful, and where T' makes additional

predictions. In a case study (comparing the classical celestial mechanics [CM] and the special theory of relativity [SR]), Feyerabend shows that these theories are incomparable. Their common class of consequences is empty. He goes on:

Even the case $c \rightarrow \infty$ (or $v \rightarrow \infty$) which gives *strictly identical predictions* cannot be used as an argument for showing that the concepts must coincide at least in this case: different magnitudes based on different concepts may give identical values on their respective scales without ceasing to be different magnitudes.

His conclusion is, "A comparison of content and a judgment of verisimilitude cannot be made" (ibid., p. 222).

All these reasonings seem to be based on a concept of "comparability" applied to the consequence classes of the theories to be compared. The existence of a nonempty class of sentences that follow from both of the theories is required to make the accomplishment of the comparison possible. The aim of such a comparison seems to be to decide which one is the closer to the truth (ibid., p. 220). There are cases, like the actual CM-SR case, which do not fulfill these presuppositions. The conceptual diversities are too great. With this I agree, but I want to object to Feyerabend's scheme of comparison. I do not accept that it cannot account for the *relation* that permits him to speak of "*strictly identical predictions*" (ibid., p. 221), even if no precise comparison of consequences is possible. He says:

Now extending the concepts of a new theory T to all its consequences[,] observational reports included, may change the interpretation of these consequences to such an extent that they disappear from the consequence classes of earlier theories.

This I accept, but does the new theory in Feyerabend's case cover quite a new area? Is it not a theory that *competes* with the older? How is it that the new theory can be said to supersede the older one?

As long as predictions from CM and SR are formulated at a high level of preciseness, they are incomparable and therefore never "strictly the same." But, descending the ladder of professional preciseness, we arrive at the level of everyday talk about velocities and lengths. At this level, where our sentences are intimately connected with a *praxis* that is loosely shared,

whether we work with CM or SR, the predictions *are* comparable. One may say that, robbed of their precise frame of conceptual reference, what we compare are not predictions unambiguously related to CM *or* SR; the necessary discriminations are lost. But the main point is to admit the indirect but inseparable connection between the precise formulations and the everyday level formulations (“the T_0 -level”). If we do that we can retain our view that CM and SR cover roughly the same area, that they compete and sometimes yield different, sometimes the same, predictions and that in a very important sense SR (and general relativity [GR]) have superseded CM. Classical mechanics and special relativity are comparable and commensurable, but not at the precise conceptual level.

The methodological anarchism or dadaism of Feyerabend (1970a: 17, 21, 104) is compatible with my pluralism, but there is a major difference of emphasis. For me, the notion of philosophical systems is central. I require a strong, “puritan” stress on internal consistency, or rather coherence, between a researcher’s and a community’s logic, methodology, epistemology, ontology, ethics, and politics. The hedonistic “Do as a researcher what you wish and have a pleasant time” is qualified by the addition “and, if a philosopher by inclination or necessity, be clear and coherent in developing your synthesis.” Because of requirements in other parts of the system, one’s methodology in scientific matters *may* have to be rather rigid. Anything will *not* go.

The Heuristic and Systematic Role of General Systems: Metaphysics, Maturity, and Stagnation

There is a growing acknowledgment of the *heuristic* role of ideas developed in the main philosophical systematizations since the time of the pre-Socratics. Referring to the participants in a recent research colloquium, Paul Weingartner is able to conclude as follows:

Die Teilnehmer stimmten darin überein, dass sich bei entsprechend gewissenhaftem Erforschen und Zurückverfolgen von Problemen der Einzelwissenschaften bis zu ihren Grundlagen die Notwendigkeit der Erörterung metaphysischer Themen und Theorien ergeben muss und dass die Metaphysik eine nicht zu unterschätzende Rolle bei der Entwicklung und Weiterentwicklung der Einzelwissenschaftlichen Theorien spielt.

(Weingartner 1967: foreword)

The reaction against metaphysical speculation has often had a positive effect on the willingness of scientists to work hard experimentally, and year in and year out to perform monotonous and repetitious acts (endless weighing and boiling in organic chemistry, inspection of thousands of photos of tracks of particles in atomic physics, etc.).

"It has, no doubt, been worth the metaphysical barbarism of a few centuries to possess modern science" (Burt 1924: 203). But mature scientists who enjoy and use speculation in their work are still afraid that young talents skip the pedestrian and sometimes onerous aspects of research. Bondi, the cosmologist, having lectured on some fascinating speculative matters, closed with this advice, "I am delighted if I have interested you in this, but do not do any work in such speculative fields until you have done some good pedestrian work so people know you are a real scientist, not just a crank" (Bondi in Yourgrau 1970).

The acknowledgment of the *systematic*—not only heuristic—role of metaphysics in its relation to science is less conspicuous, but also increasing. Among the many valuable contributions to a shift in attitude, I would like to mention the work of Joseph Agassi (1964).

Marx Wartofsky (1967: 155) defines classical metaphysics as follows: "a rational—that is a unified, coherent and critically appraisable—world-picture; in short, a model of reality." It is clear that a metaphysics in this sense will function not only heuristically, but also as a doctrine that suggests the form of future theories. Thus, the above-mentioned theory of levels suggests hidden-variable theories of subquantum physics.

Any adequate perspective of scientific development that takes account of the interdependence of science and metaphysics rules out the cleavage between mature and immature science. So-called maturity is a sign of professional isolation and stagnation.

According to Jean-Pierre Vigier, one of the leading physicists who has opposed the Copenhagen interpretation of quantum mechanics, the history of life on earth develops in accordance with concepts such as dialectics, contradiction, and totality. From Cuvier to Darwin, science progressed from formal to dialectical logic. Vigier and others—he mentions de Broglie and Yukawa—accept a theory of levels (*stufentheorie*) that suggests that there are processes and elements below the size of elementary particles and that research will have to penetrate that level and find the laws operating there. According to this metaphysics, there are an infinite

number of levels, each with a complete and specific set of laws, which, in their entirety, constitute an independent whole or totality (Vigier 1965: 65–85).¹¹

When we go from level n , within the realm of 10^{-n} cm to level $2n$ and enter the world of processes of dimensions 10^{-2n} (or $10^{-(n+k)}$, $1 < k < n$), a new science of mechanics has to be developed in order to take account of the more complex properties of reality. As part of this metaphysics of levels there is an “optimist” epistemology from which it follows that there is no level that cannot be reached by human research.

Behind the optimist terminology there is, however, an acknowledgment of accuracy as a graded magnitude, knowledge as refutable (p being known [as true or probable] at time t_1 , not p [as true or probable] at t_2 , and p again at t_3). And when judging assertions such as “science takes over the role of philosophy,” one must remember the Fichtean, Hegelian, and Marxist sources of terminology that make “science” (*wissenschaft*) into a broad term covering systematic, interpersonally and interculturally precise but “dialectically” conceived so-called knowledge. What I shall here call general systems might therefore be covered by this term “science.”

Our perspective is that of systematic, but unending, expansive research—the “spiraling” character of a research project if left to itself. Therefore, we cannot ignore research in the social sciences, psychology, history, and other fields that have as much to do with ourselves as beings capable of understanding and carrying out research as with the objects of our understanding—“the world.” This means that in talking about heuristics and systematics of metaphysics, we might as well talk about the (great) philosophical systems, or, better, “near-total systems.”

Until now, a near-total system has roughly included basic or regulative ideas in ontology, methodology, epistemology, formal logic, theory of language (including semantics), theory of value (including aesthetics), and ethics. As part of certain systems, there have been argumentations against the meaningfulness or independence of ontology, or formal logic or ethics, etc. I do not think that either these argumentations of old or recent antimetaphysics warrant any special consideration. As an antimetaphysician, antiphilosopher, or adherent of pure *praxis*, one is caught up in the game as soon as one tries to justify one’s position.

Professional philosophers in the West are now studying, with perhaps greater eagerness than ever before, the general systems of Aristotle,

Epicurus, Thomas Aquinas, Descartes, Spinoza, Hobbes, Leibniz, Kant, Hegel—and of many others whose thinking can be reconstructed in the shape of a general system. Insofar as the historical systems are inspired by strong “visions,” or general attitudes toward the world, ourselves, and everything else, they are indestructible. There are no ways of probing them from the outside.

Because of the great cultural and social distances between the systematizers themselves, and between them and our time, utilization of their works must more or less take the form of free reconstructions. As they are originally formulated, these systems use too many terms and concepts from which we are alienated today. Every way of exposition, arguing, and derivation must be reconsidered in order to get maximum contact with the manifold of present-day ways of talking and doing things.

As an example of a contemporary high-level “metaphysics,” Wartofsky mentions Whitehead, but of course there are others, although they are not so well known in scientific circles. We may refer to the general views of Cassirer, Jaspers, Meyerson, Heidegger, Sartre, Kotarbiński, and others. The system builders do not always have the kind of scientific career or background needed to appease the “tough-minded,” but there is no standing obligation to appease them.

There are a large number of different ways to conceptualize the relations between a “metaphysics”—that is, a near-total systematization—a *general system*, and systematizations of scientific “results.” I shall refer to some opinions relevant to this issue that appear at least *prima facie* to be conflicting. Carefully elaborated, they perhaps need not be so.

The following unpretentious list tries to build a bridge between those who think that selected, more or less unsystematic, metaphysical ideas have been, and perhaps are even today, a potent source of scientific ideas, and those, like the present author, who believe in a great future for general systems in which philosophical ideas, genuinely and to a large extent, furnish scientific propositions with more precise cognitive meaning than they do when left to themselves.

1. General systems help scientists to *understand* science. Without metaphysics, science degenerates to mere doing, performing operations. What is done can be understood in terms of a metaphysics.

2. A science differs from a rational, precisely formulated general system only in scope. A science treats a part or aspect of reality in special ways (methodological requirements).
3. Science gets its theoretical ideas, its leading, regulative ideas mainly from general systems. They are elaborated in the specifically scientific way.
4. The members of a group must have some central shared beliefs as part of their similar general system—this is a prerequisite for the development of a science.
5. Metaphysical parts of general systems provide the original, irreplaceable myths that may be developed so as to yield the testable components that comprise science.¹²
6. Metaphysical views offer different interpretations of facts; they function as regulative ideas. Scientific theories are special developments of these interpretations. When a theory is “refuted,” the metaphysical view itself is abandoned among active researchers. (Agassi 1964: 191 ff.).

In what follows I take *metaphysics* to be another designation for “(near) general or total philosophical view or system.” The term *near* is inserted to avoid, within the framework of this essay, those interesting but perplexing problems that confront us if we push far in the direction of the notion of an *absolutely* all-embracing view or system. Problems of self-reference are famous examples. A system is partial or fragmentary if, but not only if, it does not in principle cover all questions relevant to each proposition of the system. An anatomical description of the brain elicits questions that are deliberately and legitimately excluded from anatomy—for example, physiological problems. The physiological description or theory excludes the mind-body problem, or at least many aspects of it. Characteristic of the great general systems is the program of answering or at least formulating *all* main or essential problems in *whatever direction they lead*, but leaving out any topic that only calls for subordinate treatment. Philosophy of logic does not include technical logic; philosophy of mind does not cover theory of rote learning.

The main points in general systems may normally be roughly condensed into a small group of nontechnical utterances, each having a characteristic ambiguity and vagueness, but also a primordial suggestiveness.

An example:

1. Nothing comes of nothing.
2. What is, is corporeal.
3. Some bodies are made out of others, but some are not. The latter are what the rest are made of.
4. The bodies that are not made out of others have no properties except bulk, form, and weight.
5. The human soul is corporeal and evenly distributed throughout the human body.
6. The human body transports perceptions to its soul.
7. Sense experience is the basis of all kinds of knowledge.
8. What is sensed in sense experience is reliable and true of reality.
9. The direct objects of sense are sense images, not the things in themselves. The images are themselves corporeal.
10. Two utterances that contradict each other cannot both be true.
11. An utterance that claims to express knowledge is only meaningful when it is about something that can be experienced.
12. Every joy is a good, every pain an evil.
13. Wisdom teaches that enduring joy can only be reached through generosity, justice, friendship, and group loyalty.
14. The unwise seek power and are capable of political domination, but the wise know how to neutralize their influence on matters essential to human happiness.

This set of "point-of-departure" sentences contains some ontology, philosophical psychology, epistemology, logic, semantics, ethics, theory of value, and political philosophy. The first ten points can be roughly attributed to Epicurus as interpreted by one of his great admirers, the German philosopher Friedrich Albert Lange (1873: bk. 1, chap. 4).

Philosophical systems are elaborations, systematizations, and precisizations of basic views suggested by key sentences and terms of the kind exemplified. One of the regulative ideas common to these systems is that of coherence, the general absence of logical contradictions and inconsistencies. To test this, systems of logic and methodology are developed as part of the total view. These subsystems show great variation when made precise. Another regulative idea is that of agreement with (the one and

only) reality—tested in various ways relative to the system's inherent epistemology and methodology. The two regulative ideas are themselves incapable of being made precise without loss of neutrality in relation to the total manifold of systems. (The idea of such a manifold is itself incapable of being made precise without loss of neutrality!)

Implicit in my conception of a philosophical system with interpreted science as a genuine part is the principle of two-way criticism: scientific propositions, observations, laws, or theories may contribute to the weakening of a philosophical proposition and vice versa.¹³ The position of Lakatos is relevant here:

[W]e do not eliminate a metaphysical theory—as Wisdom suggests—if it clashes with a well-corroborated scientific theory. This would be a generalization of naive falsificationism. We eliminate it if it produces a degenerating shift in the long run and there is a better, rival, metaphysics to replace it. The methodology of a research-program with a “metaphysical” core does not differ from the methodology of one with a “refutable” core except for the logical level of the inconsistencies which are the driving force of the program.

(Lakatos 1968a: 180)

The main point here is the existence of common methodological maxims that make a two-way criticism practicable.

What can the experimentalist learn as experimenter from philosophy? Perhaps not much. But retrospectively one may note how ignorance of philosophy has impeded experimental research or has resulted in much useless labor. To take an example: Robert Boyle wasted his time carrying out experiments with the aim of refuting the theories of substantial forms—theories that, first, had long ago been abandoned by philosophers of his age and, second, were too vague to be refuted. When Spinoza read Boyle's *Certain Physiological Essays*, he thought Boyle's experiments had a worthier aim, namely to support mechanistic theories in chemistry, and criticized them on that basis. When informed by Henry Oldenburg, secretary of the Royal Society, about the real aim, Spinoza could not resist making the remark that he had not been able to persuade himself “that the very learned Mr. Boyle had set before himself in his Treatise on Nitre no other end than merely to show that the puerile and trivial doctrine of Substantial Forms, Qualities, etc., rests on a weak foundation.”¹⁴

By “science” I roughly mean the kind of science developed since the time of Galileo, with modern theoretical physics as the most notable

example. For arguments that this is only *one* kind, see, for example, the comparison by a learned historian, Joseph Needham (1963: 117–53), of Chinese with Western science.

Very little can be said *in general* about the two phenomena, philosophical system and science, even if we deliberately narrow the use of the term *science*. It has been suggested that philosophical systems have a greater vagueness, lack of clarity, and lack of logic. But preciseness or exactness may be very pronounced in a philosophical system. Johann Friedrich Herbart's philosophical psychology, a genuine part of his general system, is a good example. Empirical testability may be secured through long chains of mathematical derivations (a weak point in Herbart's psychology). On the other hand, *intersubjectivity* and *intercultural agreement* have never been greater than in the case of "modern" science. It is tempting to add to its list of characteristics quantitative methods and experimentation. However, even crude Baconian science proceeds by experiments of some kind, whereas the "system" of Galileo's and Newton's laws of motion, sometimes taken to be experimentally confirmed by Newton and inspired by Galileo's (doubtful!) experiments, may well be derived from the metaphysically loaded, nonexperimental physics of Rene Descartes. Remarking on the possibility of Galileo's influence, Brian Ellis says:

[I]t is much more plausible to suppose that Newton's laws of motion were derived directly from Cartesian physics, and that the only experimental evidence that was in any way directly relevant to the truth of Newton's laws was the evidence upon which Descartes and Huyghens supported their law of conservation of momentum. (Ellis 1965: 30)

Philosophy and science are interlocked in such a way that they can only be separated by making a number of more or less arbitrary distinctions.

A radical pluralism of approaches within a research community presupposes an adherence to the "live and let live" maxim that today perhaps is rather rare. Paul Feyerabend (1970a: 107) quotes Robert Merton, who tells us that the "organization of science operates as a system of institutionalized vigilance . . ." and adds:

In a warlike community of this kind proliferation will certainly lead to tension and nastiness (and there exists a good deal of nastiness in science, as well as in other critically rationalistic enterprises) but there is no need to combine proliferation with a war of all against all. All that is needed is less moralism, *less seriousness*, less concern for the truth, a vastly deflated "professional conscience," a more playful attitude. . . .

We might add that the concern for truth is essential, but the belief that it can be tested easily, and that when faced with two different approaches one of them must be nearer to the truth, is pernicious.

The use of the notion of a near-total philosophical system tends toward conceptual disengagement in the wars between schools and approaches. One acknowledges that *if* the differences are as deep as one could wish, clear-cut disagreements, such as we have when discussing facts, are not obtainable. Let the radically different approach live!

As it is not our task here to discuss all aspects of the complicated relations between metaphysical philosophy and science, I shall simply conclude by maintaining (1) that a theoretical idea that is part of a definite scientific theory may also be an idea within a philosophy, (2) that such an idea may well be worked out and made more precise and specific in *both* a professional philosophical system *and* a particular science, but (3) that the guidelines for the scientific exploitation of the idea are marked by stress on intersubjective (cooperational rather than operational)¹⁵ preciseness and testability.

Intrinsic Value of Research and Science

In the philosophy of science of Popper, Lakatos (1970: 162), and others, the progress and growth rate of sophisticated theory-centered science are highly valued. Sometimes it seems that only the achievements of the giants (Galileo, Newton, Einstein . . .) really count.

In contrast to this I shall stress the intrinsic value of research in general—not only scientific theory construction—among a growing number of the population, both those who directly participate in the professional life and also amateurs. The majority of researchers, the not very brilliant nor outrageously narrow-minded, determine the rate of growth. But in spite of some waste on research projects that no one enjoys, the overall picture is one of the intense satisfaction of a deep human urge: that of understanding and of a many-sided expansion and deepening of understanding. The growth rate is of secondary importance, especially considering the anarchic condition of politics: many of the results are still misdirected or used without responsibility and awareness of consequences. There is also, in the societies that stress competitive technical achievements, an overestimation of mere cleverness, brilliance, and intellectual prominence. This

severely limits the value of scientific activity. Lakatos divides research programs into progressive and degenerating, but some of his progressive ones are degenerating in the sense of having other-directed, status-seeking participants or of having acquired the character of a mere game. The above remarks exemplify valuations that are part of a philosophical system in which science is subjected to systematic valuation. It is clear that research policy is ultimately dependent (in part) on the larger system, which enables all human activity and all kinds of reasoning to be compared by considering a range of values, intrinsic and extrinsic.

By a research program, I mean a plan of how to use one's time as a researcher in the near future or over a longer period of time. Paradigms of such programs may be obtained from memos outlining the decisions of a group of collaborating scientists. They mostly contain speculations about what will be found at various stages of the research process and about how to react if "nothing" is found before certain dates. They contain strategic and tactical paragraphs, advice, rules, hypotheses, and hypothetical imperatives.

In spite of some excellent features in Lakatos's notion (or notions) of a research program, I think it wise to adhere to the above, more pedestrian idea of a program.

The New Historiography Applied to Itself: General Possibilism

The Discontinuity of Traditions and the Resulting Nonaccumulative Character of Scientific Knowledge

In view of its exceptional suggestiveness, let us start with the work of T. S. Kuhn. Roughly speaking, Kuhn suggests that a mature science develops normally within a tradition through acceptance of a certain way of “doing science.” The activity is, only in part, characterized by acceptance of a definite set of explicit theories, assumptions, and postulates as true and correct. Implicitly held views, presuppositions (à la Collingwood) contribute essentially to the tradition. Only a revolution, incited by persistent anomalies, makes the scientific community relinquish its loyalty to its tradition and take up a new way of “doing science.”

The results of a tradition N tend to be described within the new tradition $N + 1$ as mistakes eventually eliminated, or as lower approximations to truths established within the new tradition. In reality, however, there is, according to Kuhn, discontinuity between the old and the new way of doing science, which makes it impossible to do justice to the old within the framework of the new. And without a definite conceptual and practical framework, no scientific formulations intended to express *results* can be adequately understood. (The latter is implicitly assumed by Kuhn, and I shall also accept it with certain reservations.)

When scientists work consistently *within* a tradition, none of their experiments, observations, discoveries, or inventions can overthrow the dominant theory because these are framed and expressed in terms of that theory and presuppose specific implicit assumptions and a nonverbalized practice. But when persistent anomalies occur, some gifted scientists may try out entirely new ways of looking at things, and if they are compelling and successful, a new tradition will be inaugurated.

Because of the theoretical and practical disconnectedness of the old and new, there is *no genuine accumulation* of knowledge through the revolutions. There are reinterpretations of the old, certainly, so that there is a development, a progress as in philosophy or in the arts, but no simple broadening and deepening of knowledge or increase in approximations to a supertraditional and transtraditional truth. There is therefore no science *sub specie aeternitatis*.

Now even if we accept this view of the traditions and its resultant nonaccumulation thesis as true, there is nevertheless something that ought to be added.

From the point of view of general historical research, there has been, in the last thousand years, one important kind of accumulation: that of available documents and artifacts, manuscripts (observational journals, descriptions of apparatus), prints, machinery, collections of pictures.... More and more artifacts demand explanation. There may also be an accumulation, at least a very prolonged acceptance, of certain kinds of formulae, such as $F = ma$ and $S = \frac{1}{2}gt^2$. A historian will stress those materials that are retained in spite of variation of use, function, or meaning. This "accumulation," however, is noncognitive. The interpretations, meanings, and functions are not stored. Accumulation in a cognitive sense presumes persistence of function or meaning, and not only of tokens, signs, and behavior fragments. It is *cognitive* accumulation that the history of science does *not* show, according to Kuhn.

Let us then ascend to a metalevel and ask whether there is discontinuity of traditions in the history of historiography.

The Idea of Nonaccumulative Historiography Applied to Itself

In the Middle Ages, history was pictured so as to fit into a synthesis of theological speculation about God's interaction with natural events, and its methods were based largely on the doctrine of revelation and special authority. With Descartes new trends developed that changed the conception of history: on the one hand, conceptions within the frame of total systems, such as that of Spinoza, and on the other, methodological and epistemological positions in fierce reaction to dominant theological postulates. Thus *historical scepticism* introduced arguments against the decid-

ability of *any* proposition about the past. The age of enlightenment introduced a light-hearted belief in "rationality," and convictions of a one-dimensional progress, both of which highly influenced the history of science. The concept of superstition was used to combat any position in conflict with dominant theories and hypotheses. But then this image was rudely shaken when the romantic trend set up new value priorities, cultivating the not-yet-clear, the endlessness of the main human projects, the place of irrationality within the seemingly most rational undertaking. The organic view, so dominant in many environments, stressed the broad historical setting of any enterprise, including, therefore, the scientific, and thus undermining the idea of linear progress. Then came the rising storm against the excesses of romanticism—the new era of the fight against speculation, with the victory of vast pseudohistories like that of Auguste Comte, based on a model of progress borrowed from the age of enlightenment, the reinstatement of the priority of certainty and of finite projects, and the cult of scientific knowledge as certain, stable, and applicable to any field, including history.

As to the outlook for general studies of historiography—the "meta-science" of the writing of historical development—the richness of mutually incompatible historical accounts (or "theories" in a broad sense) of one and the same event, doctrine, or epoch is best seen from a study of particular cases. Examples include the well-known vagaries and fluctuations in conceptions of Platonism¹; the mutually incompatible conceptions of the historical Jesus (admirably outlined by Albert Schweitzer [1913]); and more pertinently, of course, the fluctuations in conceptions of Aristotle's physics. A new trend, very different from that of the first half of this century is gaining ground.² It was just as poorly foreseen as major changes in the conception of chemical elements or their numbers. To the great diversity of purely descriptive accounts of the past, we must add the diversity of philosophies of history—the Marxist, idealist, Hegelian, etc., conceptions of what history "is all about."³ Today philosophy of history includes the hotly controversial methodology and epistemology of history.

These remarks are meant only to furnish a reminder of the problematic character of "appeals to history" in debates that touch the metastudy of historiography. "The history of science shows that . . .," might well be replaced by the expression "There is at least one coherent conception of history of science such that it shows that. . . ."

Before Ernst Mach's inspired accounts of mechanics and optics, historiography of science received little attention from those who knew scientific research from the inside. It is not too difficult, however, to extrapolate backwards and construct images of science that are in harmony with earlier general historiographical trends. This offers a much-needed perspective for assessing current trends. An uncritical reading of the new, contemporary historiographers' exposition and criticism of the old dominant trend gives one, more or less inevitably, the impression that the new trend is the more correct. But appreciation of this very inevitability should in turn activate a desire to examine critically the sources of persuasion. These are then found to rest, in part, in the assumptions of the new, not of the old trend. *The exposition and criticism of the old is an essential part of the new.* The critical evaluation is thus, to put the weakest construction on it, not something constructed altogether independently of the new image. Let us apply this to the pretensions of the *new* historiography of science.

It seems that one is asked to accept it as *the* correct or adequate image. In my own case, I shall say that I can and shall accept it as based on at least one set of tenable assumptions, hypotheses, and observations. I accept it as an "image" that fits well into an outlook on history in general, and which is particularly well suited to certain contemporary research interests. Significant contributions to our understanding of the dependence of methodological frameworks on (research) interests are contained in the work of Jürgen Habermas (1967, 1968abc). Applied to itself, Habermas's theory of dependence announces its own limitation, its own dependence. This fits our possibilistic outlook.

More specifically one may ask: What kind or kinds of correctness or validity does one have in mind when deciding that a new image of scientific development is more correct or valid than a prior one? Considering the very indirect and complicated way in which the image, when expressed as a series of propositions, connects us with observations and artifacts of history, I propose that the kinds of correctness will have much in common with correctness of hypothetico-deductive theories. That is, observational propositions are, at best, *implied* by the theoretical proposition, given a set of auxiliary assumptions and postulates. On the other hand, nothing comparable to implication holds the other way—from observation to theory. An important consequence of this view is the plu-

ralist conception of images: there will be, in principle, an indefinite number of equally good images that fit the observations. But this anticipates a theme to be taken up later.

In what follows I shall attempt to show how Kuhn's doctrine of traditions, paradigms, or fundamental theories—a justly admired contribution to the new historiography—contains a fair portion of potential self-destructiveness if it is conceived as a doctrine intended to cover traditions, paradigms, fundamental views, and ideas outside the fields of natural science—for example, historiography itself. It should be noted, however, that such a conception of Kuhn's contribution is not the only possible one. We may, for example, take his utterances to be an invitation to adopt a certain kind of research program in metastudies of science—a different kind, for example, from that of Popper. But one need not, in all contexts, interpret his sentences in the same way.

Kuhn himself does not express any doctrine covering all traditions. Any argument against generality, however, is likely to provide only a thin wall, which is easily shattered by the pressure of parallel questions concerning the development of social science and history. For the kind of evidence that speaks against the doctrine of linear accumulation in natural science abounds in the humanities; the main difference being that dominance of one view or approach in the latter is more often restricted to particular countries or seats of learning. But these geographical limitations are not, of course, the essential point, which has to do with the effects of the dominance revealed in the history of historiography. A partial generalization from natural science to natural *and* social science is made by R. W. Friedrichs (1970).

Let us inspect Kuhn's crucial opening passage:

History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed. That image has previously been drawn, even by scientists themselves, mainly from the study of finished scientific achievements as these are recorded in the classics and, more recently, in the textbooks from which each new scientific generation learns to practice its trade. Inevitably, however, the aim of such books is persuasive and pedagogic; a concept of science drawn from them is no more likely to fit the enterprise that produced them than an image of a national culture drawn from a tourist brochure or a language text. This essay attempts to show that we have been misled by them

in fundamental ways. Its aim is a sketch of the quite different concept of science that can emerge from the historical record of the research activity itself.
(Kuhn 1962: 2)

According to this, a certain image of science has been dominant in the recent past; we can refer to it as number N in the general chronology of images. In our day a quite different conception, one may perhaps even say a new *concept*, is being created, a new image of science and of its methodology, and not through a piecemeal evolutionary process, but by a revolution, the historiographical revolution. Image number $N + 1$ is now, or soon will be, the dominant one.

What is the cognitive status of these hypotheses?

The process referred to seems very similar to what Kuhn, later in his book, calls a transition from paradigm N to a succeeding paradigm, $N + 1$, by a scientific revolution; except that this time the revolution is not in natural science but in the humanities, the *Geisteswissenschaften*. Such a transition affects all aspects of scientific activity, according to Kuhn, and it seems, therefore, that *the transition to the new historiography affects all aspects of precisely this new historiography*: the choice or selection of sources (corresponding to choice and selection of favored observational and experimental fields in mature sciences); the interpretation and conceptualization of the historical data (sources, documents); the hypotheses and theories (for example, about function of theories, chronological determination of discoveries, puzzle-solving character of normal science, function of anomalies, etc.); and the methodology (use of general historical methodology, study of the scientific community, research on "the research activity itself" rather than on their achievements in the form of theories).

But the persistence in time of historiographic reform or revolution disqualifies the new doctrine as more than image $N + 1$, the "textbook history of science" being image N . This disqualification seems to follow from the very conception of the "new historiography," the conception centering on concepts of pervasive research traditions and cognitive discontinuity.

In spite of the role of the new historiography as a new paradigm of historical and systematic research centering on science ("science of science"), it is not explicitly conceived as such by its representatives. Their own perspective, which stresses the broad historical relativity of the narrow absolutistic pronouncements of paradigm makers and their followers, is not

applied reflexively to the pronouncements of the representatives and advocates of the new historiography. But this *lack of self-reference* is just what the new historiography would say we should expect from those who share a paradigm. The historiographer cannot at the same time be both inside and outside.

Kuhn expresses the optimistic belief that the doctrines of the new historiography can “emerge from the historical record of the research activity itself.” But this formulation belongs to a kind that, elsewhere in his book, he takes to reveal the unhistorical character of the *old* image of science. Characteristic of the old way of talking was just this tendency to pretend that scientific theories somehow emerge from the facts, the observations, and “the artifacts and materials”—to use an expression employed in the humanities. The pretension conflicts with the Mach-Duhem-Poincaré theorem.

The sources of the history of science are now richer, but there were also excellent sources before our generation. It cannot be said, therefore, that we are the first to have sources to inspect and that previous generations could never have hoped to talk about what emerges from the historical record of the research activity *itself*.

Our conclusion, then, is that the way in which Kuhn assesses the old and the new historiography fits in well with the image he draws of how scientists in a new tradition assess the relation between the old tradition and their own. He is caught in his own relativism. So far as I can see, the new historiography cannot, on Kuhn’s premises, properly claim any special status in relation to other historiographies. Therefore the outsider—for example, the metahistorian—who is interested in *the history of conceptions of history* is justified on Kuhn’s premises in taking his picture as only one of the many that have been offered since Aristotle.

The doctrines of the new historiography as conceived by Kuhn are extremely general and pointed. Thus, normal science is “a strenuous and devoted attempt to force nature into the conceptual boxes supplied by professional education” (Kuhn 1962: 5). What interests the metahistoriographer in this context is the relation of such a doctrine to the view that the social sciences—presumably including sociology, methodology, and history of science—have not yet reached the stage of maturity at which scientific activity may exhibit dominating theories shared by the community of scientists. The description by the new historiographers of the old, however, presents the old in just this way: as a general way of doing

(history of) science, with dominating views common in the community. But even if this picture of the old historiography is not accepted, we may foresee that historiography, by becoming more mature, will eventually reach the required stage. Once "firm research consensus" has been reached, historiographers of science would, according to the Kuhnian conception, make a strenuous and devoted attempt to "force nature" (in this case, historical reality and the development of the sciences) into the conceptual boxes supplied by professional education.

It is not at all a question here of whether the new historiography constitutes progress of some sort or not (I feel sure that it does), but of whether it would not be wise for someone who is not himself a professional historian to expect *many* (an indefinite *Mannigfaltigkeit* of) equally persuasive historiographical movements, trends, theories, and methodologies, and to take each as essentially a hopeful project, a program for research and understanding. Working on such a program tends to elicit more or less definite images of the future. The expectations for the future development of theorizing, whether monistic or pluralistic, seem to follow the general regularities of images of the future. In the words of one of the pioneers in "futuresology," Frederik Polak:

Man's conscious striving to foreknow the future plus his partly unconscious dreams, yearnings, urges, hopes and aspirations for that future, periodically and successively, are condensed, crystallized and clarified into different sets of more or less specific, outlined and projective expectations or ideational goals. Such a set, at its end-stage of collective and positive, prospective and constructive development, may be called—introducing a new category of thought for social science—an *image of the future*. (Polak 1961: 16)

The striving of scientists, including historiographers, belongs to the most intense and well articulated; their image of the future plays a conspicuous role within their total view. Perhaps the image of theories merging into supertheories and of the succession of theories as a series of ever-closer approximations to one single theory, is expressive more of yearnings, urges, and hopes than of any realistic string of reasoning. An opposite image, however, might perhaps just as well be taken as an expression of the yearnings, urges, and hopes of sociologists of knowledge and historians who stress the irrational or discontinuous aspects of scientific

development. As far as I can judge, present methodological trends do not reject the possibility of working with hypotheses drawn from incompatible images of the future.

The new historiography of science has this profound character in common with Hegelianism, it surveys global epochs in which the actors, to some extent, must act like marionettes, not knowing their real historical function.⁴ Each epoch creates its own conception (image) of human history and interprets science within it.

By this view, however, actors from a given epoch cannot imagine that there have been, and will be in the future, other epochs of similar global character. This insight is hidden because of the discontinuous nature of the development of the human cultural enterprise (the variation of spirit), or *Zeitgeist*, from one epoch to the next.

Only the new historiographers of science can see through the epochs and tell us about their global character and the real function of certain kinds of activities—for example, “puzzle solving” (Kuhn), the character of which scientists themselves do not see through. They transcend their own historical situation, revealing what is hidden to others.

Those who, like myself, are not caught up in the act, but are simple, modest bystanders, may perhaps appreciate some of the accomplishments of the newest philosophy of scientific history, and at the same time vaguely conceive its nonglobal character, just as we are able to in the case of contemporary “mature” scientific theories. The bystander who has chanced to read drastically different general historiographical doctrines would seem well able to conceive the “unhistorical” character (in Hegel’s sense?) of the new historiography of science. But if the capacity to look at a string of historiographies from the outside should seem beyond him, who is left capable of discussing the issue at all?

In short, the application of the new historiography, to itself *hebt sie auf*, (with its stress on tradition and nonaccumulation), suspends it. But suspension (*aufhebung*) is not identical with refutation, and it is not necessarily an argument for abandoning a research program.

There is, incidentally, a touch of the Wittgensteinian approach in the work of Kuhn, and the arguments formulated by Gellner (1968: 377 ff.) in his rejection of the Wittgensteinian social science envisaged by Peter Winch also seem relevant to Kuhn’s position.

Winch acknowledges scientific activity to be a social activity. Concepts like "theory," "refutation," and "proof" have all developed in a social context; *therefore*, concepts used in that activity are social concepts. Let us inspect Gellner's critical review of the consequences Winch draws. For our own purposes, substituting "refuted" and "refutation" for "married" and "marriage," and "verbalizations in his scientific community" for "movements in church or registry office":

[A]n event acquires meaning through the fact that it is conceptualized by the agent with the help of shared concepts—and for Winch all concepts are necessarily shared—and that the conceptualization is essential to the very recognition of the event. Example: a man "gets married" not merely by going through certain motions in church or registry office, but by possessing the concept of what it is to be married. If the concept were lacking, the same physical movements, in the same places, simply could not be classified as "marriage."
(1968: 384)

After substitutions and some other pertinent changes are made, we get:

Example: a man "gets refuted" not merely by motions of the hands and mouths of critics, but by possessing the concept of what it is to get refuted. If the concept of refutation were lacking, the same physical movements, in the same places, simply could not be classified as "refutation."

From the point of view of Winch, then, to say today that Priestley was not really refuted by Lavoisier, in spite of his having been said to be refuted at his time, is nonsensical; within his scientific community, within the tradition, he *was* refuted. The meaning of "refuted" is to be found exclusively in the social relations between the users of the word. There is no external viewpoint, no independent standard. "What has to be accepted, the given, is—as one could say—forms of life" (Wittgenstein). Within the form of life of the scientific community of the time of Priestley and Lavoisier, Priestley *was* refuted. Getting married and getting refuted are only possible when we have the concepts, and the concepts form part of a definite form of (social) life. If and only if we take part in that social life do the concepts become meaningful, namely as vehicles in our specific social interactions. Therefore, in a different community, with different customs and practices, the concepts cannot be the same, and so incomparability and untranslatability result. No criticism is possible that transcends ways of community life.

What we say today using *our* concepts of refutation is utterly irrelevant when trying to *understand* what happened between Priestley and Lavoisier.

In the same vein Winch maintains that if a formulation does not express a hypothesis, but rather a true revelation within a community, it can never come to be anything else than a true revelation. "Oracular revelations (among the Azande) are not treated as hypotheses and, since their sense derives from the way they are treated in their context, they therefore *are not* hypotheses" (Winch 1964). Oracular revelations *are* oracular revelations. In a scientific community people do not take them seriously because it is a different kind of *community*, not because people see things more realistically in an absolute sense.

According to this social context- or tradition-absolutism, the methodological system of a community cannot meaningfully be criticized from outside. It can develop from within, but the frame is fixed.

Historiological Pluralism

The consequence of this Wittgenstein-inspired view is that insofar as there are historiographies, they are, as social interactions, part of a social system, a form of life, and are *specific* to this form of life. A historiographer may register the historical development of scientific activity (as understood within the system), but only within the boundaries of his system—that is, within a conceptual framework to which he and his results are bound.

This way of thinking leads to a *historiological pluralism*: There are as many historiologies as there are specific communities. And insofar as there is scientific activity, there are historiographies of science. As there are no external, no transsocietal standards of truth or correctness, all historiographies are equally valid. Criticism can only be immanent, not transcendental. Considering the absence of any meaningful comparison according to truth, correctness, plausibility or likelihood, confirmation, or any other graded predicate of cognitive import, they may be considered equally *possible*, using the term as in the Aristotelian tradition of *dynamis* and *potentia*. How many will ever be realized within finite time depends on the development and interdependence of societies on this and other planets. On our planet a definite kind, or family of kinds, is rapidly spreading over "the

industrial-technological-urban society." We may therefore expect a gradual narrowing down to one single unified historiography of science. But is this way of thinking clear, and is it consistent?

Such a philosophy inspired by the "form of life" holism *and* pluralism of Wittgenstein is only convincing as long as one does not admit that extremely different forms of life are already being compared by cultural anthropology and other social sciences, *and* that these studies at the same time are part of a Western kind or kinds of way of life. Historiography is part of a culture, and comparison of historiographies is part of cultural anthropology. Therefore, if we admit the existence of comparability in that general field, we should admit it in historiography of science. And if comparability in general is possible, why not comparability in terms of truth?

There are, however, other reasons for expecting and welcoming pluralism, and perhaps also possibilism. Let us consider some problems of observation.

The last hundred years have witnessed an increased flow of experimental results suggesting inescapable fluctuations in even the most elementary kinds of observation. Some are fairly constant, defining a "personal equation"; others are erratic and difficult to trace. Therefore not even on the most elementary level is there any basis for such utterances as "I saw what really happened," except as part of the language games of everyday life—that is, where the expression can be used irresponsibly enough for it not to seem very odd that one admits the next day that one was wrong.

In chapter 2 (page 23), we stressed that what is identified as the phenomenon observed depends on our metatheory of the process of observation. In the field of historiography, the problem of identifying the phenomena observed is still more conspicuous than in the natural sciences. If several different metaobservational theories are applied in historiography, a still greater diversity of images is expected to emerge.

The statements overriding my observation-statement may be of any kind. There is no commonsense empiricism such that observations are taken as a fundamental source of insight or truth. (Some experiments on nonphilosophers' empiricism are reported in Naess 1937–38a: 382 ff.). Any "empiricism" worth working with today takes broad experience, not narrow observation, as its key term.

It is an old maxim in the methodology of history that if there is only one eyewitness to an event, one may expect historical accounts to be categorical, whereas if there have been two or more, all documented, there will be an admission of uncertainty as to what really happened. The reason, of course, is simply that the records of two eyewitnesses tend to differ to an astonishing degree, whereas, that of one cannot. As everyone knows, the experimental psychology of witnessing has shown the vast degree to which a conception of what happens is determined by the expectations, orientations, dispositions, and emotional states of the observer.

It has also long been clear that in experimenting with higher organisms, the experimenter influences the behavior of the subjects—for example, by making them “verify” his prejudices.⁵ Ivan Pavlov explained in 1929 that on checking certain “experiments it was found that the apparent improvement in the ability to learn, on the part of successive generations of mice, was really due to an improvement in the ability to teach, on the part of the experimenter” (Gruenberg 1929: 327).

And experimental group psychology has shown how group membership and relations of authority, dependence, and so forth, introduce astonishing conformity into observations. Even the mere fact that there is a majority reporting something can have a profound influence on how things are perceived and conceived (cf. Asch 1965).

Therefore, among researchers alert to these findings, a narrator who solemnly appeals to an event “itself,” or to what “really” happened, is looked on with suspicion. A historian’s claim to be telling us what *really* happened in the French Revolution or what Hitler *really* did, will probably be interpreted by his colleagues as an effort at crude persuasion, or as irony. The more or less *irreducible multiplicity of versions* and their finite lifetime are taken for granted by historians.

But just as in persuasive political historical accounts, we find in texts in philosophy of science expressions intended to make us believe both that a definite version of what happens in a laboratory is the only correct one, and that the narrator has seen all that is worth seeing.

Claims about what “*really* happens” in a laboratory or what scientists “*really* do,” however, are made by narrators who feel the need for an undisputable observational basis for methodological doctrine and any other doctrine that is relevant to the philosophy of science.

"Let us inspect what the scientist *actually does*," "let us inspect *actual scientific practice*," how measurement "*has actually functioned*," "let us ignore what he *says* and *see what he does*"—such phrases are very common, not only in texts by P. W. Bridgman, but even in the writings of some of the new historiographers. I take it as an indication that they engage in rhetoric or have not applied their own basic views about scientific development to their own historiography.⁶

But if reporting on a *praxis* is reporting on *actions*, and not only on fragments of behavior, and group adherence is also involved, only a very simple faith can make one try to reduce the number of versions to one.⁷

The Mach-Duhem-Poincaré principle, if applied to reports of actions, leads to a doctrine of indefinite multiplicity of theories, all of which are equally well suited to cover the reports. That is, there is no inductive method that would make theories and hypotheses about scientific activity, viewed as sets of actions, into valid inductive inferences from eyewitness reports of behavior. There is no logical compulsion; as in physics, implications go *from* theories, hypotheses, auxiliary postulates, assumptions, sciences, and initial conditions *toward* observational reports—not the other way. In this realm of action, too, by implication, we may say with Lakatos: "Truth does not flow upwards."

Or is all this wrong? Is there, after all, a basic difference between the natural and the social sciences, the latter being subjected to the methodology of hermeneutics, philosophical anthropology, *Geisteswissenschaft*?

The Mach-Duhem-Poincaré thesis holds for the relation between, on the one hand, observation of actions (doings, not mere behavior-fragments) and, on the other, theories covering the actions—except that there are special methodological rules, corresponding to different doctrines of action, to be obeyed in such observation. We cannot directly "see" what another person does, even if this seems fairly obvious among good friends in favorable circumstances. A change in circumstances (due to war, business, divorce, illness . . .) can make behavior ambiguous and lead to a reevaluation of past behavior. And that involves reclassification ("I always thought he was doing such and such, but he wasn't").

Then again, several "doings" may be related in such a way that one is part of the other—we (1) buy a newspaper and (2) look for the arrival of a violinist as part of (3) the entertainment of a guest. If the doings are complicated, as they are in the case of scientists in laboratories, one cannot

trust the actor to give an adequate account—if he thinks you wish to help, he says one thing; if he thinks you are a nuisance, another; and if both, a third. And his account will, in any case, reflect his special intentions at the moment more than his basic conceptual frameworks. So unless you share them, you have to be careful; the two of you see quite different things. The plurality of conceptions, even among presumably equally competent scientists, is quite astonishing.

Accordingly, there is no basis at all for the assumption that we can single out one unique description of use as the correct one. One may wholeheartedly accept that reality is one without accepting that there is only one correct description of it. “Do not listen to what they say; see what they do!” is sheer infantilism if interpreted as a proposed guarantee of access to some *definite* way in which terms and sentences are “actually” used. Of course, fragments of verbal as well as nonverbal behavior are relevant, and what a scientist “does” includes his utterances. But a person who is observing the scientist’s actions is not in a better position to describe those actions than is the scientist himself. And there is certainly not just one description that he can give. There is no end to the different characterizations, some mutually consistent, others inconsistent, that a researcher on doings can offer in responsible social research.

If two witnesses of what is done in a definite laboratory on a definite day differ widely in their descriptions, we may analyze the differences and find them to depend, at least in part, on complicated sets of expectations, evaluations, and assumptions about the wider goals and aims of the activity on that particular day in that particular laboratory, but also on what is here called their metaobservational theory—be it conscious or not. Articulated in the form of assumptions, propositions, and (postulated) rules, the background of the two descriptions might be formed as two systems. Taking “if p , not r , is assumed or postulated, then q ” as consistent with “if r , not p , is assumed or postulated, then non- q ,” the two descriptions $[(p \ \& \ \bar{r}) \supset q \text{ and } (r \ \& \ \bar{p}) \supset \bar{q}]$ may be consistent even if their propositions (q and \bar{q}) are mutually inconsistent. And they would *both* add to our information about what was done in that laboratory on that day. Thus, the pluralist view is, heuristically, a relationalism, not a relativism; that is, each description is conceived in its relation to a system and is assessed immanently in *that* relation. The following are some conclusions:

1. Arguments today *against* the applicability of the new historiography theses (based on discontinuous, all-embracing traditions, and nonaccumulation of scientific knowledge) to the new historiography itself do not decisively outweigh the arguments *for*.
2. Applied to itself, the new historiography is just *one* way of doing history, a tradition No. $N+1$, and when articulated and made scientific, or pseudo-scientific, expresses *one set* (or one class of sets) of fundamental postulates, assumptions, definitions, and rules (recommendations, prohibitions, etc.). As in the case of physical theorizing, rules, conventions, and empirical elements are intimately interwoven, supporting neither extreme conventionalism nor naive empiricism.
3. To the outsider tentatively working out other sets, just for the sake of a survey of coherent structures, the new historiography appears as a "systematizable" possibility—a structure that is neither probable nor improbable, neither correct nor incorrect, but nevertheless *tenable, workable, and valid*—that is, valid as a fundamental systematizable possibility as part of a philosophical system.
4. Without himself in any way renouncing research, at least in the form of explicating possibilities of the most comprehensive kind, the interested outsider is grateful to those who work out a new way of doing history of science, and grateful for both the large visions and minute details offered by the participants.
5. The sets of fundamental postulates and assumptions implicitly adopted by historians of science working within a tradition are not specific to their discipline, but part of more general ways of doing research and asking questions. They may be conceived as expressive of general views, "metaphysics," or philosophical systems.
6. If one (with Kuhn) accepts, as part of one systematizable possibility, the thesis that research within a tradition only "gets going" when a community of scientists who share fundamental assumptions is formed, and also accepts the validity of all fundamental sets, then scientific results and achievements, as *understood* results and achievements (and not only as filed printed matter, tapes, and photos), must be accorded a multiplicity of interpretations of the second-order variety. The first-order variety stems from the one-many relation between observation and theory, the second-order

from the one-many relation between observation of scientific activity and historiographical metatheory.

But this leads to a final topic.

General Possibilism

A superhuman scholar, able to interrelate all dominant scientific theories at a given time, might be said to obtain a “scientific worldview.” Such an expression, however, is misleading in at least two ways.

First, by “scientific” we could refer only to what scientists, without opposition from colleagues, happen to assert and publish at a given moment. But then “scientific” would be taken largely as a sociological, not an epistemological or a methodological, concept. Dominance to the point of lack of opposition is seldom acquired, and, if acquired, it tends not to last long. Any synthesis would therefore be largely arbitrary from an epistemological point of view, and its formulation would tend to be out of date before its date of publication.

Second, an integration of the professional texts is dependent on ontologies that are not results of research but derived from everyday life or philosophy. The expectation of a definite scientific worldview (*wissenschaftliche Weltanschauung*), or successive approximation to such, is based on crude misconceptions of the scientific enterprise.⁸ Science has no autonomy in relation either to philosophy or to conceptions of everyday life.

What surveyors of research can offer as a partial substitute for comprehensive worldviews is, rather, a kind of catalogue of skeleton encyclopedias.⁹ Each might cover the whole world or “all there is,” and each may be internally consistent, but they would nevertheless be mutually inconsistent, or strictly speaking (see p. 130) incomparable because of mutual untranslatability. This possibility is the offering of carefully articulated individual *systems*.

A researcher eager to contribute to a department of science may not need such a picture in his research. He may be interested only in a tiny part of one of the pictures, or he may even find it heuristically necessary to work with an inconsistent combination of two. Or he may even neglect the philosophical distinctions required to arrive at a definite picture. The definiteness of his intentions may remain subphilosophical.

Few quantum physicists offer the philosophical public a fair chance to make a choice of how to integrate physics in their philosophy. One of the few is Bernard d'Espagnat (1965), who offers three philosophical interpretations: "... at the moment there is a true freedom as to choice face to face with fundamental problems."

Proliferation and pluralism of theoretical ideas and programs are now positively valued by Popper, Lakatos, and Feyerabend for methodological reasons, but only in physics and adjacent fields. When it comes to philosophy—I think now of attempts by great systematizers like Aristotle, Thomas, Descartes, Spinoza, Kant, Hegel, Marx, and Whitehead—it seems that a corresponding proliferation and pluralism are negatively valued. But on what grounds does one envisage growth through nonconformism in physics, but not in philosophy?

Creative physicists are adopting, and presumably will continue to adopt, mutually inconsistent views, all of which are sources of inspiration to researchers whose fields are broader, covering the relations of physics to society or to wide areas of scientific and philosophical systemization. To try to shut off the pluralist sources of inspiration is not only a methodological but a general cultural evil.

To take an example: The fight between proponents of conceptions labeled "idealist," "instrumentalist," or "realist" is largely barren unless the explicitness, comprehensiveness, and consistency of each *kind* of (allegedly) incompatible conception are worked out with the utmost care. Research promoting this sort of analysis, should, at the moment, be higher priority than attempts to show that only one of the vaguely outlined conceptions is correct.

What holds for physics holds for all other sciences today. Fundamental questions are given more or less vague and ambiguous answers. These questions provide centers of confused, partisan debate that have the primary aim of showing that one particular view is the right one. The more precisely and the more comprehensively the views are elaborated, the less chance there is of monism; a plurality of systems are created that stand out as important, workable *possibilities*. Increases in preciseness and explicitness inevitably involve thinking along the lines of a total philosophical system.

The dissimilarity of methods, pretensions, focus of interest, logical instruments, and other features closes, at least temporarily, the road back to sloppy monism.

Whatever the value, or lack of value, of a variety of scientific world pictures for the researcher in his daily work, there can be no question, to my mind, of the cultural value of such systematizations. The history of philosophy teaches us about the gigantic general systems created by outstanding personalities, each supported and elaborated in relation to a tradition or set of traditions (in the case of Spinoza, for example, medieval Jewish, Renaissance Italian, and rationalistic French traditions). No one can tell to what extent there will be original creations of this kind in the future. But with the pervasive role of scientific research in the years to come, there will at least be the more modest task of worldview typologists and systematizers—coordination and mutual elucidation of the materials derived from science and philosophy.

The relief from pressure toward conformity seems to me to offer great political advantages. Possibilism tends to diminish the conforming effect of science on society and on the individual. It increases the personal freedom in choice of philosophical principles by furnishing precise and elaborate alternatives.

When science was still frowned upon by political rulers, it was important for the researcher to fight for his position and even for his life, but now things have changed: rulers largely depend on scientists and must keep in favor with them. “Scientific” is a label that carries immense prestige in Western as well as some Eastern countries. The prestige is used by powerful “vested” interests to narrow the range of what are considered tenable views. This pressure is rarely counteracted by eminent scientists, who tend either to be retiring and uninfluential socially, or to be absorbed in particular approaches that make them unwitting supporters of crude dogmatism. Thus, today it is not by its own theoretical inclination, but through pressures on the scientific enterprise as a whole, that science is largely, but of course not consistently, moving in the direction of a *unity of outlook*, a “scientifically” sanctioned conformism.

The political consequences of such a “scientifically” sanctioned conformism have been a steadily recurring theme in the so-called Frankfurt School of sociology and philosophy, represented by authors such as Max Horkheimer, Theodor Adorno, Herbert Marcuse, and Jürgen Habermas. Compare the following from Horkheimer and Adorno’s *Dialektik der Aufklärung* (1969: 4):

Die Angst des rechten Sohns moderner Zivilization, von den Tatsachen abzugehen, die doch bei der Wahrnehmung schon durch die herrschenden Usancen in Wissenschaft, Geschäft und Politik klischemässig zugerichtet sind, ist unmittelbar dieselbe wie die Angst vor der gesellschaftlichen Abweichung.

In this situation it is imperative to stress the freedom of choice that remains despite the overwhelming mass of highly reliable detailed “knowledge” added every year through research and crudely communicated to the public as “authoritative.” But without a foundation in radical pluralism, any stress on choice tends to be insincere. Those who, for lack of philosophical education, see a lot of “hard facts” in textbooks, and mistake increase of area of agreement for increase in truth content, cannot honestly combat the ensuing conformism, any more than can the intensively, personally engaged researcher who is “in love” with a definite methodology or theory. Radical pluralism of research programs is a powerful liberating force.

People still consider a person’s general outlook, his view of life and the cosmos and world history, as part of his whole personality—or at least within many subcultures a tendency to pay lip service to such a conception of a person still persists. But popularized science, whose present tendency in the matter of worldviews is nearly always toward the authoritarian and monolithic, influences us in the direction of either abandoning all our efforts to retain a general outlook, or of forcing it into conformity with the dominant world picture.

A worldview typology that uses different scientific world pictures and different philosophical systematizations as its basis will show how freedom of choice increases as a view draws closer to the fundamentals—ontology, methodology, epistemology, philosophical logic, anthropology, and philosophy of history. In this way I see such a typology as a source of cultural diversity and personal integration. It would comprise a new trend of popularizing science, inviting the general public to study and enjoy diverse interpretations in their comprehensive settings.

In what way freedom of choice increases might be better explained by the concepts “definiteness of intention” and “preciseness,” which were introduced in my *Interpretation and Preciseness* (1953b [SWAN I]). The latter

can be sketched as follows: a sentence T_i is *more precise than* a sentence T_0 , if there is at least one interpretation that T_0 admits, but T_i does not, and if there is no interpretation admitted by T_i that is not also admitted by T_0 .

Preciseness thus defined is a transitive relation, and definiteness of intention can be measured with reference to chains of “precizations,” T_0 , T_1 , T_{11} , T_{111} , . . . , where T_{111} (and T_{112} , T_{113} , . . .) are more precise than T_{11} (and T_{12} and T_{13}) and T_{11} more precise than T_1 . If a person fails to discriminate at the level of T_{111} , but succeeds at the level of T_{11} , T_{111} is said to be a transintentional precization to his set of discriminations. All measurements are relative to discrimination grids that function as reference systems.

The following diagram illustrates the various concepts:

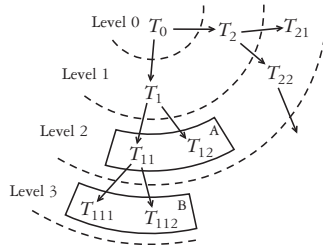


Figure 4. Illustration showing the levels of discrimination and preciseness. If an intended discrimination can be located at level 3, it is more definite—more precise—than a discrimination that can be located at level 2 but not at level 3.

Applying these concepts to the pluralist thesis, or view, I suggest that we present, for instance, the various expositions of physical theories at the T_0 level. It is a relatively neutral level only because of its low level of philosophical discrimination. The professional expositions have, of course, great preciseness and great definiteness of intention in professional, scientific directions. But what concern us here are the broad, nonscientific, untechnical interpretations of terms like *matter*, *particle*, *mass*, and *force*. These interpretations furnish our common ground in discussions. In this respect, the expositions are at the T_0 level. Any sufficiently vigorous effort to uncover and articulate the full cognitive meanings of such terms reveals

the philosophical idiosyncrasies of the author. Ultimately, the same philosophical system is presupposed. It is only within such a system that it is possible to offer precise reasons for the decision to continue working with one theory rather than any other. The requirements that a choice must be rational and the results coherent are, so to speak, formal requirements whose contents are determined by the value norms and postulates of the adopted system. Taken in isolation, they are not sufficient to advise the scientists.

From this it follows that it is problematic to compare scientific theories or images of the scientific enterprise that are made precise in different philosophical directions. Such comparison must presuppose a common standard or value system. But a standard can only be valid within a particular philosophical system. If the theories or images refer to philosophical systems that are sufficiently different, they are incomparable. Both are to be considered *valid*, but as this concept refers to a definite philosophical system or class of systems, one cannot say that the theories or images are *equally* valid. A further discussion on validity appears below.

I also want to point to another consequence of this view. It is what we may call the *interdeterminacy relation* between preciseness and definiteness of intention on the one hand, and neutrality and comparability on the other hand. If one starts making an exposition of a theory more and more precise in a definite philosophical direction, what is gained in preciseness is lost in philosophical neutrality and comparability, and vice versa. This result can also be extended to expositions of images of scientific research.

A monolithic, soul-shrinking jargon still prevails in discussions on worldviews among creative scientists. It is as if the prestige of science as a truth-seeking project depended on the emergence of one definite world picture, *the* scientific world picture, as opposed to all the unscientific ones—the confused, vague, irresponsible metaphysical views, the grand illusions of the religious believer and of the cognitively irrelevant artist.¹⁰ A consequence of this is that increase of uniformity of opinion is cheered as if it were a reliable indicator of increase in truth content, neglecting the possibility that it is the urge to conform that prevails.

Now, I should have to admit that although the foregoing remarks may have a sufficiently clear sense as suggestions within general philosophy of culture and politics, they are highly resistant to precise formulation

in relation to philosophy of science. Tentatively, however, I propose the following formulations:

1. *All consistent, comprehensive points of view have a non-zero status of validity.* As to comprehensiveness, the points of view must explicitly include logic, methodology, semantics, and ontology—or a set of arguments to the effect that a body of rules and assertions of one or more of these kinds is unnecessary, nonsensical, or has some property justifying its absence from a maximal comprehensive view. Instead of *comprehensive*, we might use the terms *near-total* or *near-global*. Instead of *points of view*, we could substitute *view* or *system*, but not *doctrine*, since norms, postulates, and assumptions are usually thought to be absent from doctrines.

The term *validity* is used in such a sense that the first links in argument chains within a particular system are taken to be true or correct—until further notice. That is, it may turn out that acceptance of the first links (initial sentences) in an argument for a methodology M_1 entail acceptance of some observation sentences that contradict some other set of first links in the system. It has then to be decided whether to modify the methodology M_1 or dispense with the initial sentences affected by the observations. If this cannot be decided *within* the system, a modification is called for that, strictly speaking, means the abandonment of the system in favor of one resembling it.

The somewhat awkward quasi-scientific expression “non-zero status of validity” is preferred to “true,” “valid,” and “correct” for several reasons. Negativity (non-) is used to stress a minimum assertion. It is untenable to proclaim that validity is *absent*. A kind of validity is present. But what kind? “True” has too strong associations with “agreement with reality”; systems constitute conceptions of reality, rather than mirrors or isomorphical structures. “Correct” is too suggestive of measurement against an already established yardstick.

2. *Reality is one.* This short sentence functions as a pronouncement against those who think that the acceptance of the above thesis of equivalence of validity goes against ontological realism. There cannot be but one true system, it is said, because there is only one

world, or reality. If two systems are mutually inconsistent, they cannot both be true—that is, agree with reality. But the relation between system and reality is not one of mirroring or copying, not even one of structural isomorphism. Therefore:

3. *Two mutually inconsistent systems may both correspond to reality.* The view we have of the relation of systems to reality *need* not be of the special kind that makes it impossible for there to be many mutually inconsistent systems covering the same reality or part of reality. The rules of use for the term *reality* make it a kind of “regulative idea” in the sense of Kant. They do not make it represent a definite structure or substructure.
4. *A comprehensive and consistent point of view is not something a person (or community, or society, or epoch) can “have.”* This strange assertion is needed to counter those who equate the equivalidity thesis with what they call “voluntarism” and “subjectivism.” One can, as a person, without restrictions, *choose* one’s philosophy or metaphysics and adopt the kind of language one wishes to use. So, according to this perspective, the particular comprehensive view one adopts is purely a question of personal choice. But against this I hold that if an explicit choice is made, and based on a scale of priorities, one already *has* a very substantial part of such a comprehensive view. Any view that has the required degree of comprehensiveness is such that a whole person cannot stay outside and choose. If a person is, nevertheless, said to “have” such a view, it must be regarded as part of his personality. He cannot step out of his own skin. But he can change. And therefore “his” view may change.

Here I am only making use of the vague distinction stressed by Gabriel Marcel between being and having. A complex entity like a community, a society, or an epoch, can be said to “have” a comprehensive and consistent view only in a rather general, vague sense. There are trends, forces, and traditions that dominate the overall picture when seen from the outside (by the historian). But these trends, forces, and traditions are in part constitutive of the collectivities; we do not find the comprehensive views inside the community, society, or epoch. They are integrated in such a way that the community minus the tradition would not be that community.

And they are not to be identified with theories that the members of the community articulate and call “our view of life” and so forth.

5. *Points 1 through 4 are not capable of being made precise beyond certain (ill-defined, modest) limits of definiteness.* That is, any effort to construct a precise, professionally adequate theory (doctrine) of comprehensive points of view, including a “typology of possible worldviews,” is bound to be self-defeating because it requires an elaborate conceptual taxonomy or framework, and there are no such systems that are neutral in relation to the comprehensive points of view to be classified. They can each include a metasystemic doctrine, but the variation of systems cannot be maximal. The efforts of Georg Hegel, Wilhelm Dilthey, Karl Jaspers, and others to erect a general typology of *Weltanschauungen* show the dependence of their taxonomy on their particular comprehensive views. Hegel forces his framework on *his* classification; Dilthey imposes his historicism on all systems; and Jaspers lets them all float in a kind of Kierkegaardian pseudopsychiatric medium.

Appendix

Historical Note on Possibilistic Pluralism

A prominent Popperian and a historian of the Vienna Circle asked me to write a few words about the historical background of some of my ideas. In 1934 and 1935, I spent fourteen months in Vienna and profited immensely from the friendliness, eagerness, and intellectual fertility of Otto Neurath, R. Carnap, M. Schlick, and F. Waismann. I saw little of K. R. Popper and P. Frank, but certainly learned from them. During my stay I wrote the final version of my *Erkenntnis und wissenschaftliches Verhalten* (1936)—an effort to use biological rather than physical or logical models in epistemology *and* methodology. It is fiercely “scientistic” in the sense of proclaiming that an attempt should be made to investigate epistemological problems by existing scientific methods or techniques or by new ones created for the purpose. But because of the many-sidedness and perhaps inexhaustiveness of philosophical questions, research in different sciences would always be relevant. Logic and psychology are equally relevant.

I found Neurath’s term *Ballungen* excellent for characterizing philosophical sentences.¹ (“Will is free,” “Matter is discrete,” “Truth is agreement with reality,” etc.) Pluralism and proliferation of theories on the highest level are necessary because of the inexhaustible many-sidedness of philosophical formulations. My own total, behavioral approach I characterized as *one* model among an indefinite plurality, just as relative in its achievement as opposite models (Naess 1937–38a). Thought models such as mine or those of the biologist Jakob Von Uexküll may be used to survey other models and to make a theory of the relativity of any model. But other models may in turn survey all the biological ones, including the biologically inspired theory of relativity of models just mentioned. There is no end anywhere, and, therefore, not even an ultimate relativity. (A defense of this position against certain arguments is found in Naess 1937.)

Because of intervening initial-condition statements, auxiliary hypotheses, postulates, and ad hoc assumptions, falsification and verification are symmetrical: “observations” can only give theories faint pluses and minuses, and one and the same observation always supports (in principle) a variety of mutually contradictory theories. This goes directly against Popper’s views as many of us interpreted him in 1934–35 in Vienna. Let me go into certain details here.

The Vienna Circle was a nucleus of a movement for “rationality” and against certain forms of metaphysics, which at the time were closely allied with fascism and national socialism. It had all the missionary zeal of a movement, and it was touching, but also somewhat alarming, to watch Otto Neurath embrace aloof and aristocratic Polish logicians of various philosophical affiliations and proclaim, “We agree! You are one of us!” If Neurath sensed that one was *somehow* on the right side, one was identified as a sort of logical positivist. Protestations were of little use, and disagreements were conceived as due only to “unhappy formulations” (*unglückliche formulierungen*), and there was always a remedy for that.

When friends who *certainly* were on the right side persisted in remonstrating, saying, “No, no, I definitely *disagree*”—and Popper and myself belonged to this category—the situation became somewhat awkward. How could we continue to misconceive our own views? Personally, I felt the nagging questions: Do I *try* to be original, do I cling to a view *because* it is mine? I do not know how Popper felt, but in the air there was a suspicion that he grossly overrated his disagreement with the inner members of the circle. A groundless and unfortunate suspicion! Looking back I see more clearly how we should have made better use of the resources of positive, original ideas in *Logik der Forschung*. In my case, little was lost because I had only a handful of ideas and some of them were too sceptically colored to inspire concentrated research.

In the case of the theory of falsification, I sided with Neurath against Popper, as we conceived his position at that time, but tried to avoid the implicit conventionalism of the former by taking sets of criteria of confirmatory and disconfirmatory instances to have an objective, testable status on the metalevel and therefore not taking them to be a matter of “decisions” (Neurath’s *Beschlüsse*). I believed in a broad, many-faceted science of science in which decisions were looked on as ultimately based on (not implied by) logical and empirical research.

In Vienna in 1934–35 the hypothetico-deductive method was taken to be the central method of natural science, and natural science the most “scientific” of all kinds of science. It was therefore a crucial question to ask to what extent potential pluralism of mutually inconsistent or incomparable theories was derivable from an exact account of the method itself. Accepting neither Popper’s theory of refutation with its antirevivalism nor the idea that theories in an interesting sense could be rated as more or less probable, I maintained in *Innføring i logikk og metodelære* (1949) and *Symbolisk logikk* (1942 and 1961) an extreme pluralism coupled with the opinion that “assertion strength” (proportional to improbability) is a plus (but not the only plus) for a theory.

The pluralism presented in my small, stenciled works in the 1940s (we used this method in Norway around this time as a substitute for printing, but distributed selected items internationally) was a pluralism of models in the broad sense in which we talk about atomic models and thought models (*gedankenmodelle*). However sweeping in their range of application, they should be distinguished from systems with theorems and theses. From models one was to derive, not general theses, but general working programs (of research). Against the physicalism of Neurath and Carnap, I (1937–38b) insisted that generalizations were not guesses or inductions but mainly disguised programs. (See also *Wie fördert man heute die empirische Bewegung. Eine Auseinandersetzung mit dem Empirismus von Otto Neurath und Rudolph Carnap*, written in 1937–39, but published as late as 1956, by Universitetsforlaget, Oslo.) The “radical empiricism” I fought for was to replace physicalism (“Zeit- und Raumangaben schützen nicht vor Anti-Empirismus”) and consisted only of a superprogram of holding all conceivable approaches to all conceivable problems “open.” In this light I conceived the *Encyclopedia of Unified Science*, and when early in the game I was asked to write on psychology in its second volume, the pluralism of psychologies of the 1930s was to occupy a central place. It was later decided that the eminent psychologist Egon Brunswik, who was closely associated with the Vienna Circle, should work together with me, but as he was a believer in his own sophisticated approach within psychology, collaboration was difficult. So, in 1945, I resigned as an author of the volume. The comparison of theories condensed on page 31 in the present work is taken from a chapter in my “Encyclopedia” manuscript, some chapters of which were later published (Naess 1948).

APPENDIX: HISTORICAL NOTE ON POSSIBILISTIC PLURALISM

Thanks to amicable relations with both C. L. Hull and E. C. Tolman, I was able to alternate between their research centers, Yale and Berkeley, and study intimately their mutual “refutations” and their competing research programs. Such metastudies, with strong ingredients of “sociology of knowledge,” inevitably suggest a pluralism: some theories are good for some things, and no theory is good for all things—for “all” psychological problems. The same was suggested by my own experiments within rat psychology at Berkeley in 1938–39.

Under the influence of Carnap, Popper, and others, I finally gave up my pragmatism and instrumental bias (really an attempt to use biology as a fundamental science), and also my empiricism. That is, I made the transition from scientism to philosophy, from theory of (scientific) models to theory of philosophical systems, using Spinoza’s as a paradigm. After all, there are wider *possibilities* of understanding than those that can be stated as research programs! More specifically, I see the transition from belief in empiricism to belief in the possibility of valid, but mutually inconsistent or incomparable, wide systematization, in terms of a rejection of the theory-observation asymmetry: that theories are tested by observation rather than vice versa. The realist, not instrumentalist, view of models, such as the atomic, implies that theoretical possibilities are cognitively on par with any kind of observation sentences, and these in turn are never “pure,” but, as far as they are understandable at all, are so in terms of a theoretical framework. They are therefore liable to be rejected from theoretical considerations; that is, observations are tested by theory.

Notes

Chapter I: The Impact of the New Historiography of Science

1. Where in the literature does one find “the neat image” expressed? It would be better to ask “Where does one not find the neat image?” In textbooks, with very few exceptions, such as Gerald Holton (1958). Among prominent philosophers of science, the early works of Popper, Carnap, and Carl Hempel present the neat image at its best.
2. [Editor's note: source not identified.]
3. Authors, among many others, who have recently contributed (and to whom I feel in debt) include: J. Agassi, H. Butterfield, R. S. Cohen, J. B. Conant, A. C. Crombie, E. D. Dijksterhuis, B. Ellis, P. K. Feyerabend, M. Jammer, A. Koyré, T. S. Kuhn, I. Lakatos, L. Laudan, M. Polanyi, R. Popkin, K. R. Popper, and A. Wolf.
4. See Koestler's eminently readable *The Sleepwalkers* (1959).
5. We take “historiography” to be a synonym for “writing of history”—for example, Caesar's crossing of the Rubicon—whereas “historiology” is taken to be a synonym for “doctrine or science about historical writings.” Only the latter is at the metalevel.
6. The word *praxis* is used to denote the integration of practice and cognitive content in the total scientific enterprise.
7. Cf. Hans Gadamer, *Wahrheit und Methode*, p. 281: “Es genügt zu sagen, dass man *anders* versteht, wenn man überhaupt versteht.”
8. Cf. Gadamer, p. 288: “Es macht die geschichtliche Bewegtheit des menschlichen Daseins aus, dass es keine mitwandert.”
9. See Lakatos (1968a: 176). His later discussion (1970) of the same matter does not seem to have resulted in a new conclusion.

Chapter II: Experimental Setup, Rank Dimensions, and Pluralism

1. The term “methodological prediction” is shorthand for “prediction in the wide nonchronological sense used in methodology.” Thus, if a prediction

about tomorrow's weather is worked out from a meteorological theory, it is a methodological prediction even if it takes ten days to work it out. Common sense, not methodology, might insist that a prediction must be formed before the event predicted. When we start calculating, using T , we do not know the next day's weather, and the day after's observations do not affect the continuing calculation during the next nine days.

2. *Relative strength of an assertion* (of a formula in propositional calculus) is defined in my *Symbolisk logikk* (*Symbolic logic*) (1961: 30), as $k/2^n$, where k is the number of "falses" in the truth-table and n the number of variables in the formula. Compared with the calculus of probabilities, it is easily seen that the relative strength of assertion is equal to $1 - s$, where s is the probability of the assertion that emerges if the variables in the formula are replaced by propositions, all with the same probability, 0.5. Let us take the example $[(p \vee q) \supset r]$. The truth-table gives $tftftftt$ and the relative strength of assertion is $3/2^3 = 0.375$. The formula is equivalent to $\sim(p \vee q) \vee r$, which gives the probability $1 - (1 - (0.5 \times 0.5) \times (1 - 0.5)) = 0.625$, when p, q , and r all have the probability 0.5. Thus, $0.375 = 1 - 0.625$. The term "strength of assertion" in the following is sometimes also used in a generalized, intuitive sense.
3. In diagrams of hypothetico-deductive systems *undergoing tests*, I generally place the peripheral initial-condition sentences to the left of the level of the theory formulations and the hypotheses, in a big bag. From this stems the name "the left-hand bag" (*venstresekken*), or "the surplus bag"—used in earlier publications. Considering the largely arbitrary character of the decision—as long as no particular research situation is pictured—the mass of conditions are aptly symbolized by a voluminous dark bag of strange, unsurveyable content.
4. It was the opinion of Duhem (1962: 294) that "Henri Poincaré was the first to proclaim and teach in a formal manner that the physicist could make use, in succession, of as many theories, incompatible among themselves, as he deemed best." It is my impression that, in his lectures on mechanics, Poincaré already asserted that if one "explanation" (explanatory hypothesis) covered a phenomenon (described observationally), an indefinite number of other explanations could be found.
5. Did Popper or Lakatos ever have an unemotional confrontation with psychoanalysts of some standing and training in the philosophy of science? Lakatos maintains that psychoanalysts never answered the question concerning refutation. Were they *asked*? Not only talked down to? If the psychoanalyst does not have to be more specific than Lakatos in answering the crucial question, anyone can offer a satisfactory answer: "I, [of] course, can easily answer the question when I would give up my criterion of demarcation: when another one is proposed which is better according to my metacriterion" (Lakatos MS 1971, published in 1974).

6. For elaboration on Feyerabend's hedonistic proclamation see p. 98.
7. Nagel continues: "A theory is confirmed to the degree that it performs its specific instrumental function. From this point of view, which has been developed with much detail by Dewey, the degree of confirmation for a theory may be interpreted as a mark of its proved effectiveness as an intellectual tool for the purposes for which it has been instituted." It is not clear from the text whether Nagel thinks there is a possibility of introducing a useful measurement of "degrees of confirmation" of this kind.

In the passage quoted there seems to be a confusion of retrospective with prospective evaluation. Let us say a theory already has been brilliantly used to bridge two parts of physics—for example, parts of mechanics with parts of optics—and gets a ++ mark for confirmation. This would tell us very little about the prospects for its future use. It may be a very awkward theory to use in solving certain current problems within mechanics. The fruitfulness of a tree is the potentiality of another basket of fruit.

The multiplicity of kinds of functions and uses makes it difficult to introduce a score—which is better confirmed, a hammer or a saw? And it is certainly difficult to get a score of value for decisions on what to choose in the future.

8. See Feyerabend (1969) on the desirability of working with refuted theories. The paradoxical formulation is due to the use of the Popperian identification of "direct disconfirmation by observation" with "refutation."
9. With reference to the conflict between Copernican and pre-Copernican cosmology at the time of Galileo, Feyerabend (1970b: 293–94) says:

What is needed for a test of the Copernican cosmology is therefore not just a simple-minded and direct comparison of its predictions with what is *seen*, but the interpolation, between the "perfect world" and "our world" of a well-developed meteorology (in the good old sense of the word, as dealing with the things below the moon) and of an equally well-developed science of *physiological optics* dealing both with the subjective (brain) and the objective (light, lenses) aspects of vision, telescopic vision included.

Chapter III: Theory and Theoretical Idea

1. In Carnap's work, *Philosophical Foundations of Physics* (1966), there is nothing to prevent a physical interpretation of all levels.
2. Concerning the latter see, e.g., Feyerabend (1969: 251).
3. These are thought by Thomas S. Kuhn (1962: 32–33) to be attempts to reformulate Newton's theory in an equivalent but logically and aesthetically more satisfying form. The equivalence is perhaps not meant to be equivalence (or even consistency) of logical content, but somehow equivalence of

functions. According to Nagel (1961: 159), the versions of Lagrange and Hamilton are mathematically equivalent to Newton's own "version." Physical equivalence would, however, require identity of physical interpretations.

4. For recent interpretations, see, e.g., the contributions by Brian Ellis and Hilary Putnam in Colodny (1965), and Peter Zinkernagel (1962) for laws of motion as part of precise formulation of the language of things. See also Hanson (1958: chap. 5), where he presents at least nine uses—i.e., meanings—of $F = ma$.
5. For a discussion of theoretical ideas as "point-of-departure formulations" for "precizations," see Naess (1966: chap. 2 [SWAN VII]), and as *Ballungen* (Otto Neurath).
6. Used as a motto by the editors of Kramers's *Collected Scientific Papers* (1956: 1). "Well-defined" I take in a nontechnical sense.
7. See, e.g., Hermann Bondi (1968: 19–24).
8. Carnap offers an interesting example on p. 242 (Maxwell's theory).

Chapter IV: The Unimpressiveness of Impossibilities

1. Sir Oliver Lodge reminded his contemporaries in 1893 of what might happen to the optimist in the following forceful way:

[B]ut I have met educated persons who while they might laugh at the men who refused to look through a telescope lest they should learn something they did not like, yet also themselves commit the very same folly. . . . I am constrained to say this much: Take heed lest some prophet, after having excited your indignation at the follies and bigotry of a bygone generation, does not turn upon you with the sentence "thou art the man."
(Quoted in Holton and Roller 1958: 160)
2. A second reservation: the tenability of the historical generalization does not preclude the tenability of a hypothesis concerning the future. Given favorable conditions for pluralism, it is possible that intolerance, egotistic narrowness, and "unshakeable" convictions will prove less important in mobilizing maximum energy for limited projects.
3. For more about the relativity of a pronouncement of "possible!" to kind and stage of a discussion—the dialectic aspect of possibilism—see Naess (1967).
4. I propose to interpret the exposition of theories as implying pretension of truth, or of the possibility of truth, if nothing is explicitly said to the contrary. That is, even if the "acceptance" of a theory may primarily be intended to be an acceptance of it as an instrument, a rule of inference, a working hypothesis (in a large sense of "hypothesis"), I shall assume that the acceptance also implies that it is taken to be "true or false," and *not* false. The view that a theory is simply true is rare among workers in physics because of its

tremendous “strength of assertion.” An unbroken series of confirmations over the next centuries is viewed as nearly miraculous or a sure sign of some kind of circularity in the testing process. A theory is expected to break down sooner or later.

5. Empirical investigations suggest that even among nonprofessionals, there is a strong tendency to hesitate to call theories true or false, an even stronger tendency than in the case of sentences about future events (Naess 1953a).
6. From a biological and ecological point of view, humans have a built-in need to combat uncertainties that have daily threatened their lives in an inimical Pleistocene environment. The frantic effort to increase levels of certainty is no longer functional, but is sustained by instinctual mechanisms (cf. works of authors quoted in the highly readable R. Ardrey, *African Genesis* [1961]).
7. The suggestion that excessive tolerance may be involved has been made by Wolfgang Yourgrau (1970) and others.
8. “Observations on mental education,” *Experimental Research in Chemistry and Physics* (1959: 480); quoted in Williams (1968: 236).
9. *British Journal for the Philosophy of Science*, vol. 18 (1967: 186) (diagram). I do not consider the terms *positivism* and *metaphysics* helpful in the writings of Polanyi. Too many, too different views are termed *positivistic*.
10. The existence of a single $P-P^*$ relation is already a sufficient condition for calling the theories incommensurable. The more comprehensive the changes in conceptual matters, the more the changes affect the frameworks and thereby produce indefinability of the one theory in terms of the other.
11. *Existentialismus und Marxismus: Eine Kontroverse zwischen Sartre, Garaudy, Hypolite, Vigier und Orel. Mit einem Beitrag von Alfred Schmidt* (1965: 65–85).
12. These five points are formulated in close agreement with points made by Wartofsky (1967: 138 ff.) and attributed to Popper.
13. Agassi (1964) speaks about science as interpretations of metaphysical views. One may also conceive of philosophical propositions as derived from interpretations of scientific ones. What is the ontological status of electrons?
14. Letter 13 (in the posthumous works, letter 11) to Oldenburg (1663), translated by A. Wolf (Spinoza 1928).
15. This point is elaborated in Naess (1948). The characteristics of “near-total systems” are elaborated in Naess (1969).

Chapter V: The New Historiography Applied to Itself: General Possibilism

1. For historical trends influencing the conception of Plato’s *Parmenides*, see, for example, Knud Johansen (1964: 230, 277).
2. Cf. Ingemar Düring, *Aristoteles* (1966).
3. Ernst Bernheim’s (1914) survey of philosophies of history is still one of the

most informative, even if, of course, his own perspective colors his classifications and characterizations. See section 5 of chapter 5 in his monumental *Lehrbuch der historischen Methode*. A useful modern reader, Joseph Kockelmann's *Philosophy of Science: The Historical Background* (1968), contains selections from Kant, Herschel, Whewell, J. S. Mill, von Helmholtz, Boltzmann, and others.

4. "[M]ost scientists tend to understand little more *about* science than fish about hydrodynamics" (Lakatos 1970: 145).
5. Fresh material has been gathered by R. Rosenthal and associates. See, for example, Rosenthal and Fode (1963).
6. Perhaps not surprisingly, Feyerabend seems to see himself as the old Nordic god Thor using "the hammer of history" to smash false images of science in thunder and lightning. His source of authority—"actual scientific practice." Proliferation at the metalevel is not consistent with his belief that one can show "the tremendous abyss that exists between a certain philosophical picture of science and the real thing" (Feyerabend 1970b: 277).
7. For detailed argumentation, see Naess (1965 [in SWAN IX]).
8. It is heartening to see a thinker from the continent, Karl O. Apel (1962), elaborate this theme: "Kann es ein wissenschaftliches 'Weltbild' überhaupt geben. . . ."
9. The idea is discussed in Naess (1967).
10. Against this, one could say with Horkheimer and Adorno (1969: 4): "Die falsche Klarheit ist nur anderer Ausdruck für den Mythos." See also Feyerabend (1967b).

Appendix: Historical Note on Possibilistic Pluralism

1. [Editor's note: *Ballungen* are literally "agglomerations." They are imprecise and complex terms or notions ("verbal clusters") that are built from more simple terms or statements, but they cannot merely be replaced by the sum of these more precise terms or statements. For a definitive discussion of Neurath's *Ballungen* see Cartwright (1996: 81, 190–93, 208–36).]

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Author's Introduction to the Series

At ninety-two it is a great honor to be still alive and to witness the publication of my selected works in English. Few philosophers have their work published in a series, fewer still receive this honor before they die. When I was originally approached with the idea of publishing my complete works, I was overwhelmed and overjoyed, but added that not all my books and articles were important enough to merit such an honor. Selected works? Yes, and I am extremely grateful for this initiative and the final result, which presents a representative selection of my work from the earliest to the most recent. [*The Selected Works of Arne Naess* are hereafter referred to as SWAN.]

My interest in philosophy began with Spinoza's *Ethics*, which as a seventeen-year-old I was fortunate to read in Latin. I appreciated Spinoza's grand vision and trusted him implicitly as a person. I accepted that human beings could, and should, have a general outlook with the grandeur of Spinoza's, but I recognized that our individual views on this grand scale will not be identical. Through the years I have realized that there is a splendid variety of interpretations of Spinoza (SWAN VI and IX). His texts are exceptionally rich. As the years have gone on, I have focused on how he leads us to realize we can increase our freedom and sense of connection with the world through strengthening and intensifying our *positive* emotions. For example, loving and caring for our place and others leads to an expansive sense of being part of a much larger world. Emphasizing hatred and anger, on the other hand, makes us feel smaller and isolated from the world. Spinoza, as I interpret him, would express this by saying that "We are as large as our love." Increasing our freedom as human beings leads us toward life in communities colored by friendship, sharing joy and sorrow.

Before I left gymnasium [the end of secondary education] the headmaster asked me, "What do you intend to be?" My immediate answer was "A philosopher." In fact, I had already conceived of myself as one. I viewed the

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writings of many contemporary philosophers that I was familiar with, however, as vague and airy and certainly not as inspiring as Spinoza.

My doctoral thesis in philosophy of science was an effort to remind us that in science the content of a theory is not independent of research behavior—the activities of observing, confirming, disconfirming, and so on, and that these are set within a deep context of place, history, and culture. Later, as a postdoctoral researcher at the University of California at Berkeley, I studied the behavior of experimental psychologists doing animal research.

In 1934 and 1935 I studied in Vienna and while there became a member of the famous Schlick seminar, the main discussion group of the Vienna Circle. Their quest for clarity and cordial cooperation in pursuit of knowledge led me to appreciate that “What do I mean?” is an open question. I concluded that we never intend to express anything extremely definite, even in mathematics or symbolic logic. I saw the importance of using empirical methods to find out how we actually use certain expressions and sentences. I developed and applied a wide variety of such methods, which became part of the core for the empirical semantics that runs through my work. I continued to do this type of research into the 1990s, my last project being one in which I questioned experts and policy makers about their ideas of values intrinsic to the natural world (in SWAN X).

In one of my earlier studies, I reviewed about 700 articles from philosophers concerning their use of the word *truth*. For the most part, I found these unconvincing and soon started on empirical studies of the use of *truth* among ordinary nonprofessional people and schoolchildren (in SWAN VIII). Many philosophers seemed to assume that ordinary people hold very naive views about these deep matters. I found through research that, on the contrary, the views articulated by these “ordinary” people were every bit as sophisticated as those held by professional philosophers. This reinforced my conviction that, generally, we greatly underestimate ourselves. Much academic philosophy was narrowly focused and abstract. Philosophers who elicited interest in wide-ranging issues of practical and global importance, such as nonviolence and social justice, have in my lifetime said things that were considered creative, but often too far out. In spite of consistent proclamations that science neither would nor could take over all the problems discussed by philosophers, I tried to argue in ways that reminded readers of science done as open inquiry, and I tried to emphasize that it is occasionally

necessary to perform empirical research to illuminate or support a philosophical viewpoint.

My empirical and historical research led me to realize that there are no certainties and that there is a great diversity in our spontaneous experience as well as endless ways to describe and appreciate the complexities and values of the world. Thus, I realized that I am one of those lifetime seekers that the ancient Greeks called a *zetetic* (see SWAN II and VIII). From my research on scepticism and the foundations of science and logic, it became clear to me that pluralism (every event has many descriptions and possible outcomes), possibilism (anything can happen), and a healthy scepticism (always seeking the truth but never claiming it) make up the most consistent approach to respecting the perspectives and experiences of others, human and nonhuman.

From my empirical studies of semantics, and from my knowledge of several languages, I came to appreciate the complexity of communication. Being committed to Gandhian nonviolent communication, I saw the importance of avoiding dogmatism and fanaticism. One of the most important discoveries coming from this research, leading to the publication of my major book, *Interpretation and Preciseness* (SWAN I), was the insight that we cannot avoid values in any field of endeavor or research. There are no value-free inquiries or theories. Even if we refuse to express our values, this is itself an expression and choice of values. We must, therefore, be clear about our value choices and try to make them explicit. The choices we make, as Spinoza pointed out, shape the quality of our lives, and values emphasizing positive emotions or feelings are expansive and lead to our growth. We must become ever more aware of our choices and the values involved. Even pure logic assumes certain norms. Empirical research can shed light on these matters. My colleagues in philosophy often found my empirical work perplexing. I, in turn, grew to underrate the necessity of visiting great centers of philosophy, as I preferred to be close to or in the mountains.

When I visited the United States, it was mostly to climb in the mountains or walk and camp in the desert. On one fortunate visit, I dropped in at the graduate students' discussion room at Harvard. Speaking with students who were writing their doctoral theses in philosophy, I understood that my knowledge of contemporary philosophy, and of recent important contributions in its various fields, was narrowly limited to special themes of lively personal interest. Even in later years, the tendency to take personal inclina-

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tion very seriously colored my contribution to the philosophical literature. As can be seen, though, from the titles in these *Selected Works*, my strongly felt interests span a rich variety of fields, philosophical traditions, and movements.

Since childhood I have experienced an intense joy in being together with animals and plants and in contemplating the immense evolutionary development of life on earth over millions of years. From an early age I also developed an intense love for mountains and for being in them. Much of my creative philosophical work was done at Tvergastein, my mountain hut in Norway (see SWAN X). My devotion to outdoor life is in the Norwegian tradition called *friluftsliv* (literally, free-air-life). In many respects, I approached philosophical and cross-cultural studies as if I were a field ecologist or naturalist. It was against this background that my work from the 1960s onward focused with close attention on cultural diversity, biodiversity, sustainability, and the deep ecology movement.

My work since the Second World War has been increasingly within movements such as those furthering social justice, peace, and ecological responsibility. During the war, I engaged in anti-Nazi activism, and from that time also in promilitant Gandhianism, a nonviolence that is not pacifist in the usual sense but insists that if it is a bloody fight for justice against injustice, we seek "the center of the conflict" and, if necessary, cooperate with people who use arms. During the Cold War, I participated in the "third side," against both communism and extreme anticommunism, for example, as the scientific leader of a UNESCO project bringing Marxist and anti-Marxist politicians and political science researchers together in an unbiased discussion of the essence of democracy and freedom. Some of the relevant publications are included in SWAN IX.

The broad spectrum of books and articles included in the *Selected Works* represents, in many ways, a chronicle of my passions and influences. The *Selected Works* record, albeit in an inevitably fragmentary way, one possible expression of these. My dream and hope is that some readers will be inspired by their sheer variety, and that young philosophers will be encouraged to let strong personal motivations steer their studies.

Working habits vary. Some people write an article and go on to the next without looking back on the old one; others come back from time to time, radically revising and changing the old one. The latter is my way of working. Lecturing in many places about these subjects, I have found it

natural to revise the old manuscripts until sometimes very little is left of the original. Therefore, I have always viewed my writing as preliminary; a year, five years, ten years after publication of the first editions I have itched to revise, *thoroughly* revise them. When my first book was printed in 1936, I went to watch the hulking presses printing out one page at a time. I was terrified, thinking of mistakes or some awkward sentences being duplicated again and again.

When I was offered the opportunity to have a selected-works series published, I immediately thought I would like to review all my work and ask how, from today's perspective, I might answer the difficult questions I had earlier attempted to probe. Such a task would have been a particularly difficult proposition, because although many of my books and articles contain new ideas, the ideas are often not developed as well as I might have hoped. But alas, I am saved—at my age there is not time for me to accomplish such a comprehensive reevaluation of my work; I do not even have the capacity to do it now in any case.

Who could contemplate undertaking a publishing project of such ambitious proportions? Douglas Tompkins, mountaineer, entrepreneur, protector of wilderness in Chile and Argentina, and creator of the Foundation for Deep Ecology, is such a person. "Miracle Doug," as I call him, likes the idea that the deep ecology slogans and the deep ecology approach were introduced by a philosopher. I am grateful to him for his firm conviction, inspiration, and great generosity. My gratitude, however, extends well beyond my thanks to Doug, to others who have supported and championed this project.

Quincey Imhoff, when executive director of the Foundation for Deep Ecology, supported SWAN with generous grants and other contributions. SWAN has also benefited from faithful assistance and cooperation in the preparation and editing of the manuscripts. The late Professor Ingemund Gullvåg prepared the initial translation of *Which World Is the Real One?* (SWAN III). Professor Alastair Hannay translated the first edition of *Communication and Argument* (SWAN VII) and offered invaluable suggestions for improving the readability of the first editions of *Scepticism* (SWAN II), *The Pluralist and Possibilist Aspect of the Scientific Enterprise* (SWAN IV), and *Gandhi and Group Conflict* (SWAN V).

Most of all, however, I am grateful to Harold Glasser, the series editor, and his assistant, Kim Zetter, who oversaw all aspects of the project from design to production. Glasser's unique combination of intellectual tenacity,

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attention to detail, mastery of my work, and cooperative spirit made him a natural to take on the monumental task of selecting and editing my works. Glasser not only labored to improve the English and clarity of each manuscript, but his keen ability to ferret out countless technical and pedagogical errors has resulted in substantial new editions of volumes II–VII that are both far more comprehensible and accessible than the originals. I thank him for his valiant work on this project, both during his stay in Norway as a visiting Fulbright professor, where we collaborated on a strategy for revising the previously existing material, and in the subsequent years it has taken to complete the project.

From the beginning of the SWAN Project in 1994, Alan Drengson has encouraged and helped to move this work forward in numerous ways. Especially in the last crucial stages of completing volumes I, VIII, IX, and X, his help and editorial oversight have been invaluable. Thanks for his devotion, good humor, and positive enthusiasm. Thanks to both Drengson and Tim Quick for their extensive bibliographic research. Thanks to Bill Devall for his support and encouragement and especially his help on the completion of volume X, *Deep Ecology of Wisdom*. Thanks to Anne Collins for her outstanding work as the copyeditor of the SWAN volumes. Thanks to George Sessions for his support and encouragement.

Last, but certainly not least, immeasurable thanks go to my wife, Kit-Fai Naess, who has worked beside me throughout the years to provide invaluable assistance, encouragement, and inspiration.

Arne Naess

2004

Author's Preface to This Edition

SWAN V is my third book on Gandhi, and, as stated in the subtitle, it explores *satyāgraha* and its “theoretical background.” A volume on the practice of *satyāgraha*, the way of militant nonviolence, was to follow. It was to be written by my old collaborator on nonviolent action, Johan Galtung. However, my friend Johan had other, more pressing themes to write about, such as the Cultural Revolution in China. There was no such volume on nonviolence.

A central theme in Gandhi's philosophy is central to this book. It is perhaps most simply stated in these passages from Gandhi, where he expresses his feelings for the oneness and divine nature of all beings.

I know that you had naturally the art of looking upon trees and animals as friends. I wanted you to extend the idea so as not to feel the want of friends from outside. [There] should be a definite realization that personal friends and relations are no greater friends than strangers of the human family and bird, beast and plant. They are all one, and they are all an expression of God if we would but realize the fact. . . .

(*The Collected Works of Mahatma Gandhi*, vol. 51, p. 57)

There is nothing inanimate for Him. We are of the earth and earthy. . . . I feel nearer God by feeling Him through the earth. . . . [I] rejoice in establishing kinship with not only the lowliest of human beings, but also with the lowest forms of creation whose fate—reduction to dust—I have to share. . . .

(*Ibid.*, vol. 45, p. 80)

Thus, we are most intimately connected with every living creature in the world and with everything that exists; everything depends for its existence on everything else. . . . [Every] obstacle which we place between ourselves and the sky harms us physically, mentally and spiritually.

(*Ibid.*, vol. 49, p. 295; the above three passages are quoted in Power 1991: 100–102)

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There is another central theme in Gandhi's philosophy that needs mentioning: "Fearlessness is indispensable for the growth of the other noble qualities. How can one seek Truth, or cherish Love, without fearlessness?" (Power 1991: 109). "I believe that, where there is only a choice between cowardice and violence, I would advise violence" (ibid., p. 110). I try in a special chapter to interpret in this way the violence expressed in the 1970s by militant blacks against whites. As Gandhi might have said, Yes, there has been timidity among blacks facing arrogant and violent whites. In such cases, violence against whites may sometimes be the only way to overcome fear. Gandhi recognized that sometimes we have to act with determination and force to overcome fear and past anger so that we can move on to a nonviolent way of relating and acting.

The term *self-realization* as used at my Level 1 verbalization of a deep ecology total view is closely related to the term as used by Gandhi. In his doctoral thesis, *Gandhi and Deep Ecology: Experiencing the Non-human Environment*, done at the University of Salford, Shahed Ahmed Power documents in detail the remarkable relations between Gandhian and deep ecology thinking.¹ Only in the last years of his long life did Gandhi seem to admit with regret that sometimes human beings may be forced to kill or even exterminate "dangerous" animals.

Arne Naess

2004

Note

1. So far (2004) this thesis is unpublished, and if published it might be shortened and the relation to deep ecology neglected. The original dissertation can be obtained by writing to Dr. Shahed Ahmed Power at Environmental Resources Unit, University of Salford, Salford, England M5 4WT.

Author's Foreword to the First Edition

Since the 1950s and 1960s, Gandhi's teaching has acquired a new and unexpected significance. Technical and industrial development has put most decisions that touch the individual and his local environment into the hands of specialists. In addition, the division, or rather fragmentation, of labor has proceeded unhampered and has contributed to the unsurveyable character of these developments. Partly as a consequence, the traditional parliamentary procedures in Western democracies have degenerated to the extent that they neither furnish decisions that express "the will of the majority" (no such thing exists in the area of specialist knowledge) nor take sufficient care of vital minority interests. Reconsideration of philosophies of direct action has forced itself on the impatient and underprivileged.

Racial and cultural minorities have made use of violent means or, rather, have lowered restraints against the outbreak of violence. This anti-Gandhian development has, however, underlined the importance of Gandhi's teaching of self-respect—and the feeling of "being something"—as a necessary condition of nonviolence. Outbreaks of violence in ghettos and universities have been followed by despair: "the establishment" has at its command a superior capability to perpetrate violence. Chaos helps the more sinister forces on both sides to increase their power.

In the West, majorities still indulge in the righteous repression of minorities, forgetting that the traditional democratic procedures that were designed to protect minorities can work only imperfectly in technocracies. Gandhi himself reacted against majority rule and the utilitarian precept, "the greatest happiness for the greatest *number*." In his fight for minorities, his motto was "the greatest good for *everyone*," and the kind of fight he led based on direct, nonviolent action, is widely applicable to the problems of the underprivileged.

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The evils of great cities and suburban unlife have made Gandhi's fanatical antimetropolis attitude and his ideals of decentralization (the *panchayat* system) discussible. His tendency to support agriculture and decentralized industry in villages and small towns in order to stop the disastrous flight of peasants to the great cities is quite modern. Gandhi's utopia is one of the few that shows ecological balance, and today his rejection of the Western world's material abundance and waste is accepted by progressives of the ecology movement. A decade ago, Gandhi tended to be denounced as a reactionary dreamer by both Marxist and anti-Marxist economists. Now they are forced to take his conclusions seriously.

Gandhi did not want followers, and we cannot today submit to his leadership. However, we can, and I think must, consider his life and teaching when groping for solutions to our problems.

This book tries to concentrate on central topics of Gandhi's teaching. The religious and philosophical background has been given more space than usual. Our questions are: What has a completely secularized technocracy to offer? How can we replace the vast religious or philosophical sources of energy that have been available in all great societies? In searching for answers, it is important to find out to what extent the religious thought of Gandhi was independent of dogma and myths that today have no chance of being accepted as truths. In this respect, it is not only a question of where Gandhi stands in matters of dogma and myths, but of where the whole world of modern Buddhist and Hindu thinking stands. As will be made clear in what follows, the Gandhian approach is surprisingly free from dogma, and it is even able to accommodate the militant atheist among "believers in God." (He or she must be *militant*, however!)

The studies resulting in the conclusions of this book have been generously supported by the Norwegian Research Council for Science and the Humanities, and have also benefited greatly from a trip to Varanasi, which allowed me to become familiar with contemporary Gandhian groups in the motherland. The small but resourceful group of mostly young people who have tried to apply Gandhi's principles have been a constant source of joy and consolation to me when struggling with the vast theoretical questions of nonviolence. To them I owe a special debt of gratitude.

This book, which was originally planned to be the first volume of a more comprehensive work, will have a sequel by Professor Johan Galtung

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dealing with the practical implementation of Gandhian norms in contemporary societies. Professor Galtung also cooperated with me in the writing of this book. I hope that his now independent work will soon be available because a theoretical background is worth little without a practical foreground.

I would like to thank Alastair Hannay for his generous efforts to improve my English. I am also grateful to the publishers of my book *Gandhi and the Nuclear Age* (1965) for permission to use material from that work. Chapter 1 is taken, with slight alterations and additions, from pages 3–15 and 21–23 of that work; chapter 5 contains material from pages 81–92 and 98–106.

I

Gandhi's Experiments

Gandhi: Merely a Man

We find two diametrically opposed views of Mohandas K. Gandhi's moral stature. One has it that, ethically speaking, he was nearly perfect. Albert Einstein said of him, for instance, that generations to come would scarcely believe that such a man actually walked this earth, and in a collection of essays that appeared under the title *Gandhi Memorial Peace Number* (Roy 1949), a large number of eminent persons accord Gandhi the highest of praise as a moral being. We must also ask ourselves, however, what exactly is the nature of Gandhi's contribution and what is the basis for the tremendous esteem and adulation in which he has been held. For with regard to his own moral achievement, we find a second opinion that is, perhaps, as near the truth as the first: the opinion that Gandhi was often mistaken and that it would be wrong to take him unreservedly as a moral example for everyone.

The best known representative of this latter and more modest view happens to be Gandhi himself. "I claim no infallibility. I am conscious of having made Himalayan blunders . . ." (quoted in Pyarelal 1932: 133; also in Prabhu and Rao 1967: 9). There are other people also who firmly accept that he fell short of his own very high aims. The best collection of Gandhi's teaching, *The Mind of Mahatma Gandhi*, compiled by Ramachandra K. Prabhu and U. R. Rao (1946, revised and enlarged in 1967), opens with two chapters in which Gandhi speaks of his own personal imperfection, his mistakes, their painful consequences, and his unrequited desire for support.

Like many other strong personalities, Gandhi was authoritarian in his family life and, perhaps without clearly perceiving it himself, pressed his wife and eldest son into conformity with his own ideas. Gandhi admitted this and elaborated the point by saying that it was especially in his early life, with its violent changes of lifestyle, that he was not aware of his own

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coercive powers and that he hoped his younger sons did not have as bad a time as his eldest. About his first son, Gandhi says:

His grievance against me has always been that I sacrificed him and his brothers at the altar of what I wrongly believed to be public good. My other sons have laid more or less the same blame at my door, but with a good deal of hesitation, and they have generously forgiven me. My eldest son was the direct victim of my experiments. . . .

(*Harijan* 18.8.1940: 253–54; quoted in Gandhi 1961, vol. 1: 378)

This is not the place to go into details, but the general point is of importance: to stress Gandhi's exemplary moral character runs counter to his own perception and tends to force discussion away from his phenomenal achievements and their relevance today.

Discussion is also diverted from central issues when it is said that strong, nonviolent action presupposes a personal belief in God. Especially among socialists, one finds atheism highly esteemed, and people are mindful of Lenin's implacable fight against religion. What they forget is that Gandhi fought godlessness, not atheism. "You may call yourself an atheist, but so long as you feel akin with mankind you accept God in practice."¹ His prayers were self-directed and not childish begging. He called himself an "orthodox Hindu," but this very orthodoxy he interpreted more widely than anybody else, including under Hinduism the teachings of Jesus and Mohammed and treating Buddha as the great, inestimable reformer of Hinduism. His religiousness was revolutionary, of a sort that if it is crushed, then mankind is lost. "You believe in some principle, clothe it with life. . . . I should think it is enough" (*Harijan* 17.6.1939). The pervasive occurrence of the term *God* in the speeches and writings of Gandhi should not discourage any serious atheist or agnostic. Even a cursory study of the use of this word in religious contexts reveals its pragmatic and performatory, rather than purely descriptive, character.

Many of Gandhi's political opponents have maintained that it was difficult to bring him to the realization that he had made a mistake when he was convinced he had not, but even those criticisms do not detract from the sincerity of Gandhi's own declarations of imperfection. He saw then, as clearly as we can with hindsight, how uninhibited idealizing came to play a fateful role for him. We can see this process by distinguishing three separate phases in his relationships with his supporters. In the first phase the re-

lationship can be expressed as the attitude "We won't manage any better with him, but it would be unwise to shake him off"; in the second, as "Together with him we'll certainly manage"; and in the third, as "He'll manage for us." In the second phase, Gandhi achieved his best work; in the third, his contribution greatly diminished. A great cause, Gandhi reflects, "can only be injured rather than advanced by glorification of its leaders" (*Young India* 13.7.1921; quoted in Prabhu and Rao 1967: 12). His successes in that last stage came increasingly to be attributed to his own high moral attainments. He was already a saint, a demigod. It became all too easy for people to think, "Whatever he says or does can't possibly have much to do with what I can do. I have no great moral ambition nor any special abilities; I can't reasonably be expected to follow his example." By thus becoming clothed in a mystique of remoteness and divinity, Gandhi's words lost the special appeal they had when his prestige had not yet reached its peak.

People came to stress the morality, not the efficiency, of nonviolent campaigns. As a leader of the nation, Jawaharlal Nehru insisted, "I am not a Gandhi," not a man of that lofty character. "Therefore," he seemed to say, "I have to rely on arms and on other means that Gandhi despised."

The case of Albert Schweitzer presents a parallel. Here we have a man of outstanding ability, the recipient of several doctorates, an eminent musician, missionary, theologian, and healer; how possibly could such a paragon inspire ordinary mortals to action? Excellence of this order seems more likely to induce amazement and reverence than friendly cooperation. It is quite different, of course, if the man next door goes off to Africa to start up a small hospital: knowing Jones as we do, we feel there are no real obstacles to our following in his footsteps, should we feel so inspired.

Gandhi was never able, moreover, to make it sufficiently clear to his supporters that a nonviolent army needs soldiers, not just a general. When the crowds pressed in on all sides to touch him, they did not come to listen to what he said or to work with him; they came for comfort. These occasions were among the few when Gandhi lost control and showed anger. ("It is not that I do not get angry. I do not give vent to anger" [*Harijan* 11.5.35; quoted in Prabhu and Rao 1967: 16].) With Gandhi, as with others, reverence for the man himself is a product of the literature that grows up around him, but this literature contains little support for the reverential attitude. The essential picture we get of Gandhi agrees less with that of his famous admirers than with his own.

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I have become literally sick of the adoration of the unthinking multitude. I would feel certain of my ground if I was spat upon by them. Then there would be no need for confession of Himalayan and other miscalculations, no retracing, no re-arranging.

(*Young India* 2.3.1922: 135; quoted in Prabhu and Rao 1967: 11)

To describe Gandhi as a moral genius, however, would not be altogether absurd if what we want to stress is his constructive imagination and uncommon ingenuity in finding and applying morally acceptable forms of political action. In this field, Gandhi was an Edison. He made incredible discoveries in the field of ethics and politics and showed how to apply those discoveries. This, rather than any high moral level of his own conduct, is the truly remarkable feature of his achievement. Personally, of course, he did continually exert himself to maintain a high moral standard, but many people do that without ever making a moral discovery, a contribution to moral thinking and practice.

Especially in the West, Gandhi has been described as an ascetic. This is inadequate. He lived in certain respects like his followers among the poor, but his modest consumption was motivated less by Indian ascetic ideas than by certain views on the healthy life.

The life I am living is entirely very easy and very comfortable, if ease and comfort are a mental state. I have all I need without the slightest care of having to keep any personal treasures. . . . I regard myself as a householder, leading a humble life of service and, in common with my fellow-workers, living upon the charity of friends. . . .

(*Young India* 1.10.1925: 338; quoted in Prabhu and Rao 1967: 4)

He was also influenced by the idea that a person devoted to a great cause cannot afford to use energy for other purposes.²

India has long traditions of ascetic exercises and austerities, the *tapas*. A *saṃnyāsin* is a person who renounces the world, who abandons and resigns worldly affairs and often starves himself in the effort to reach higher levels of religious consciousness. Gandhi explicitly repudiates that he is or ever tried to be a *saṃnyāsin*.

Gandhi earned the title *mahātman*, great soul, primarily because of the effect of his work. To understand this, we must see the difference between external and internal criteria of moral quality. Referring to external criteria, the

goodness of an act is judged relative to its consequences. Referring to internal criteria, on the other hand, the strength of one's will to do good, the inflexibility of one's good intentions, is what counts. In practical life, where it is primarily what people do that matters, we tend to measure moral value in terms of achievement, not intention; and for Gandhi, too, it was what was accomplished that mattered. Thus, when we judge him, we must bear in mind that from the practical viewpoint, great moral achievement need not presuppose a corresponding degree of personal morality.

It Works

Gandhi and his influence can be studied from many points of view. In the following chapters, we shall concentrate on his teaching, in particular on his direct instruction for group conflict. The first question, then, must be, What is most characteristic and highly developed in Gandhi's teaching? Perhaps we are inclined to answer immediately that it was his conviction that the use of violence against living, sensible beings is never morally warranted, that it always infringes valid moral principles. Accordingly, Gandhi's doctrine might be summed up in one commandment: "Thou shalt not use violence." However, such a commandment would be highly misleading. The essential and most important point in Gandhi's doctrine, taken as a whole, is not a principle or a commandment, but the working hypothesis that the nonviolent resolution of group conflict is a practicable goal—despite our own and our opponents' imperfections, nonviolent means are in the long run more effective and reliable than violent ones; and they should be trusted even if they seem unsatisfactory for the moment. He teaches that nonviolence is a practical method that we may, indeed must, adopt immediately and without hesitation in social, political, national, and international conflicts. Here Gandhi is talking to all of us, not mainly to politicians whose power is dependent on the opinions of others.

Understood in this way, the essential and most original aspect of Gandhi's teaching is his descriptive and explanatory account of man and of man's ability to resolve his own conflicts. In the realm of principles and metaphysics, Gandhi shows no remarkable originality.

Any systematic morality must base itself on a number of purely descriptive or causal assumptions, as well as on intuitive, normative notions.

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Indeed, it is often precisely the emphasis it puts on one or the other of these two factors that gives a moral view its distinctive stamp; generally the tendency is for systems to lean toward descriptive and causal characteristics rather than intuitive and normative ones. It is therefore not so remarkable that new working hypotheses and methods covering group action can have immense import morally and normatively. Let us see what this general feature of philosophical systems can tell us about Gandhi.

There is nothing very original in condemning violence; and in any case, Gandhi's condemnation of physical violence is considerably less radical and more qualified than that of many other moralists. The doctrine that violence and coercion against one's fellow humans are indications of moral poverty is to be found in the teachings of prophets, philosophers, and wise men as far back as historical records go. In fact, among the generally acknowledged moral leaders from the time of ancient China and India down to the present, the principle of nonviolence has been the rule, and the condoning of violence, even in defense, the exception. In ancient India, not only was vengeance condemned, but the commandment "Thou shalt not kill" was often extended to all animal life. However, alongside this one often finds pessimistically and fatalistically colored theories of human frailty and of man's inability to adhere strictly to such commandments. Evil nature and ignorance have usually been considered to be so deeply ingrained in man that general use of nonviolence in ordinary political and social struggle is thought to be impracticable, or else the principle of nonviolence has been associated with the view that nonviolent methods, even if the individual could and did employ them, would be ineffective in any significant social and political conflict. In our own culture, influential studies of mass psychology have similarly stressed the impulse to mass violence, even if conceding that, individually, men are peaceable enough.³ All of these theories espousing "it will not work" are clearly anti-Gandhian, for Gandhi's teaching in its essence and originality is the straightforward doctrine that it *will* work and that it can be shown to work. The proof of this for him lay in his own all-important "experiments."

It follows that the focus of our examination of Gandhi's thought must be centered on his view of man and man's possibilities, especially on his faith in the inexhaustible richness of ways of mass action without violence and in the practical possibility of influencing every individual and group

by the example of nonviolent conduct. That this is the correct approach should be clear if we first base our study on, among other things, an examination of the contributions Gandhi made at the time when he was forming his ideas, that is, on a study of his activities in South Africa.

If the originality of Gandhi's teaching lies in his account of what men are constitutionally capable of, what it is in man's power to accomplish, it would nevertheless be misleading to say that his teaching was mainly of a descriptive character; above all, Gandhi stressed man's duties. To UNESCO's inquiry about individual rights, Gandhi replied characteristically that primarily man has no rights, only duties. From the duties, the rights follow like spring follows winter.

Gandhi maintained that the key to his faith in nonviolence lay in his practical experience with men. Since he believed only in the truth of what he was able to test, we are in a position to test his own power of judgment by reviewing and examining as far as possible what his actual experience with men was.

So far, research on Gandhi has neglected to view his activity from this standpoint. Although we cannot attempt any very comprehensive survey here, what we can and will do is to describe briefly some of his experiments.

When the Boer War broke out in 1899, Gandhi, though his "personal sympathies were all with the Boers," felt that if he was to demand rights as a British citizen, it was his duty as such to participate in the defense of the British Empire (Tendulkar 1951, vol. 1: 63). He collected 1,100 comrades to form an ambulance corps. Although opposed initially, Gandhi was eventually able to convince the British to accept their services at the end of 1899. This was done only after great difficulty, however, since the British apparently thought his countrymen unsuited to carrying weapons, unsuited even to carrying stretchers on the battlefield, and hence consigned them to transporting dead and wounded behind the lines. However, Gandhi's men showed unexpected courage and were eventually accorded the "honor" of working in the front line. A few years later, during the so-called Zulu rebellion of 1906, Gandhi himself organized an ambulance corps, which brought him into contact with some Zulus who had been flogged by the British. He and his corps also took care of people who had received burns when the British set fire to villages. Had he chosen to, he could have written an account of his experiences that would have caused

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consternation and horror and have increased the bitterness and hatred that already existed. However, instead of inflaming negative feeling, Gandhi did all he could to improve relations between the British and the Zulus. Thus we see him at this early stage, a courageous and influential man, already looking for positive solutions to problems of bloody conflict.

Another instance is worth mentioning. Indians in South Africa had become embittered by a judgment of the Supreme Court in 1913 that the state was henceforth to recognize Christian marriages only. Indian mothers were thus considered unmarried unless married as Christians. As a consequence, political demonstrations by women, something quite exceptional at the time, broke out. Gandhi succeeded, however, in getting the women actively involved in a wider struggle by persuading them to undertake a long march to the mines; the miners were then persuaded to stop work and to join the protest. Indignation grew to a high pitch when the men's wives were thrown into jail with male criminals, but Gandhi did not exploit the negative feelings. He persuaded the miners that whatever they did, they must avoid the use of violence, and he urged them to recognize that they would best attain their ends by other means. The miners, between two and six thousand of them, marched resolutely into the Transvaal, completely avoiding the use of violence—despite repressive measures taken against them by the police and frustrations caused by privation and hunger.

This march strengthened Gandhi's belief in the ability of the common man to grasp the meaning of nonviolence. The marchers were wholly illiterate; far from belonging to any culturally enlightened section of the community, they were, on the contrary, neither peaceable nor meek by disposition, but oppressed men who, seething with anger, had joined together to oppose repression and the discrimination shown against them.

Another case is even more illuminating. When a violent railway strike broke out that caused the government to declare martial law, Gandhi's own campaign had not been progressing very well. Now, suddenly, the government's own position was endangered, and Gandhi held a very good card. Ordinary political strategy would dictate that he play this card, take full advantage of the situation, and enlarge the immediate goals of his campaign. However, all Gandhi did was to enunciate once again his aims, adding that he had said that his goal, as it was then and as it had been, was to bring about the end of racial discrimination. The railway strike could not help

to persuade the opposition of the justice of his goal; quite the contrary, if Gandhi's forces were now to make use of the difficult position of the government to push through their demands, they should have done so without persuading their opponent of the justice of the Indians' cause.⁴

Gandhi broke off his campaign until the strike ended. In the long history of political strife, this event must surely have few parallels. The impression it made was profound. Gandhi's opponents saw that he and his followers literally meant what they said when they claimed, "This and only this is our aim in this struggle." By not exploiting their advantageous position, Gandhi's supporters remained true to his and their own aim, which was future cooperation with those who were then their opponents. He was able to draw his own conclusions from the effect made by such a plea for moderation.

If Gandhi had written a psychology of the masses, it would, no doubt, have been quite different from those that equate mass man with the aggressive coward, for he had seen with his own eyes how the masses are capable of being led to two extremes—on the one hand, to the most horrifying violence, and on the other, to the most inspiring kind of nonviolence. Gandhi invites us all to continue *experimenting* with nonviolence, and to see for ourselves what can be achieved.

Empirical Basis of Nonviolent Extremism

The combination of courage, sacrifice, and devotion to humanity shown by Gandhi does not significantly distinguish him from many thousands, even millions, of forgotten men who, in the course of history, have shown similar moral qualities. "The world knows so little of how much my so-called greatness depends upon the incessant toil and drudgery of silent, devoted, able and pure workers, men as well as women" (*Young India* 26.4.1928: 130). Perhaps the greatest moral heroes of all time have slipped by unknown and unacclaimed, or at least unrecorded. This may be true, but let us remember that Gandhi's experiments are not mentioned to illustrate any exceptional level of morality. Moral evaluations, at least those speaking of courage and so forth, assume insight into another person's motives, and motives are notoriously elusive. We invite the reader to study Gandhi's "experiments with truth" *within group conflicts* and apart from any opinion about their moral

value. As I have already remarked, it is on the field of practical principle and action that our interest in Gandhi's teaching on conflict must mainly focus, not on the moral quality of his individual acts.⁵

It was Gandhi's claim that the greater the efficiency he acquired in the use of nonviolence, the greater the impression nonviolence made on his opponents. This claim he held to be a legacy of his experiences in South Africa. Was he right in this? Did his claim follow, according to inductive principles, as a valid conclusion from what he observed?

The railway strike episode and others of a similar kind did in fact provide Gandhi with an empirical basis for the hypothesis that the more he applied, even to fanatical extremes, the principle of nonviolence, the greater was its effect, and that every increase, no matter how slight, in the purity of the application of the principle meant an increase in the chances of success. Thus we can see what was meant by Gandhi's seemingly extreme claim that if one man were able to achieve an entirely perfect, nonviolent method, all the opposition in the world would vanish. Yet we must be careful to note that Gandhi explicitly stated that we are all more or less imperfect, not least himself, and that therefore we can talk only in terms of degrees of success and not perfection.

Gandhi, then, had a substantial experimental basis for his claim that the consistent, or pure, forms of *satyāgraha* (strictly, "method of holding on to truth") are more effective than the less consistent, or less pure, and that an increase in consistency or purity is especially favorable when a struggle is already well advanced. He had, in other words, an argument for nonviolence over and above the purely moral one, and this argument is strongly empirical and utilitarian. It may not seem so strange, then, that the versions that Gandhi's opponents gave of the political struggle in South Africa agree with his own, for where nonviolence was at once most consistent and effective, no side suffered from the struggle itself or from its outcome.

Gandhi's campaigns were fought in widely different environments. Only in South Africa were the numbers of people on his and on the other side small enough to make it possible for him to lead the campaigns personally. The opposing groups were sometimes rather rough—the extremely violent Pathans being one of the groups he antagonized but eventually turned into cooperating friends. In South Africa and in India, he met the British Empire's colonial police and jailers. Blood flowed freely, but the high ad-

ministration was eager not to use extreme forms of terror. In the conflicts between various religious groupings, terror was extreme; here Gandhi and his helpers showed how nonviolent methods were able to bring huge riots under control. It has been objected that under Hitler, Gandhi's nonviolence would have been of no avail. However, the Jews could scarcely have suffered more than they did, and it is an open question whether active nonviolent resistance would not have reduced the suffering and number of deaths. No leaders were trying out ways of resistance adapted to the special features of the German situation. From an empirical point of view, it is therefore of little value to discuss the consequences of nonviolent struggles in the Fascist and National Socialist areas.

When judging Gandhi's influence by the standards he himself set for empirical adequacy, we must subject it to the same rigorous critical scrutiny that we apply to any piece of social research. We should, however, also note the enormous complexity of Gandhi's experiments compared with ordinary experiments in, say, social psychology. The number of unknown, or insufficiently known quantities is overwhelming—so much so, in fact, that no conclusions can really claim the title of "scientific." Nevertheless, not all worthwhile research need culminate in well-founded scientific conclusions, nor need the unavoidable uncertainty of a conclusion cause us to reject it.

What then is our verdict to be? Judging from the material available to us, I think we may agree with Gandhi that his approach did in fact work and that the positive results of his action can to a large extent be traced to the nonviolence that characterized his campaign. If we look at nonviolence as a *working hypothesis*, the conclusion that nonviolence can be a vital force in resolving conflicts appears to be a valid inference from the experiments in which Gandhi was involved, however few and scattered these operations were.

Moralism and Pragmatism

Gandhi's writings and speeches are full of moral injunctions and exhortations. He was forever moralizing. That this did not depress those around him seems to be due to his humility, cheerful disposition, and profound sense of humor. "If I had no sense of humour, I should long ago have committed suicide" (*Young India* 18.8.1921: 238). Before that he might have been murdered!

When reading the moralizing speeches, one's first impression is that Gandhi is severely curtailing political action: do *not* do this, do *not* do that. The promise to use only nonviolent means might seem tantamount to caring rather little for the aim and subordinating it to moral exercise. However, there is no known case in fifty years of fighting in which Gandhi states that although an aim could be reached more easily or more thoroughly through some measure of violence, one ought to remain nonviolent. He never gave expression to the view that a *satyāgraha*, or any part of a (genuine) *satyāgraha* was less effective than an alternative involving violence. Thus, every conclusion in the form of a moral prescription had a nonnormative equivalent. That is, the conclusion could be reached from premises of a purely instrumental character, lacking any moral ingredients.

If the reader suspects that this is only a sort of argumentational sleight of hand, it may be because he is unaware of the multiplicity of and different kinds of aims that a given political action is intended to help realize. A plan to murder someone or a plan to support a false rumor has an immediate objective of little or no intrinsic importance. It is the widespread indirect effects of such actions that count. If certain effects of an action obviously occur only through violence, Gandhi turns our attention to other effects that can hardly occur except by nonviolence. This example suggests how Gandhi never *needed* to rely on moral principles but could always argue empirically and pragmatically.

"Would you recommend nonviolence even if the world were such that it could not succeed?" This kind of question Gandhi never answered very clearly, it being in his opinion based on a contrary-to-fact hypothesis.

One may say that Gandhi had, or found himself committed to, a definite political ethics. Having said this, however, one must add, first, that the definiteness had limits: if one tries step by step to make the normative formulations more precise, to eliminate borderline cases, and to cover all kinds of hypothetical cases, it soon becomes clear that the implications of Gandhi's actions and speeches do not solve all dilemmas.

Second, one must add that the commitment to a definite political ethics does not imply the abdication of "the gentle inner voice." The ethical system can at most only codify or systematize past ethical decisions. No new decision follows from it. Each new decision, and each repetition of a kind of decision made earlier, must rest on the free reflection and deliberation of the subject. The system may help to make clear the interconnection

of past decisions, and the implications for the present, if one follows the same rules and uses the same hypotheses.

All of this will become clearer when we discuss definite norms and hypotheses in chapter 3. Here it is our aim only to clarify the nature of our approach to describing that part of Gandhi's political ethics that covers behavior in group conflicts.

Taking Gandhi to be a sender of norms, who is the intended receiver? The primary group of receivers were the listeners to his speeches and the readers of *Young India* and *Harijan*. Much was explicitly aimed at the British, and Gandhi often expressed his wish to reach a world audience, calling attention to what was happening in India. The more special norms can only be understood by describing special situations in India and South Africa, but adequate formulation must be such that the claim to universality is apparent. However dependent upon local circumstances, including the level of nonviolent training of the campaigners, Gandhi, like Hume, Kant, and others, conceived ethical norms as having a universal validity. Under certain sets of circumstances, certain kinds of behavior in conflict are timelessly right, others wrong.

For Gandhi, as for others who act in political life, there are questions of strategy and tactics. Some find a contradiction here. However, the timing of a campaign and all the measures and countermeasures must be chosen in harmony with hypotheses concerning causes and effects. Cleverness is necessary. The norms and hypotheses of nonviolence apply to strategy and tactics, but if Gandhi is right in his hypotheses, they never rule out the use of effective means. No effective tactical moves are ruled out. The norms do not introduce any limitations in that respect.

This point, like many others, reminds us of the fundamental function of *factual hypotheses* in Gandhi's ethics. Sometimes an ethics is conceived as a set of norms without any descriptive components. There is, however, no instance of an ethics that is capable of adequate formulations but contains no ordinary declarative sentences.

In what follows, we make extensive use of Gandhi's own explicit reasonings in the description of his ethics. Therefore, his many norms have unavoidably colored our exposition. Nearly all of them could be eliminated in favor of nonnormative statements, for example, "If one's intention is to reach a certain goal, then this particular behavior is more effective than that behavior." Although sentences of this and similar kinds often do not con-

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form to Gandhi's own way of talking, they can be used to express the bulk of his teaching.

The extensive use of Gandhi's own lucid argumentation does not imply an underestimation of the obscurity and complexity of his imperfectly articulated, or even unconscious, motivations. Erik H. Erikson (1969) has recently delved into this latter realm. His findings confirm that the basic norms and hypotheses of Gandhi had deep psychological roots in his character.

II

The Metaphysics of *Satyāgraha*

Truth

Absence of Theology: Pragmatic and Agnostic Leanings

Any adequate account of Gandhi's ethics and strategy of group conflict must take account not only his most general and abstract metaphysical ideas, but also the religious content of his sermons. His basic ideas and attitudes influenced his concrete norms and hypotheses and his conflict *praxis*. His numerous public prayers were part of his political campaigns, his political campaigns part of his dealings with God.

As mentioned, Gandhi considered himself a Hindu. He gives a condensed characterization of his belief in Hinduism and his relations to other religions in his article "Hinduism" (*Young India* 6.10.1921). Yet, Gandhi found Truth in many religions and faiths, and this explains why his teaching on group conflicts has no definite theological premises. The passage quoted earlier elaborates: "You believe in some principle, clothe it with life, and say it is your God, and you believe in it. . . . I should think it is enough" (*Harijan* 17.6.1939).

Gandhi's view is well within the wide perspective of modern theological movements. According to the religious thinker Paul Tillich, there is a dimension of depth in being¹:

That depth is what the word *God* means. And if that word has not much meaning for you, translate it, and speak of the depths of your life, of the source of your being, of your ultimate concern, of what you take seriously without any reservation. Perhaps, in order to do so, you must forget everything traditional that you have learned about God, perhaps even that word itself.

(Tillich 1948: 63 f.)

Militant atheism in the traditional sense is thus fully compatible with theism in Tillich's sense, perhaps even a necessary condition. Gandhi refused to call the atheist social workers of India "godless." Tillich refuses to call militant traditional atheists "atheists." "He who knows about depth knows about God" (Tillich 1948: 63 f.). Hence, militant atheists know about God; hence, they are not atheists.

Many Hindus denied that Gandhi was an orthodox Hindu and rejected his interpretations of the sacred texts, but he did not give up his universalist tendency for that reason. "If I am a Hindu, I cannot cease to be one even though I may be disowned by the whole of the Hindu population" (*Young India* 29.5.1924: 175; quoted in Prabhu and Rao 1967: 116).

His discussion with the atheist social worker G. Ramachandra Rao ("Gora") gave Gandhi the opportunity to stress the distinction between accepting God in theory and accepting God in practice: "You may call yourself an atheist, but so long as you feel akin with mankind you accept God in practice."²

For Gandhi, it is a necessary and sufficient condition for the truth of the sentences "N. N. believes in God," "N. N. accepts God," "N. N. believes in the existence of God," and "N. N. is not godless" that N. N. lives and acts in certain ways. N. N. may never have used the term *God*, or N. N. may be a militant atheist — these characteristics are not among the decisive ones.

Holding that belief or disbelief in God could only be shown and tested in practice, Gandhi did not take Rao's professed atheism as proof of disbelief in God. Nor would he take professed theism as proof of belief in God.

One may say that Gandhi accepted a pragmatic criterion, an action-oriented criterion, of truth for sentences like "God exists." He has no ontological conception of God such that those who believe that God has or does not have certain properties believe in God and those who believe he has or does not have certain other properties do not believe in God.

As to the certainty of our (intercultural) knowledge about God, Gandhi was largely an agnostic and stressed our limited powers of understanding. "God is the undefinable 'something' that we shall follow but do not know" (*Young India* 5.3.1925). He makes a distinction between God as worshiped and sought and God as the object of our thoughts and reflections. It is the former that counts.

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