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Against Method: Outline of an Anarchistic Theory of Knowledge

What is all this commotion good for? The most it can achieve is to ruin one's peace of mind. There one has one's little rooms. Everything in them is known, has been added, one item after another, has become loved, and well esteemed. Need I fear that the clock will breathe fire into my face or that the bird will emerge from its cage and greedily attack the dog? No. The clock strikes six when it is six like it has been six for three thousand years. This is what I call order. This is what one loves, this is what one can identify with. CARL STERNHEIM, *Die Hose*

Preface

The following essay has been written in the conviction that *anarchism*, while perhaps not the most attractive *political* philosophy, is certainly an excellent foundation for *epistemology*, and for the *philosophy of science*.

The reason is not difficult to find.

"History generally, and the history of revolutions in particular, is always richer in content, more varied, more manysided, more lively and 'subtle' than even" the best historian and the best methodologist can imagine.¹ * "Accidents and conjunctures, and curious juxtapositions of events"² are the very substance of history, and the "complexity of human change and the unpredictable character of the ultimate consequences of any given act or decision of men"³ its most conspicuous feature. Are we really to believe that a bunch of rather naive and simple-minded rules will be capable of explaining such a "maze of interactions"?⁴ And is it not clear that a person who *participates* in a complex process of this kind will succeed only if he is a ruthless *opportunist*, and capable of quickly changing from one method to another?

This is indeed the lesson that has been drawn by intelligent and

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* The notes for this essay begin on p. 94.

thoughtful observers. "From this [character of the historical process]," writes Lenin, continuing the passage just quoted, "follow two very important practical conclusions: first, that in order to fulfill its task, the revolutionary class [i.e., the class of those who want to change either a part of society, such as science, or society as a whole] must be able to master *all* forms and sides of social activity [it must be able to understand, and to apply not only one particular methodology, but any methodology, and any variation thereof it can imagine], without exception; second, [it] must be ready to pass from one to another in the quickest and most unexpected manner." ⁵ "The external conditions," writes Einstein, "which are set for [the scientist] by the facts of experience do not permit him to let himself be too much restricted in the construction of his conceptual world by the adherence to an epistemological system. He therefore must appear to the systematic epistemologist as a type of unscrupulous opportunist . . ." ⁶

The difference between epistemological (political, theological) *theory* and scientific (political, religious) *practice* that emerges from these quotations is usually formulated as a difference between "certain and infallible" (or, at any rate, clear, systematic, and objective) *rules*, or *standards*, and "our fallible and uncertain faculties [which] depart from them and fall into error." ⁷ Science as it should be, third-world science, ⁸ agrees with the proscribed rules. Science as we actually find it in history is a combination of such rules and of error. It follows that the scientist who works in a particular historical situation must learn how to recognize error and how to live with it, always keeping in mind that he himself is liable to add fresh error at any stage of the investigation. He needs a *theory of error* in addition to the "certain and infallible" rules which define the "approach to the truth."

Now error, being an expression of the idiosyncrasies of an individual thinker, observer, even of an individual measuring instrument, *depends* on circumstances, on the particular phenomena or theories one wants to analyze, and it *develops* in highly unexpected ways. *Error is itself a historical phenomenon*. A theory of error will therefore contain rules of thumb, useful hints, heuristic suggestions rather than general laws, and it will relate these hints and these suggestions to historical episodes so that one sees in detail how some of them have led some people to success in some situations. It will develop the imagination of the student without ever providing him with cut-and-dried prescriptions and

procedures. It will be more a collection of stories than a theory in the proper sense and it will contain a sizable amount of aimless gossip from which everyone may choose what fits in with his intentions. Good books on the art of recognizing and avoiding error will have much in common with good books on the art of singing, or boxing, or making love. Such books consider the great variety of character, of vocal (muscular, glandular, emotional) equipment, of personal idiosyncrasies, and they pay attention to the fact that each element of this variety may develop in most unexpected directions (a woman's voice may bloom forth after her first abortion). They contain numerous rules of thumb, useful hints, and they leave it to the reader to choose what fits his case. Clearly the reader will not be able to make the correct choice unless he has already some knowledge of vocal (muscular, emotional) matters and this knowledge he can acquire only by throwing himself into the process of learning and hoping for the best. In the case of singing he must start using his organs, his throat, his brain, his diaphragm, his buttocks before he really knows how to use them, and he must learn from their reactions the way of learning most appropriate to him. And this is true of all learning: choosing a certain way the student, or the "mature scientist," creates a situation as yet unknown to him from which he must learn how best to approach situations of this kind. This is not as paradoxical as it sounds as long as we keep our options open and as long as we refuse to settle for a particular method, including a particular set of rules, without having examined alternatives. "Let people emancipate themselves," says Bakunin, "and they will instruct themselves of their own accord." ⁹ In the case of science the necessary tact can be developed only by *direct participation* (where "participation" means something different for different individuals) or, if such direct participation cannot be had, or seems undesirable, from a study of past episodes in the *history* of the subject. *Considering their great and difficult complexity these episodes must be approached with a novelist's love for character and for detail*, or with a gossip columnist's love for scandal and for surprising turns; they must be approached with insight into the positive function of strength as well as of weakness, of intelligence as well as of stupidity, of love for truth as well as of the will to deceive, of modesty as well as of conceit, rather than with the crude and laughably inadequate instruments of the logician. For nobody can say in abstract terms, without paying attention to idiosyncrasies of person and circumstance, what pre-

cisely it was that led to progress in the past, and nobody can say what moves will succeed in the future.

Now it is of course possible to simplify the historical medium in which a scientist works by simplifying its main actors. The history of science, after all, consists not only of facts and conclusions drawn therefrom. It consists also of ideas, interpretations of facts, problems created by a clash of interpretations, actions of scientists, and so on. On closer analysis we even find that there are no "bare facts" at all but that the facts that enter our knowledge are already viewed in a certain way and are therefore essentially ideational. This being the case the history of science will be as complex, as chaotic, as full of error, and as entertaining as the ideas it contains and these ideas in turn will be as complex, as chaotic, as full of error, and as entertaining as are the minds of those who invented them. Conversely, a little brainwashing will go a long way in making the history of science more simple, more uniform, more dull, more "objective," and more accessible to treatment by "certain and infallible" rules: a theory of errors is superfluous when we are dealing with well-trained scientists who are kept in place by an internal slave master called "professional conscience" and who have been convinced that it is good and rewarding to attain, and then to forever keep, one's "professional integrity."¹⁰

Scientific education as we know it today has precisely this purpose. It has the purpose of carrying out a rationalistic simplification of the process "science" by simplifying its participants. One proceeds as follows. First, a domain of research is defined. Next, the domain is separated from the remainder of history (physics, for example, is separated from metaphysics and from theology) and receives a "logic" of its own.¹¹ A thorough training in such a logic then conditions those working in the domain so that they may not unwittingly disturb the purity (read: the sterility) that has already been achieved. An essential part of the training is the inhibition of intuitions that might lead to a blurring of boundaries. A person's religion, for example, or his metaphysics, or his sense of humor must not have the slightest connection with his scientific activity. His imagination is restrained¹² and even his language will cease to be his own.¹³

It is obvious that such an education, such a cutting up of domains and of consciousness, cannot be easily reconciled with a humanitarian attitude. It is in conflict "with the cultivation of individuality which [alone]

produces, or can produce well developed human beings";¹⁴ it "maim[s] by compression, like a Chinese lady's foot, every part of human nature which stands out prominently, and tends to make a person markedly dissimilar in outline"¹⁵ from the ideal of rationality that happens to be fashionable with the methodologists.

Now it is precisely such an ideal that finds expression either in "certain and infallible rules" or else in standards which separate what is correct, or rational, or reasonable, or "objective" from what is incorrect, or irrational, or unreasonable, or "subjective." Abandoning the ideal as being unworthy of a free man means abandoning standards and relying on theories of error entirely. Only these theories, these hints, these rules of thumb must now be renamed. Without universally enforced standards of truth and rationality we can no longer speak of universal error. We can only speak of what does, or does not, seem appropriate when viewed from a particular and restricted point of view, different views, temperaments, attitudes giving rise to different judgments and different methods of approach. Such an anarchistic epistemology—for this is what our theories of error now turn out to be—is not only a better means for improving knowledge, or of understanding history. It is also more appropriate for a free man to use than are its rigorous and "scientific" alternatives.

We need not fear that the diminished concern for law and order in science and society that is entailed by the use of anarchistic philosophies will lead to chaos. The human nervous system is too well organized for that.¹⁶ Of course, there may arrive an epoch when it becomes necessary to give reason a temporary advantage and when it is wise to defend its rules to the exclusion of everything else. I do not think we are living in such an epoch today.

When we see that we have arrived at the utmost extent of human [understanding] we sit down contented. HUME¹⁷

The more solid, well defined, and splendid the edifice erected by the understanding, the more restless the urge of life . . . to escape from it into freedom. [Appearing as] reason it is negative and dialectical, for it dissolves into nothing the detailed determinations of the understanding. HEGEL¹⁸

Although science taken as whole is a nuisance, one can still learn from it. BENN¹⁹

1. Introduction; The Limits of Argument

The idea of a method that contains firm, unchanging, and absolutely binding principles for conducting the business of science gets into con-

siderable difficulty when confronted with the results of historical research. We find, then, that there is not a single rule, however plausible, and however firmly grounded in epistemology, that is not violated at some time or other. It becomes evident that such violations are not accidental events, they are not the results of insufficient knowledge or of inattention which might have been avoided. On the contrary, we see that they are necessary for progress. Indeed, one of the most striking features of recent discussions in the history and philosophy of science is the realization that developments such as the Copernican Revolutions, or the rise of atomism in antiquity and recently (kinetic theory; dispersion theory; stereochemistry; quantum theory), or the gradual emergence of the wave theory of light occurred either because some thinkers decided not to be bound by certain "obvious" methodological rules or because they unwittingly broke them.²⁰

This liberal practice, I repeat, is not just a fact of the history of science. It is not merely a manifestation of human inconstancy and ignorance. It is reasonable and absolutely necessary for the growth of knowledge. More specifically, the following can be shown: considering any rule, however "fundamental," there are always circumstances when it is advisable not only to ignore the rule, but to adopt its opposite. For example, there are circumstances when it is advisable to introduce, elaborate, and defend ad hoc hypotheses, or hypotheses which contradict well-established and generally accepted experimental results, or hypotheses whose content is smaller than the content of the existing and empirically adequate alternatives, or self-inconsistent hypotheses, and so on.²¹

There are even circumstances—and they occur rather frequently—when argument loses its forward-looking aspect and becomes a hindrance to progress. Nobody wants to assert²² that the teaching of *small children* is exclusively a matter of argument (though argument may enter into it and should enter into it to a larger extent than is customary²³), and almost everyone now agrees that what looks like a result of reason—the mastery of a language, the existence of a richly articulated perceptual world,²⁴ logical ability—is due partly to indoctrination, partly to a process of growth that proceeds with the force of natural law. And where arguments do seem to have an effect this must often be ascribed to their *physical repetition* rather than to their *semantic content*.²⁵ This much having been admitted, we must also concede the possibility of non-argumentative growth in the *adult* as well as in (the theoretical parts of)

institutions such as science, religion, and prostitution. We certainly cannot take it for granted that what is possible for a small child—to acquire new modes of behavior on the slightest provocation, to slide into them without any noticeable effort—is beyond the reach of his elders. One should expect that catastrophic changes of the physical environment, wars, the breakdown of encompassing systems of morality, political revolutions, will transform adult reaction patterns, too, including important patterns of argumentation.²⁶ This may again be an entirely natural process and rational argument may but increase the mental tension that precedes and causes the behavioral outburst.

Now, if there are events, not necessarily arguments, which cause us to adopt new standards, including new and more complex forms of argumentation, will it then not be up to the defenders of the status quo to provide, not just arguments, but also contrary causes? (Virtue without terror is ineffective, says Robespierre.) And if the old forms of argumentation turn out to be too weak a cause, must not these defenders either give up or resort to stronger and more "irrational" means? (It is very difficult, and perhaps entirely impossible, to combat the effects of brainwashing by argument.) Even the most puritanical rationalist will then be forced to stop reasoning and to use, say, *propaganda* and *coercion*, not because some of his reasons have ceased to be valid, but because the *psychological conditions* which make them effective, and capable of influencing others, have disappeared. And what is the use of an argument that leaves people unmoved?²⁷

Of course, the problem never arises quite in this form. The teaching of standards never consists in merely putting them before the mind of the student and making them as clear as possible. The standards are supposed to have maximal *causal efficacy* as well. This makes it very difficult to distinguish between the *logical force* and the *material effect* of an argument. Just as a well-trained pet will obey his master no matter how great the confusion he finds himself in and no matter how urgent the need to adopt new patterns of behavior, in the very same way a well-trained rationalist will obey the mental image of his master, he will conform to the standards of argumentation he has learned, he will adhere to these standards no matter how great the difficulty he finds himself in, and he will be quite unable to discover that what he regards as the "voice of reason" is but a *causal aftereffect* of the training he has received. We see here very clearly how the appeal to "reason" works. At

first sight this appeal seems to be to some ideas which convince a man instead of pushing him. But conviction cannot remain an ethereal state; it is supposed to lead to action. It is supposed to lead to the appropriate action, and it is supposed to sustain this action as long as necessary. What is the force that upholds such a development? It is the causal efficacy of the standards to which appeal was made and this causal efficacy in turn is but an effect of training, as we have seen. It follows that appeal to argument either has no content at all, and can be made to agree with any procedure,²⁸ or else will often have a conservative function: it will set limits to what is about to become a natural way of behavior.²⁹ In the latter case, however, the appeal is nothing but a concealed political maneuver. This becomes very clear when a rationalist wants to restore an earlier point of view. Basing his argument on natural habits of reasoning which either have become extinct or have no point of attack in the new situation, such a champion of "rationality" must first restore the earlier material and psychological conditions. This, however, involves him in "a struggle of interests and forces, not of argument."³⁰

That interests, forces, propaganda, brainwashing techniques play a much greater role in the growth of our knowledge and, a fortiori, of science than is commonly believed can also be seen from an analysis of the relation between idea and action. One often takes it for granted that a clear and distinct understanding of new ideas precedes and should precede any formulation and any institutional expression of them. (An investigation starts with a problem, says Popper.) First, we have an idea, or a problem; then we act, i.e., either speak, or build, or destroy.³¹ This is certainly not the way in which small children develop. They use words, they combine them, they play with them until they grasp a meaning that so far has been beyond their reach. And the initial playful activity is an essential presupposition of the final act of understanding.³² There is no reason why this mechanism should cease to function in the adult. On the contrary, we must expect, for example, that the idea of liberty could be made clear only by means of the very same actions which were supposed to create liberty. Creation of a thing, and creation plus full understanding of a correct idea of the thing, very often are parts of one and the same indivisible process and they cannot be separated without bringing the process to a standstill. The process itself is not guided by a well-defined program; it cannot be guided by such a program for it contains the conditions of the realization of programs. It is rather guided by a

vague urge, by a "passion" (Kierkegaard). The passion gives rise to specific behavior which in turn creates the circumstances and the ideas necessary for analyzing and explaining the whole development, for making it "rational."³³

The development of the Copernican point of view from Galileo up to the twentieth century is a perfect example of the situation we want to describe. We start with a strong belief that runs counter to contemporary reason. The belief spreads and finds support from other beliefs which are equally unreasonable, if not more so (law of inertia; telescope). Research now gets deflected in new directions, new kinds of instruments are built, "evidence" is related to theories in new ways until there arises a new ideology that is rich enough to provide independent arguments for any particular part of it and mobile enough to find such arguments whenever they seem to be required. Today we can say that Galileo was on the right track, for his persistent pursuit of what once seemed to be a silly cosmology created the material needed for the defense of this cosmology against those of us who accept a view only if it is told in a certain way and who trust it only if it contains certain magical phrases, called "observational reports."³⁴ And this is not an exception—it is the normal case: theories become clear and "reasonable" only after incoherent parts of them have been used for a long time. Such unreasonable, nonsensical, unmethodical foreplay thus turns out to be an unavoidable precondition of clarity and of empirical success.³⁵

Trying to describe developments of this kind in a general way, we are of course obliged to appeal to the existing forms of speech which do not take them into account and which must be distorted, misused, and beaten into new patterns in order to fit unforeseen situations (without a constant misuse of language there cannot be any discovery and any progress). "Moreover, since the traditional categories are the gospel of everyday thinking (including ordinary scientific thinking) and of everyday practice, [such an attempt at understanding] in effect presents rules and forms of false thinking and action—false, that is, from the standpoint of [scientific] commonsense."³⁶ This is how *dialectical thinking* arises as a form of thought that "dissolves into nothing the detailed determinations of the understanding."³⁷

It is clear, then, that the idea of a fixed method, or of a fixed (theory of) rationality, arises from too naive a view of man and of his social surroundings. To those who look at the rich material provided by history,

and who are not intent on impoverishing it in order to please their lower instincts, their craving for intellectual security as it is provided, for example, by clarity and precision, to such people it will seem that there is only one principle that can be defended under all circumstances, and in all stages of human development. It is the principle: *anything goes*.³⁸

This abstract principle (which is the one and only principle of our anarchistic methodology) must now be elucidated, and explained in concrete detail.

2. Counterinduction I: Theories

It was said that when considering any rule, however fundamental or "necessary for science," one can imagine circumstances when it is advisable not only to ignore the rule, but to adopt its opposite. Let us apply this claim to the rule that "experience," or "the facts," or "experimental results," or whatever words are being used to describe the "hard" elements of our testing procedures, measure the success of a theory, so that agreement between the theory and "the data" is regarded as favoring the theory (or as leaving the situation unchanged), while disagreement endangers or perhaps even eliminates it. This rule is an essential part of all theories of induction, including even some theories of corroboration. Taking the opposite view, I suggest introducing, elaborating, and propagating hypotheses which are inconsistent either with well-established theories or with well-established facts. Or, as I shall express myself: *I suggest proceeding counterinductively in addition to proceeding inductively*.

There is no need to discuss the first part of the suggestion which favors hypotheses inconsistent with well-established theories. The main argument has already been published elsewhere.³⁹ It may be summarized by saying that evidence that is relevant for the test of a theory T can often be unearthed only with the help of an incompatible alternative theory T'. Thus, the advice to postpone alternatives until the first refutation has occurred means putting the cart before the horse. In this connection, I also advised increasing empirical contents with the help of a *principle of proliferation*: invent and elaborate theories which are inconsistent with the accepted point of view, even if the latter should happen to be highly confirmed and generally accepted. Considering the arguments just summarized, such a principle would seem to be an essential part of any critical empiricism.⁴⁰

The principle of proliferation is also an essential part of a humanitarian

outlook. Progressive educators have always tried to develop the individuality of their pupils, and to bring to fruition the particular and sometimes quite unique talents and beliefs that each child possesses. But such an education very often seemed to be a futile exercise in daydreaming. For is it not necessary to prepare the young for life? Does this not mean that they must learn one *particular set of views* to the exclusion of everything else? And, if there should still remain a trace of their youthful gift of imagination, will it not find its proper application in the arts, that is, in a thin domain of dreams that has but little to do with the world we live in? Will this procedure not finally lead to a split between a hated reality and welcome fantasies, science and the arts, careful description and unrestrained self-expression?⁴¹ The argument for proliferation shows that this need not be the case. It is possible to *retain* what one might call the freedom of artistic creation *and to use it to the full*, not just as a road of escape, but as a necessary means for discovering and perhaps even changing the properties of the world we live in. For me this coincidence of the part (individual man) with the whole (the world we live in), of the purely subjective and arbitrary with the objective and lawful, is one of the most important arguments in favor of a pluralistic methodology.⁴²

3. Philosophical Background: Mill, Hegel

The idea that a pluralistic methodology is necessary both for the advancement of knowledge and for the development of our individuality has been discussed by J. S. Mill in his admirable essay *On Liberty*. This essay, according to Mill, is "a kind of philosophical text book of a single truth, which the changes progressively taking place in modern society tend to bring out into ever stronger relief: the importance, to man and society, of a large variety in types of character, and of giving full freedom to human nature to expand itself in innumerable and conflicting directions."⁴³ Such variety is necessary both for the production of "well-developed human beings" (page 258) and for the improvement of *civilization*. "What has made the European family of nations an improving, instead of a stationary, portion of mankind? Not any superior excellence in them, which, when it exists, exists as the effect, not as the cause, but their remarkable diversity of character and culture. Individuals, classes, nations have been extremely unlike one another: they have struck out a great variety of paths, each leading to something valuable; and although

at every period those who traveled in different paths have been intolerant of one another, and each would have thought it an excellent thing if all the rest would have been compelled to travel his road, their attempts to thwart each other's development have rarely had any permanent success, and each has in time endured to receive the good which the others have offered. Europe is, in my judgment, wholly indebted to this plurality of paths for its progressive and many-sided development" (pages 268–269).⁴⁴ The benefit to the individual derives from the fact that "[t]he human faculties of perception, judgment, discriminative feeling, mental activity, and even moral preference are exercised only in making a choice . . . [t]he mental and moral, like the muscular, powers are improved only by being used. The faculties are called into no exercise by doing a thing merely because others do it, no more than by believing a thing only because others believe it" (page 252). Choice presupposes alternatives between which to choose; it presupposes a society which contains and encourages "different opinions" (page 249), "antagonistic modes of thought,"⁴⁵ as well as "different experiments of living" (page 249), so that the "worth of different modes of life is proved not just in the imagination, but practically" (page 250).⁴⁶ "[U]nity of opinion," however, "unless resulting from the fullest and freest comparison of opposite opinions, is not desirable, and diversity not an evil, but a good . . ." (page 249).

This is how proliferation is introduced by Mill. It is not the result of a detailed epistemological analysis, or, what would be worse, of a linguistic examination of the usage of such words as "to know" and "to have evidence for." Nor is proliferation proposed as a solution to epistemological problems such as Hume's problem, or the problem of the testability of general statements. (The idea that experience might be a basis for our knowledge is at once removed by the remark that "[t]here must be discussion to show how experience is to be interpreted," page 208.) Proliferation is introduced as the solution to a problem of *life*: how can we achieve full consciousness; how can we learn what we are capable of doing; how can we increase our freedom so that we are able to decide, rather than adopt by habit, the manner in which we want to use our talents? Considerations like these were common at a time when the connection between truth and self-expression was still regarded as a problem, and when even the arts were supposed not just to please, but to elevate and to instruct.⁴⁷ Today the only question is how science can improve its own resources, no matter what the human effect of its meth-

ods and of its results. For Mill the connection still exists. Scientific method is part of a general theory of man. It receives its rules from this theory and is built up in accordance with our ideas of a worthwhile human existence.

In addition, pluralism is supposed to lead to the *truth*: ". . . the peculiar evil of silencing the expression of an opinion is that it is robbing the human race, posterity as well as the existing generation—those who dissent from the opinion, still more than those who hold it. If the opinion is right, they are deprived of the opportunity of exchanging error for truth; if wrong, they lose, what is almost as great a benefit, the clearer perception and livelier impression of truth produced by its collision with error" (page 205).⁴⁸ "The beliefs which we have most warrant for have no safeguard to rest on but a standing invitation to the whole to prove them unfounded" (page 209). If "with every opportunity for contesting it [a certain opinion, or a hypothesis] has not been refuted" (page 207), then we can regard it as better than another opinion that has "not gone through a similar process" (page 208).⁴⁹ "If even the Newtonian philosophy were not permitted to be questioned, mankind could not feel as complete assurance of its truth as they now do" (page 209). "So essential is this discipline to a real understanding of moral and human subjects [as well as of natural philosophy—page 208] that, if opponents of all-important truths do not exist, it is indispensable to imagine them and to supply them with the strongest arguments which the most skillful devil's advocate can conjure up" (page 228). There is no harm if such opponents produce positions which sound absurd and eccentric: "Precisely because the tyranny of opinion is such as to make eccentricity a reproach, it is desirable, in order to break through that tyranny, that people should be eccentric" (page 267).⁵⁰ Nor should those who "admit the validity of the arguments for free discussion[s] . . . object to their being 'pushed to an extreme' . . . unless the reasons are good for an extreme case, they are not good for any case" (page 210).⁵¹ Thus methodological and humanitarian arguments are intermixed in every part of Mill's essay,⁵² and it is on *both* grounds that a pluralistic epistemology is defended, for the natural as well as for the social sciences.⁵³

One of the consequences of pluralism and proliferation is that stability of knowledge can no longer be guaranteed. The support a theory receives from observation may be very convincing; its categories and basic principles may appear well founded; the impact of experience itself may be

extremely forceful. Yet there is always the possibility that new forms of thought will arrange matters in a different way and will lead to a transformation even of the most immediate impressions we receive from the world. Considering this possibility, we may say that the long-lasting success of our categories and the omnipresence of a certain point of view is not a sign of excellence or an indication that the truth or part of the truth has at last been found. It is, rather, the indication of a failure of reason to find suitable alternatives which might be used to transcend an accidental intermediate stage of our knowledge. This remark leads to an entirely new attitude toward success and stability.

As far as one can see, the aim of all methodologies is to find principles and facts which, if possible, are not subjected to change. Principles which give the impression of stability are, of course, tested. One tries to refute them, at least in some schools. If all attempts at refutation fail, we have a positive result, nevertheless: we have succeeded in discovering a new stable feature of the world that surrounds us; we have come a step closer to the truth.

Moreover, the process of refutation itself rests on assumptions which are not further investigated. An instrumentalist will assume that there are stable facts, sensations, everyday situations, classical states of affairs, which do not change, not even as the result of the most revolutionary discovery. A "realist" may admit changes of the observational matter, but he will insist on the separation between subject and object and he will try to restore it wherever research seems to have found fault with it.⁵⁴ Believing in an "approach to the truth," he will also have to set limits to the development of concepts. For example, he will have to exclude incommensurable concepts from a series of succeeding theories.⁵⁵ This is the traditional attitude, up to, and including, Popper's critical rationalism.

As opposed to it, the attitude about to be discussed regards any prolonged stability, either of ideas and impressions which are capable of test or of background knowledge which one is not willing to give up (realism; separation of subject and object; commensurability of concepts), as an indication of failure, pure and simple. Any such stability indicates that we have failed to transcend an accidental stage of knowledge, and that we have failed to rise to a higher stage of consciousness and of understanding. It is even questionable whether we can still claim to possess knowledge in such a state. As we become familiar with the existing categories and with the alternatives that are being used in the examination

of the received view, our thinking loses its spontaneity until we are reduced to the "bestial and goggle-eyed contemplation of the world around us."⁵⁶ "The more solid, well defined, and splendid the edifice erected by the understanding, the more restless the urge of life to escape from it into freedom."⁵⁷ Each successful refutation, by opening the way to a new and as yet untried system of categories, temporarily returns to the mind the freedom and spontaneity that is its essential property.⁵⁸ But complete freedom is never achieved. For any change, however dramatic, always leads to a new system of fixed categories. Things, processes, states are still separated from each other. The existence of different elements, of a manifold, is still "exaggerated into an opposition by the understanding."⁵⁹

This "evil manner of reflection,⁶⁰ to always work with fixed categories,"⁶¹ is extended by the customary modes of research to the most widely presupposed and unanalyzed opposition between a subject and an entirely different world of objects.⁶² The following assumptions which are important for a methodological realism have been made in this connection: "the object . . . is something finished and perfect that does not need the slightest amount of thought in order to achieve reality while thought itself is . . . something deficient that needs . . . material for its completion⁶³ and must be soft enough to adapt itself to the material in question."⁶⁴ "If thought and appearance do not completely correspond to each other, one has, to start with, a choice: the one or the other may be at fault. [Scientific empiricism] blames thought for not adequately mirroring experience . . ."⁶⁵ "These are the ideas which form the core of our customary views concerning the relation between subject and object,"⁶⁶ and they are responsible for whatever immobility remains in science, even at a time of crisis.

How can this immobility be overcome? How can we obtain insight into the most fundamental assumptions, not only of science and common sense, but of our existence as thinking beings as well? Insight cannot be obtained as long as the assumptions form an unreflected and unchanging part of our life. But, if they are allowed to change, can we then finish the task of criticizing as identically the same persons who started it? Problems like these are raised not only by the abstract question of criticism, but also by more recent discoveries in anthropology, history of science, and methodology. I shall return to them when I discuss incommensurable theories. For the moment, I would like to indicate, very briefly,

how certain ideas of Hegel can be used to get a tentative first answer, and thus to make a first step in our attempt to reform the sciences.

Science, common sense, and even the refined common sense of critical rationalism use certain fixed categories ('subject'; 'object'; 'reality'), in addition to the many changing views they contain. They are therefore not fully rational. Full rationality can be obtained by extending criticism to the stable parts also. This presupposes the invention of alternative categories and their application to the whole rich material at our disposal. The categories, and all other stable elements of our knowledge, must be set in motion. "Our task is to make fluid the petrified material which we find, and to relight [*wieder entzünden*] the concepts contained in this dead stuff."⁶⁷ We must "dissolve the opposition of a frozen subjectivity and objectivity and comprehend the origin of the intellectual and real world as a becoming, we must understand their being as a product, as a form of producing."⁶⁸ Such dissolving is carried out by reason, which is "the force of the negative absolute, that is, an absolute negation,"⁶⁹ that "annihilates"⁷⁰ science and common sense, and the state of consciousness associated with both. This annihilation is not a conscious act of a scientist who has *decided* to eliminate some basic distinctions in his field. For although he may consciously try to overcome the limitations of a particular stage of knowledge, he may not succeed for want of objective conditions (in his brain, in his social surroundings, in the physical world⁷¹) favoring his wish.⁷² Hegel's general theory of development, his cosmology, as one might call it, gives an account of such conditions.

According to this cosmology, every *object*, every determinate being, is related to everything else: "a well determined being, a finite entity is one that is related to others; it is a content that stands in the relation of necessity to another content and, in the last resort, to the world. Considering this mutual connectedness of the whole, metaphysics could assert . . . the tautology that the removal of a single grain of dust must cause the collapse of the whole universe."⁷³ The relation is not external. Every process, object, state, etc., actually *contains* part of the nature of every other process, object, state, etc.⁷⁴ *Conceptually* this means that the complete description of an object is self-contradictory. It contains elements which say *what the object is*; these are the elements used in the customary accounts provided by science and by common sense which consider part of its properties only and ascribe the rest to the outside.

And the description also contains other elements which say *what the object is not*. These are the elements which science and common sense put outside the object, attributing them to things which are supposed to be completely separated but which are actually contained in the object under consideration. The result is that "all things are beset by an internal contradiction."⁷⁵ This contradiction cannot be eliminated by using different words, for example, by using the terminology of a *process* and its *modifications*. For the process will again have to be separated, at least in thought, from something other than itself; otherwise it is pure being which is in no way different from pure *nothingness*.⁷⁶ It will contain part of what it is separated from, and this part will have to be described by ideas inconsistent with the ideas used for describing the original process, which therefore is bound to contain contradictions also.⁷⁷ Hegel has a marvelous talent for making visible the contradictions which arise when we examine a concept in detail, wishing to give a complete account of the state of affairs it describes. "Concepts which usually appear stable, unmoved, dead are analyzed by him and it becomes evident that they move."⁷⁸

Now we come to a second principle of Hegel's cosmology. The motion of concepts is not merely a motion of the *intellect*, which, starting the analysis with certain determinations, moves away from them and posits their negation. It is an *objective* development as well, and it is caused by the fact that every finite (well-determined, limited) object, process, state, etc., has the tendency to emphasize the elements of the other objects present in it, and to become what it is not. The object, "being restless within its own limit,"⁷⁹ "strives not to be what it is."⁸⁰ "Calling things *finite*, we want to say that they are not merely limited . . . but rather that the negative is essential to their nature and to their being . . . Finite things are, but the truth of their being is their *end*."⁸¹ What is finite does not merely change . . . it passes away; nor is this passing away merely possible, so that the finite thing could continue to be, without passing away; quite the contrary, the being of a finite thing consists in its having in itself the seeds of passing away . . . the hour of its birth is the hour of its death."⁸² "What is finite, therefore, can be set in motion."⁸³

Moving beyond the limit, the object ceases to be what it is and becomes what it is not; it is *negated*. A third principle of Hegel's cosmology is that the result of the negation is "not a mere nothing; it is a spe-

cial content, for . . . it is the negation of a determined and well defined thing.”⁸⁴ Conceptually speaking, we arrive at a “new concept which is higher, richer than the concept that preceded it, for it has been enriched by its negation or opposition, contains it as well as its negation, being the unity of the original concept and of its opposition.”⁸⁵ This is an excellent description, for example, of the transition from the Newtonian conception of space to that of Einstein, *provided* we continue using the *unchanged* Newtonian concept.⁸⁶ “It is clear that no presentation can be regarded as scientific that does not follow the path and simple rhythm of this method, for this is the path pursued by the things themselves.”⁸⁷

Considering that the motion beyond the limit is not arbitrary, but is directed “towards its [i.e., the object’s] end”⁸⁸ it follows that not *all* the aspects of other things which are present in a certain object are realized in the next stage. Negation, accordingly, “does not mean simply saying No, or declaring a thing to be non-existent, or destroying it in any way one may choose . . . Each kind of thing . . . has its own peculiar manner of becoming negated, and in such a way that a development results from it, and the same holds good for each type of ideas and conceptions . . . *This must be learned like everything else.*”⁸⁹ What has to be learned, too, is that the “negation of the negation” does not lead further away from the original starting point but that it returns to it.⁹⁰ This is an “extremely universal and just on that account extremely far-reaching and important law of development in nature, history and thought; a law which . . . asserts itself in the plant and animal world, in geology, in mathematics,⁹¹ in history, in philosophy.”⁹² Thus for example “a grain of barley falling under suitable conditions on suitable soil disappears, is negated, and in its place there arises out of it the plant, the negation of the grain . . . This plant grows, blossoms, bears fruit and finally produces other grains of barley, and as soon as these ripen, the stalk dies, is in turn negated. As a result of this negation of the negation, we again have the grain of barley we started with, not singly, but rather in ten, twenty or thirtyfold number . . . and perhaps even qualitatively improved . . .”⁹³ “It is evident that I say nothing whatever about the particular process of development which, for example, the grain of barley undergoes from its germination up to the dying off of the fruit-bearing plant, when I state that it is the negation of the negation . . . I rather comprise these processes altogether under this one *law of motion* and

just for that reason disregard the peculiarities of each special process. *Dialectics*, however, is nothing else than the science of the general laws of motion and development in nature, human society and thought.”⁹⁴

In the foregoing account, concepts and real things have been treated as separate. Similarities and correspondences were noted: each thing contains elements of everything else, it *develops* by turning into these alien elements, it *changes*, and it finally tries to *return* to itself. The *notion* of each thing, accordingly, contains contradictory elements. It is negated, and it moves in a way corresponding to the movement of the thing. This presentation has one serious disadvantage: “Thought is here described as a mere subjective and formal activity while the world of objects, being situated vis-à-vis thought, is regarded as something fixed and as having independent existence. This dualism . . . is not a true account of things and it is pretty thoughtless to simply take over the said properties of subjectivity and objectivity without asking for their origin . . . Taking a more realistic view we may say that the subject is only a stage in the development of being and essence.”⁹⁵ The concept, too, is then part of the general development of nature, in a materialistic interpretation of Hegel. “Life,” for example, “or organic nature is that phase of nature when the concept appears on the stage; it enters the stage as a blind concept that does not comprehend itself, i.e., does not think.”⁹⁶ Being part of the *natural behavior*, first of an organism, then of a thinking being, it not only mirrors a nature that “lies entirely outside of it,”⁹⁷ it is not merely “something subjective and accidental,”⁹⁸ it is not “merely a concept”;⁹⁹ it participates in the general nature of all things, i.e., it contains an element of everything else, it has the tendency to be the end result of the development of a specific thing, so that, finally, the concept and this thing become one.¹⁰⁰ “That real things do not agree with the idea [“read: with the total knowledge of man”¹⁰¹] constitutes their *finitude*, their *untruth* because of which they are *objects*, each determined in its special sphere by the laws of mechanics, chemistry, or by some external purpose.”¹⁰² In this stage “there can be nothing more detrimental and more unworthy of a philosopher than to point, in an entirely vulgar fashion, to some experience that contradicts the idea . . . When something does not correspond to its concept, it must be led up to it”¹⁰³ (counterinduction!) until “concept and thing have become one.”¹⁰⁴

To sum up: Knowledge is part of nature and is subjected to its gen-

eral laws. The laws of dialectics apply to the motion of objects and concepts, as well as to the motion of higher units comprising objects and concepts. According to these general laws, every object participates in every other object and tries to change into its negation. This process cannot be understood by attending to those elements in our subjectivity which are still in relative isolation and whose internal contradictions are not yet revealed. (Most of the customary concepts of science, mathematics, and especially the rigid categories used by our modern axiomatics are of this kind.) To understand the process of negation we must attend to those other elements which are fluid, about to turn into their opposites, and which may, therefore, bring about knowledge and truth, "the identity of thing and concept."¹⁰⁵ The identity itself cannot be achieved mechanically, i.e., by arresting some aspect of reality and fiddling about with the remaining aspects, or theories, until agreement is achieved (the aspects one wants to arrest, being in motion, will soon be replaced by dogmatic opinions of them, rigid perceptions included). We must rather proceed dialectically, i.e., by an interaction of concept and fact (observation, experiment, basic statement, etc.) that affects both elements. The lesson for methodology is, however, this: Do not work with stable concepts. Do not eliminate counterinduction. Do not be seduced into thinking that you have at last found the correct description of "the facts" when all that has happened is that some new categories have been adapted to some older forms of thought, which are so familiar that we take their outlines to be the outlines of the world itself.

4. Counterinduction II: Experiments, Observations, "Facts"

Considering now the invention, the use, and the elaboration of theories which are inconsistent, not only with other theories, but even with experiments, facts, observations, we may start by pointing out that *not a single theory ever agrees with all the known facts in its domain*. And the trouble is not created by rumors, or by the results of sloppy procedure. It is created by experiments and measurements of the highest precision and reliability.

It will be convenient, at this place, to distinguish two different kinds of disagreement between theory and fact: numerical disagreements and qualitative failures.

The first case is quite familiar: a theory makes a certain numerical prediction and the value that is actually obtained differs from the prediction

made outside the margin of error. Precision instruments are usually involved here. Numerical disagreements abound in science.

Thus the Copernican view at the time of Galileo was inconsistent with facts so plain and obvious that Galileo had to call it "surely false."¹⁰⁶ "There is no limit to my astonishment," he writes in a later work,¹⁰⁷ "when I reflect that Aristarchus and Copernicus were able to make reason so conquer sense that, in defiance of the latter, the former became mistress of their belief." Newton's theory of gravitation was beset, from the very beginning, by a considerable number of difficulties which were serious enough to provide material for refutations. Even today, and in the nonrelativistic domain, there exist "numerous discrepancies between observation and theory."¹⁰⁸ Bohr's atomic model was introduced and retained in the face of very precise and unshakable contrary evidence.¹⁰⁹ The special theory of relativity was retained, despite D. C. Miller's decisive refutation. (I call the refutation "decisive" because the experiment was, from the point of view of contemporary evidence, at least as well performed as the earlier experiment of Michelson and Morley.¹¹⁰) The general theory of relativity, though surprisingly successful in some domains, failed to explain about 10" in the movement of the nodes of Venus and more than 5" in the movement of the perihelion of Mars. All these are quantitative difficulties which can be resolved by discovering a better set of numbers but which do not force us to make qualitative adjustments.

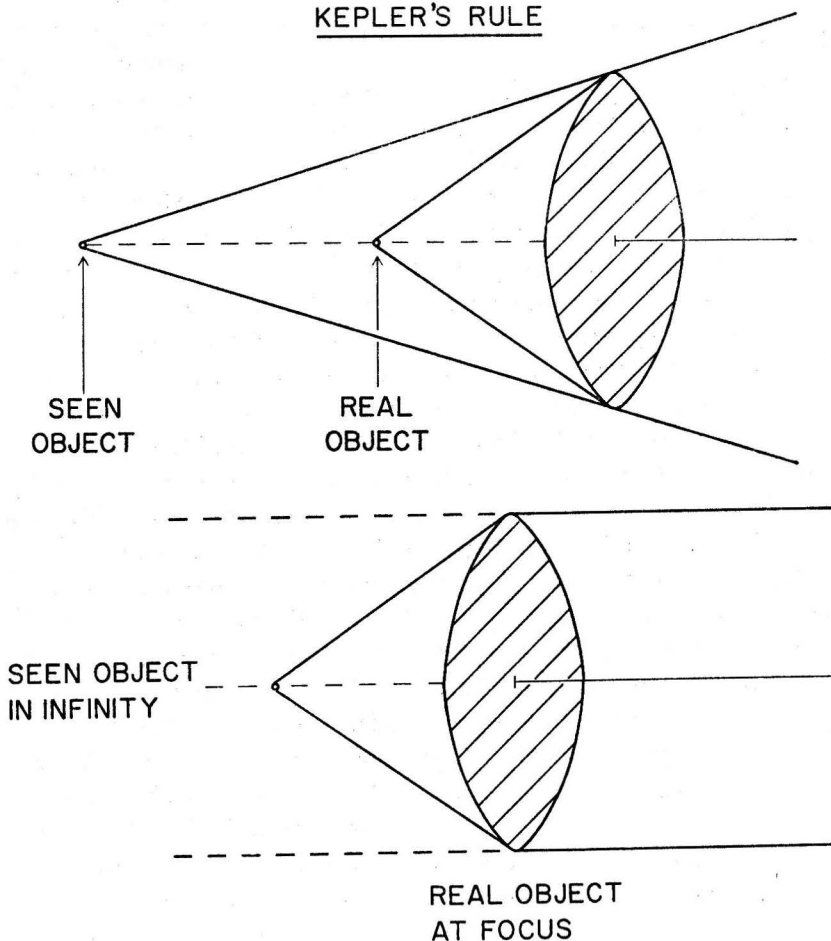
The second case, the case of qualitative failures, is less familiar, but of much greater interest. In this case a theory is inconsistent not with a recondite fact that must be unearthed with the help of complex equipment and is known to experts only, but with circumstances which can be noticed with the unaided senses and which are familiar to everyone.

The first and to my mind the most important example of an inconsistency of this kind is Parmenides' theory of the unchanging One. The theory has much in its favor¹¹¹ and it plays its role even today, for example in the general theory of relativity.¹¹² Used in an undeveloped form by Anaximander it led to the insight, repeated by Heisenberg in his theory of elementary particles,¹¹³ that the basic substance, or the basic elements of the universe, cannot obey the same laws as do the visible elements. Zeno's arguments, on the other hand, show the difficulties inherent in the idea of a continuum consisting of isolated elements. Aristotle took these arguments seriously and developed his own theory of the continuum.¹¹⁴ Yet the idea of a collection of elements remained and continued

to be used, despite the quite obvious difficulties, until these difficulties were removed early in the twentieth century.¹¹⁵

Another example of a theory with qualitative defects is Newton's theory of colors. According to this theory light consists of rays of different refrangibility which can be separated, reunited, refracted, but which are never changed in their internal constitution, and which have a very small lateral extension in space. Considering that the surface of mirrors is much rougher than is the lateral extension of the rays, the ray theory is found to be inconsistent with the existence of mirror images (as is admitted by Newton himself: *Opticks*, book II, part III, proposition viii): if light con-

KEPLER'S RULE



sists of rays, then a mirror should behave like a rough surface, i.e., it should look to us like a wall. Newton retains his theory, eliminating the difficulty with the help of an ad hoc hypothesis: "the reflection of a ray is effected, not by a single point of the reflecting body, but by some power of the body which is evenly diffused all over its surface . . ." ¹¹⁶

In Newton's case the qualitative discrepancy between theory and fact is removed by an ad hoc hypothesis. In other cases not even this very flimsy maneuver is used. One retains the theory and tries to forget its shortcomings. An example is the attitude toward Kepler's rule according to which an object seen through a lens is perceived at the distance at which the rays traveling from the lens toward the eye intersect (see the first diagram).¹¹⁷ The rule implies that an object situated at the focus will be seen infinitely far away (see the second diagram). "But on the contrary," writes Barrow, Newton's teacher and predecessor in Cambridge, commenting on this predication, "we are assured by experience that [a point situated close to the focus] appears variously distant, according to the different situations of the eye . . . And it does almost never seem farther off than it would be if it were beheld with the naked eye; but, on the contrary, it does sometime appear much nearer . . . All which does seem repugnant to our principles. But for me," Barrow continues, "neither this nor any other difficulty shall have so great an influence on me, as to make me renounce that which I know to be manifestly agreeable to reason." ¹¹⁸

Barrow mentions the qualitative difficulties, and he says that he will retain the theory nevertheless. This is not the usual procedure. The usual procedure is to forget about the difficulties, never to talk about them, and to proceed as if the theory were without fault. This attitude is very common today.

Thus classical electrodynamics contains the absurd consequence that the motion of a free particle is self-accelerated.¹¹⁹ This consequence is little known though it makes it impossible to calculate even the simplest case of a motion in a homogeneous electric field. What one does is to make "an approximation" which neglects effects too small to be noticed but which also eliminates the quite noticeable absurd consequence. Theory plus "approximation" produces a reasonable prediction though the theory itself suffers from qualitative difficulties. The quantum theory of fields which one might want to consult in order to remove the troubles of classical electromagnetic theory has absurdities of its own such as

the infinite self energies. The situation is not improved by the remark that these self energies can be corrected by renormalization. They can of course be corrected by this method, and in a consistent manner, but only *after* redefining certain terms in the calculations with an eye to the results to be achieved. This procedure, which is ad hoc, certainly does not establish the excellence of the theory; it shows that as it stands the theory is either refuted¹²⁰ or else woefully incomplete.

Another example of modern physics is quite instructive, for it might have led to an entirely different development of our knowledge concerning the microcosm. Ehrenfest has proved a theorem¹²¹ according to which the classical electron theory of Lorentz taken together with the equipartition principle excludes induced magnetism. The reasoning is exceedingly simple: according to the equipartition principle the probability of a motion is proportional to $\exp[-U/kT]$, where U is the energy of the motion. Now the energy in a constant magnetic field is, according to Lorentz, $= q(E + [vB]) \cdot v$, where q is the charge of the moving parts, E the electric field, B the magnetic field, v the velocity of the moving parts. This magnitude reduces to qE in all cases unless one is prepared to admit the existence of single magnetic poles (given the proper context, this result strongly supports the ideas and the experimental findings of the late Felix Ehrenhaft¹²²).

Occasionally it is impossible to survey all the interesting consequences and to discover the absurd results of a theory. This may be due to a deficiency in the existing mathematical methods; it may also be due to the ignorance of those who defend the theory.¹²³ Under such circumstances the most common procedure is to use an older theory up to a certain point (which is often quite arbitrary) and to add the new theory for calculating refinements. Seen from a methodological point of view the procedure is a veritable nightmare. Let us explain it, using the relativistic calculation of the path of Mercury as an example.

The perihelion of Mercury moves along at a rate of about 5600" per century. Of this value, 5026" are geometric, having to do with the movement of the reference system; 575" are dynamical, due to perturbations in the solar system. Of these perturbations all but the famous 43" are accounted for by classical celestial mechanics. And the remaining 43" are accounted for by general relativity. This is how the situation is usually explained.

Now this explanation shows that the premise from which we derive

the 43" is not the general theory of relativity plus suitable initial conditions. The premise contains classical physics *in addition* to whatever relativistic assumptions are made. Furthermore, the relativistic calculation, the so-called "Schwarzschild solution," does not deal with the planetary system as it exists in the real world (i.e., our own asymmetric galaxy); it deals with the entirely fictional case of a central symmetrical universe containing a singularity in the middle and nothing else. What are the reasons for employing such an insane conjunction of premises?¹²⁴

One reason, so the customary reply continues, is that we are dealing with approximations. The formulas of classical physics do not appear because relativity is incomplete. Nor is the central symmetrical case used because relativity does not offer anything better. Both schemata flow from the general theory under the special circumstances realized in our planetary system *provided* we omit magnitudes too small to be considered. Hence, we are using the theory of relativity throughout, and we are using it in an adequate manner.

Note, now, how this idea of an approximation differs from the legitimate idea: usually one has a theory, one is able to calculate the particular case one is interested in, one notes that this calculation leads to magnitudes below experimental precision, one omits such magnitudes, and one obtains a vastly simplified formalism. In the present case making the required approximations would mean calculating the n-body problem relativistically, omitting magnitudes smaller than the precision of observation reached, and showing that the theory thus curtailed coincides with classical celestial mechanics as corrected by Schwarzschild. This procedure has not been used by anyone simply because the relativistic n-body problem has as yet withstood solution.¹²⁵ There are not even approximate solutions for important problems such as, for example, the problem of stability (a first great stumbling stone for Newton's theory). This being the case, the classical part of the explanans is not only used for convenience, *it is absolutely necessary*. And the approximations made are not a result of relativistic calculation, they are introduced in order to make relativity fit the case. One may properly call them *ad hoc approximations*.

Ad hoc approximations abound in modern mathematical physics. They play a very important part in the quantum theory of fields and they are an essential ingredient of the correspondence principle. At the moment we are not concerned with the reasons for this fact, we are only concerned with its consequences: ad hoc approximations conceal, and even entirely

eliminate, qualitative difficulties. They create a false impression of the excellence of our science. It follows that a philosopher who wants to study the adequacy of science as a picture of the world, or who wants to build up a realistic scientific methodology, must look at modern science with special care. In most cases modern science is more opaque and much more deceptive than its sixteenth- and seventeenth-century ancestors have ever been.

As a final example of qualitative difficulties we mention again the heliocentric theory at the time of Galileo. We shall soon have occasion to show that this theory was inadequate both qualitatively and quantitatively, and that it was also philosophically absurd.

To sum up this brief and very incomplete list: Wherever we look, whenever we have a little patience and select our evidence in an unprejudiced manner, we find that theories fail to adequately reproduce certain quantitative results; and that they are *qualitatively incompetent* to a surprising degree.¹²⁶ Science gives us theories of high beauty and sophistication. Modern science has developed mathematical structures which exceed anything that has existed so far in coherence and generality. But in order to achieve this miracle all the existing troubles had to be pushed into the relation between theory and fact, and had to be concealed, by ad hoc approximations, and by other procedures.

This being the case—what shall we make of the methodological demand that a theory must be judged by experience and must be rejected if it contradicts accepted basic statements? What attitude shall we adopt toward the various theories of confirmation and corroboration which all rest upon the assumption that theories can be made to completely agree with the known facts and which use the amount of agreement reached as a principle of evaluation? This demand, these theories, are now all quite useless. They are as useless as a medicine that heals a patient only if he is bacteria free. In practice they are never obeyed by anyone. Methodologists may point to the importance of falsifications—but they blithely use falsified theories; they may sermonize how important it is to consider all the relevant evidence, and never mention those big and drastic facts which show that the theories which they admire and accept, the theory of relativity, the quantum theory, are at least as badly off as the older theories which they reject. In practice methodologists slavishly repeat the most recent pronouncements of the top dogs in physics, though in doing

so they must violate some very basic rules of their trade. Is it possible to proceed in a more reasonable manner? Let us see!

According to Hume theories cannot be *derived from facts*. The demand to admit only those theories which follow from facts leaves us without any theory. Hence, a science as we know it can exist only if we drop the demand and revise our methodology.

According to our present results hardly any theory is consistent with the facts. The demand to admit only those theories which are consistent with the available and accepted facts again leaves us without any theory. (I repeat: *without any theory*, for there is not a single theory that is not in some trouble or other.) Hence, a science as we know it can exist only if we drop this demand also and again revise our methodology, *now admitting counterinduction in addition to admitting unsupported hypotheses*. The right method no longer consists of rules which permit us to choose between theories on the basis of falsifications. It must rather be modified so as to enable us to choose between theories which we have already tested and which are falsified.

To proceed further. Not only are facts and theories in constant disharmony, they are not even as neatly separated as everyone makes them out to be. Methodological rules speak of “theories” and “observations” and “experimental results” as if these were clear and well-defined objects whose properties are easy to evaluate and which are understood in the same sense by all scientists.

However, the material which a scientist *actually* has at his disposal, his laws, his experimental results, his mathematical techniques, his epistemological prejudices, his attitude toward the absurd consequences of the theories which he accepts, is indeterminate in many ways, it is ambiguous, and *never fully separated from the historical background*. This material is always contaminated by principles which he does not know and which, if known, would be extremely hard to test. Questionable views on cognition, such as the view that our senses, used in normal circumstances, give reliable information about the world, may invade the observation language itself, constituting the observational terms and the distinction between veridical and illusory appearances. As a result observation languages may become tied to older layers of speculation which affect, in this roundabout fashion, even the most progressive methodology. (Example: the absolute space-time frame of classical physics which was codified and consecrated by Kant.) The sensory impression, however simple,

always contains a component that expresses the reaction of the perceiving subject and has no objective correlate. This subjective component often merges with the rest, and forms an unstructured whole which must then be subdivided from the outside with the help of counterinductive procedures. (An example of this is the appearance of a fixed star to the naked eye, which contains the subjective effects of irradiation, diffraction, diffusion, restricted by the lateral inhibition of adjacent elements of the retina.) Finally, there are the auxiliary premises which are needed for the derivation of testable conclusions, and which occasionally form entire auxiliary sciences.

Consider the case of the Copernican hypothesis, whose invention, defense, and partial vindication run counter to almost every methodological rule one might care to think of today. The auxiliary sciences here contained laws describing the properties and the influence of the terrestrial atmosphere (meteorology); optical laws dealing with the structure of the eye and telescopes, and with the behavior of light; and dynamical laws describing motion in moving systems. Most importantly, however, the auxiliary sciences contained a theory of cognition that postulated a certain simple relation between perceptions and physical objects. Not all these auxiliary disciplines were available in explicit form. Many of them merged with the observation language, and led to the situation described at the beginning of the preceding paragraph.

Consideration of all these circumstances, of observation terms, sensory core, auxiliary sciences, background speculation, suggests that a theory may be inconsistent with the evidence, not because it is not correct, but because the evidence is contaminated. The theory is threatened either because the evidence contains unanalyzed sensations which only partly correspond to external processes, or because it is presented in terms of antiquated views, or because it is evaluated with the help of backward auxiliary subjects. The Copernican theory was in trouble for all these reasons.

It is this *historico-physiologic character of the evidence*,¹²⁷ the fact that it does not merely describe some objective state of affairs, but also expresses some subjective, mythical, and long-forgotten views concerning this state of affairs, that forces us to take a fresh look at methodology. It shows that it would be extremely imprudent to let the evidence judge our theories directly, and without any further ado. A straightforward and unqualified judgment of theories by "facts" is bound to eliminate ideas

simply because they do not fit into the framework of some older cosmology. Taking experimental results and observations for granted and putting the burden of proof on the theory means taking the observational ideology for granted without having ever examined it. (Note that the experimental results are supposed to have been obtained with the greatest possible care. Hence "taking observations, etc., for granted" means "taking them for granted after the most careful examination of their reliability"—for even the most careful examination of an observation statement does not interfere with the concepts in terms of which it is expressed, or with the structure of the sensory expression.)

Now—how can we possibly examine something we are using all the time? How can we criticize the terms in which we habitually express our observations? Let us see!¹²⁸

The first step in our criticism of commonly used concepts is to create a *measure of criticism*, something with which these concepts can be compared. Of course, we shall later want to know a little more about the measure stick itself, for example, we shall want to know whether it is better than, or perhaps not as good as, the material examined. But in order for this examination to start there must be a measure stick in the first place. Therefore the first step in our criticism of customary concepts and customary reactions is to step outside the circle and to invent a new conceptual system, a new theory, for example, that clashes with the most carefully established observational results and confounds the most plausible theoretical principles. This step is, again, counterinductive. Counterinduction, therefore, is both a fact—science could not exist without it—and a legitimate and much-needed move in the game of science.

5. The Tower Argument Stated: First Steps of Analysis

As a concrete illustration and as a basis for further discussion, I shall now briefly describe the manner in which Galileo defused an important counterargument against the idea of the motion of the earth. I say "defused," and not "refuted," because we are dealing with a changing conceptual system as well as with certain attempts at concealment.

According to the argument which convinced Tycho, and which is used against the motion of the earth in Galileo's own *Trattato della sfera*, observation shows that "heavy bodies . . . falling down from on high, go by a straight and vertical line to the surface of the earth. This is considered an irrefutable argument for the earth being motionless. For if it

made the diurnal rotation, a tower from whose top a rock was let fall, being carried by the whirling of the earth, would travel many hundreds of yards to the east in the time the rock would consume in its fall, and the rock ought to strike the earth that distance away from the base of the tower." ¹²⁹

In considering the argument, Galileo at once admits the correctness of the sensory content of the observation made, viz. that "heavy bodies . . . falling from a height, go perpendicularly to the surface of the earth." ¹³⁰ Considering an author (Chiaramonti) who sets out to convert Copernicans by repeatedly mentioning this fact, he says: "I wish that this author would not put himself to such trouble trying to have us understand from our senses that this motion of falling bodies is simple straight motion and no other kind, nor get angry and complain because such a clear, obvious, and manifest thing should be called into question. For in this way he hints at believing that to those who say such motion is not straight at all, but rather circular, it seems they see the stone move visibly in an arc, since he calls upon their senses rather than their reason to clarify the effect. This is not the case, Simplicio; for just as I . . . have never seen nor ever expect to see the rock fall any way but perpendicularly, just so do I believe that it appears to the eyes of everyone else. It is therefore better to put aside the appearance, on which we all agree, and to use the power of reason either to confirm its reality or to reveal its fallacy." ¹³¹ The correctness of the observation is not in question. What is in question is its "reality" or "fallacy." What is meant by this expression?

The question is answered by an example that occurs in Galileo's next paragraph, and "from which . . . one may learn how easily anyone may be deceived by simple appearances, or let us say by the impressions of one's senses. This event is the appearance to those who travel along a street by night of being followed by the moon, with steps equal to theirs, when they see it go gliding along the eaves of the roofs. There it looks to them just as would a cat really running along the tiles and putting them behind it; an appearance which, if reason did not intervene, would only too obviously deceive the senses."

In this example we are asked to start with a sensory impression and consider a statement that is forcefully suggested by it. (The suggestion is so strong that it has led to entire systems of belief and rituals as becomes clear from a closer study of the lunar aspects of witchcraft and of other religions.) Now "reason intervenes": the statement suggested by the im-

pression is examined, and one considers other statements in its place. The nature of the impression is not changed a bit by this activity. (This is only approximately true; but we can omit for our present purpose the complications arising from the interaction of impression and proposition.) But it enters new observation statements and plays new, better or worse, parts in our knowledge. What are the reasons and the methods which regulate such exchange?

To start with we must become clear about the nature of the total phenomenon: appearance plus statement. There are not two acts, one, noticing a phenomenon, the other, expressing it with the help of the appropriate statement, *but only one*, viz. saying, in a certain observational situation, "the moon is following me," or "the stone is falling straight down." We may of course abstractly subdivide this process into parts, and we may also try to create a situation where statement and phenomenon seem to be psychologically apart and waiting to be related. (This is rather difficult to achieve and is perhaps entirely impossible.¹³²) But under normal circumstances such a division does not occur; describing a familiar situation is, for the speaker, an event in which statement and phenomenon are firmly glued together.

This unity is the result of a process of learning that starts in one's childhood. From our very early days we learn to react to situations with the appropriate responses, linguistic or otherwise. The teaching procedures both *shape* the 'appearance' or the 'phenomenon' and establish a firm *connection* with words, so that finally the phenomena seem to speak for themselves, without outside help or extraneous knowledge. They just are what the associated statements assert them to be. The language they 'speak' is of course influenced by the beliefs of earlier generations which have been held for such a long time that they no longer appear as separate principles, but enter the terms of everyday discourse, and, after the prescribed training, seem to emerge from the things themselves.

Now at this point we may want to compare, in our imagination and quite abstractly, the results of the teaching of different languages incorporating different ideologies. We may even want to consciously change some of these ideologies and adapt them to more 'modern' points of view. It is very difficult to say how this will change our situation, *unless* we make the further assumption that the quality and structure of sensations (perceptions), or at least the quality and structure of those sensations which enter the body of science, are independent of their linguistic expression.

I am very doubtful about even the approximate validity of this assumption which can be refuted by simple examples. And I am sure that we are depriving ourselves of new and surprising discoveries as long as we remain within the limits defined by it. Yet the present essay will remain quite consciously within these limits. (My first task, if I should ever resume writing, would be to explore these limits and to venture beyond them.)

Making the additional simplifying assumption, we can now distinguish between (a) sensations, and (b) those "mental operations which follow so closely upon the senses"¹³³ and are so firmly connected with their reactions that a separation is difficult to achieve. Considering the origin and the effect of such operations, I shall call them *natural interpretations*.

6. Natural Interpretations

In the history of thought, natural interpretations have been regarded either as *a priori* presuppositions of science or else as prejudices which must be removed before any serious examination can proceed. The first view is that of Kant, and, in a very different manner and on the basis of very different talents, that of some contemporary linguistic philosophers. The second view is due to Bacon (who had, however, predecessors, such as the Greek skeptics).

Galileo is one of those rare thinkers who neither wants to forever retain natural interpretations nor wants to altogether eliminate them. Wholesale judgments of this kind are quite alien to his way of thinking. He insists upon *critical discussion* to decide which natural interpretations can be kept and which must be replaced. This is not always clear from his writings. Quite the contrary, the methods of reminiscence, to which he appeals so freely, are designed to create the impression that nothing has changed and that we continue expressing our observations in old and familiar ways. Yet his attitude is relatively easy to ascertain: natural interpretations are necessary. The senses alone, without the help of reason, cannot give us a true account of nature. What is needed for arriving at such a true account are "the . . . senses, accompanied by reasoning."¹³⁴ Moreover, in the arguments dealing with the motion of the earth, it is this reasoning, it is the connotation of the observation terms, and not the message of the senses or the appearance, that causes trouble. "It is therefore better to put aside the appearance, on which we all agree, and to use the power of reason either to confirm [its] reality or to reveal [its]

fallacy."¹³⁵ "To confirm the reality or reveal the fallacy of appearances" means, however, to examine the validity of those natural interpretations which are so intimately connected with the appearances that we no longer regard them as separate assumptions. I now turn to the first natural interpretation implicit in the argument from falling stones.

According to Copernicus the motion of a falling stone should be "mixed straight-and-circular."¹³⁶ By the "motion of the stone" is meant, not just its motion relative to some visible mark in the visual field of the observer, or its *observed motion*, but rather its motion in the solar system, or in (absolute) space, or its *real motion*. The familiar facts appealed to in the argument assert a different kind of motion, a simple vertical motion. This result refutes the Copernican hypothesis only if the concept of motion that occurs in the observation statement is the same as the concept of motion that occurs in the Copernican prediction. The observation statement "the stone is falling straight down" must therefore, likewise refer to a movement in (absolute) space. It must refer to a real motion.

Now, the force of an "argument from observation" derives from the fact that the observation statements it involves are firmly connected with appearances. There is no use appealing to observation if one does not know how to describe what one sees, or if one can offer one's description with hesitation only, as if one had just learned the language in which it is formulated. An observation statement, then, consists of two very different psychological events: (1) a clear and unambiguous *sensation* and (2) a clear and unambiguous *connection* between this sensation and parts of a language. This is the way in which the sensation is made to speak. Do the sensations in the argument above speak the language of real motion?

They speak the language of real motion in the context of seventeenth-century everyday thought. At least this is what Galileo tells us. He tells us that the everyday thinking of the time assumes the "operative" character of *all* motion.¹³⁷ or, to use well-known philosophical terms, it assumes a *naive realism with respect to motion*: except for occasional and unavoidable illusions, apparent motion is identical with real (absolute) motion. Of course, this distinction is not explicitly drawn. One does not first distinguish the apparent motion from the real motion and then connect the two by a correspondence rule. Quite the contrary, one describes, perceives, acts toward the apparent motion as if it were already the real

thing. Nor does one proceed in this manner under all circumstances. It is admitted that objects may move which are not seen to move; and it is also admitted that certain motions are illusory (see the example in section 7 above). Apparent motion and real motion are not always identified. However, there are *paradigmatic* cases in which it is psychologically very difficult, if not plainly impossible, to admit deception. It is from these paradigmatic cases, and not from exceptions, that naive realism with respect to motions derives its strength. These are also the situations in which we first learn our kinematic vocabulary. From our very childhood we learn to react to them with concepts which have naive realism built right into them, and which inextricably connect movement and the appearance of movement. The motion of the stone in the tower argument, or the alleged motion of the earth, is such a paradigmatic case. How could one possibly be unaware of the swift motion of a large bulk of matter such as the earth is supposed to be! How could one possibly be unaware of the fact that the falling stone traces a vastly extended trajectory through space! From the point of view of seventeenth-century thought and language, the argument is, therefore, impeccable and quite forceful. However, notice how *theories* ("operative character" of all motion: essential correctness of sense reports), which are not formulated explicitly, enter the debate in the guise of observational terms. We realize again that observational terms are Trojan horses which must be watched very carefully. How is one supposed to proceed in such a sticky situation?

The argument from falling stones seems to refute the Copernican view. This may be due to an inherent disadvantage of Copernicanism; but it may also be due to the presence of natural interpretations which are in need of improvement. The first task, then, is to discover and to isolate these unexamined obstacles to progress.

It was Bacon's belief that natural interpretations could be discovered by a method of analysis that peels them off, one after another, until the sensory core of every observation is laid bare. This method has serious drawbacks. First, natural interpretations of the kind considered by Bacon are not just added to a previously existing field of sensations. They are instrumental in *constituting* the field, as Bacon says himself. Eliminate all natural interpretations, and you also eliminate the ability to think and to perceive. Second, disregarding this fundamental function of natural interpretations, it should be clear that a person who faces a perceptual field

without a single natural interpretation at his disposal would be *completely disoriented*; he could not even start the business of science. Third, the fact that we do start, even after some Baconian analysis, shows that the analysis has stopped prematurely. It has stopped at precisely those natural interpretations of which we are not aware and without which we cannot proceed. It follows that the intention to start from scratch, after a complete removal of all natural interpretations, is self-defeating.

Furthermore, it is not possible to even *partly* unravel the cluster of natural interpretations. At first sight the task would seem to be simple enough. One takes observation statements, one after the other, and analyzes their content. However, concepts that are hidden in observation statements are not likely to reveal themselves in the more abstract parts of language. If they do, it will still be difficult to nail them down; concepts, just as percepts, are ambiguous and dependent on background. Moreover, the content of a concept is determined also by the way in which it is related to perception. Yet how can this way be discovered without circularity? Perceptions must be identified, and the identifying mechanism will contain some of the very same elements which govern the use of the concept to be investigated. We never penetrate this concept completely, for we always use part of it in the attempt to find its constituents.¹⁸⁸ There is only one way to get out of this circle, and it consists in using an *external measure of comparison*, including new ways of relating concepts and percepts. Removed from the domain of natural discourse and from all those principles, habits, and attitudes which constitute its form of life, such an external measure will look strange indeed. This, however, is not an argument against its use. Quite the contrary, such an impression of strangeness reveals that natural interpretations are at work, and it is a first step toward their discovery. Let us explain this situation with the help of the tower example.

The example is intended to show that the Copernican view is not in accordance with 'the facts.' Seen from the point of view of these 'facts,' the idea of the motion of the earth appears to be outlandish, absurd, and obviously false, to mention only some of the expressions which were frequently used at the time, and which are still heard wherever professional squares confront a new and counterfactual theory. This makes us suspect that the Copernican view is an external measuring rod of precisely the kind described above.

We now can turn the argument around and use it as a *detecting device*

that helps us to discover the natural interpretations that exclude the motion of the earth. Turning the argument around, we *first assert* the motion of the earth and *then inquire* what changes will remove the contradiction. Such an inquiry may take considerable time, and there is a good sense in which one can say that it is not yet finished, not even today. The contradiction, therefore, may stay with us for decades or even centuries. Still, *it must be upheld* (Hegel!) until we have finished our examination or else the examination, the attempt to discover the antediluvian components of our knowledge, cannot even start. This, we have seen, is one of the reasons one can give for *retaining*, and, perhaps, even for *inventing*, theories which are inconsistent with the facts: Ideological ingredients of our knowledge and, more especially, of our observations, are discovered with the help of theories which are refuted by them. *They are discovered counter-inductively.*

Let me repeat what has been asserted so far. Theories are tested and possibly refuted by facts. Facts contain ideological components, older views which have vanished from sight or were perhaps never formulated in an explicit manner. These components are highly suspicious, first, because of their age, because of their antediluvian origin; second, because their very nature protects them from a critical examination and always has protected them from such an examination. Considering a contradiction between a new and interesting theory and a collection of firmly established facts, the best procedure is, therefore, not to abandon the theory but to use it for the discovery of the hidden principles that are responsible for the contradiction. Counterinduction is an essential part of such a process of discovery. (Excellent historical example: the arguments against motion and atomicity of Parmenides and Zeno. Diogenes of Sinope, the Cynic, took the simple course that would be taken by many contemporary scientists and all contemporary philosophers: he refuted the arguments by rising and walking up and down. The opposite course, recommended here, led to much more interesting results, as is witnessed by the history of the case. One should not be too hard on Diogenes, however, for it is also reported that he beat a pupil who was content with his refutation, exclaiming that he had given reasons which the pupil should not accept without additional reasons of his own.¹³⁹)

Having discovered a particular natural interpretation, the next question is how it is to be *examined* and *tested*. Obviously, we cannot proceed in the usual way, i.e., derive predictions and compare them with "results of

observation." These results are no longer available. The idea that the senses, employed under normal circumstances, produce correct reports of real events, for example reports of the real motion of physical bodies, has now been removed from all observational statements. (Remember that this notion was found to be an essential part of the anti-Copernican argument.) But without it our sensory reactions cease to be relevant for tests. This conclusion has been generalized by some rationalists, who decided to build their science on reason only and ascribed to observation a quite insignificant auxiliary function. Galileo does not adopt this procedure.

If one natural interpretation causes trouble for an attractive view, and if its *elimination* removes the view from the domain of observation, then the only acceptable procedure is to use *other* interpretations and to see what happens. The interpretation which Galileo uses restores the senses to their position as instruments of exploration, *but only with respect to the reality of relative motion*. Motion "among things which share it in common" is "nonoperative," that is, "it remains insensible, imperceptible, and without any effect whatever."¹⁴⁰ Galileo's first step in the joint examination of the Copernican doctrine, and of a familiar but hidden natural interpretation, consists therefore in *replacing the latter by a different interpretation*, or, considering the function of natural interpretations, *he introduces a new observation language*.

This is, of course, an entirely legitimate move. In general, the observation language which enters an argument has been in use for a long time and is quite familiar. Considering the structure of common idioms on the one hand, and of the Aristotelian philosophy on the other, neither this use nor the familiarity can be regarded as a test of the underlying principles. These principles, these natural interpretations, occur in every description. Extraordinary cases which might create difficulties are defused with the help of "adjuster words,"¹⁴¹ such as "like" or "analogous," which divert them so that the basic ontology remains unchallenged. A test is, however, urgently needed. It is needed especially in those cases where the principles seem to threaten a new theory. It is then quite reasonable to introduce alternative observation languages and to compare them both with the original idiom and with the theory under examination. Proceeding in this way, we must make sure that the comparison is *fair*. That is, we must not criticize an idiom that is supposed to function as an observation language because it is not yet well known and is therefore less strongly connected with our sensory reactions and less plausible than is another

and more "common" idiom. Superficial criticisms of this kind, which have been elevated into an entire new "philosophy," abound in discussions of the mind-body problem. Philosophers who want to introduce and to test new views thus find themselves faced not with *arguments*, which they could most likely answer, but with an impenetrable stone wall of well-entrenched reactions. This is not at all different from the attitude of people ignorant of foreign languages, who feel that a certain color is much better described by "red" than by "rosso." As opposed to such attempts at conversion by appeal to familiarity ("I know what pains are, and I also know, from introspection, that they have nothing whatever to do with material processes!"), we must emphasize that a comparative judgment of observation languages, e.g., materialistic observation languages, phenomenalist observation languages, objective-idealistic observation languages, theological observation languages, can start only *when all of them are spoken equally fluently*.

Let me assert at this point that while it is possible to consider and to actively apply various rules of thumb, and while we may in this way arrive at a satisfactory judgment, it is not at all wise to go further and to turn these rules of thumb into necessary conditions of science. For example, one might be inclined to say, following Neurath, that an observation language A is preferable to an observation language B, if it is at least as useful as B in our everyday life, and if more theories and more comprehensive theories are compatible with it than are compatible with B. Such a criterion takes into account that *both* our perceptions (natural interpretations included) and our theories are fallible, and it also pays attention to our desire for a harmonious and universal point of view. (One always seems to assume that observation languages should be employed not only in laboratories, but also at home, and in the "natural surroundings" of the scientist.) However, we must not forget that we find and improve the assumptions hidden in our observational reports by a method that makes use of inconsistencies. Hence, we might prefer B to A as a starting point of analysis, and we might in this way arrive at a language C which satisfies the criterion even better, but which cannot be reached from A. Conceptual progress like any other kind of progress depends on psychological circumstances, which may prohibit in one case what they encourage in another. Moreover the psychological factors which come into play are never clear in advance. Nor should the demand for practicality and sensory content be regarded as a *conditio sine qua non*. We possess de-

tecting mechanisms whose performance outdistances our senses. Combining such detectors with a computer, we may test a theory directly, without intervention of a human observer. This would eliminate sensations and perceptions from the process of testing. Using hypnosis, one could eliminate them from the transfer of the results into the human brain also, and thus arrive at a science that is completely without experience.¹⁴² Considerations like these, which indicate possible paths of development, should cure us once and for all of the belief that judgments of progress, improvement, etc., are based on rules which can be revealed *now* and will remain in action for all the years to come. My discussion of Galileo has not, therefore, the aim of arriving at the "correct method." It has rather the aim of showing that such a "correct method" does not *and cannot* exist. More especially, it has the limited aim of showing that counterinduction is very often a reasonable move. Let us now proceed a step further in our analysis of Galileo's reasoning!

7. The Tower Argument: Analysis Continued

Galileo replaces one natural interpretation by a very different and as yet (1630!) at least partly unnatural interpretation. How does he proceed? How does he manage to introduce absurd and counterinductive assertions such as the assertion that the earth moves, and how does he manage to get them a just and attentive hearing? One may anticipate that arguments will not suffice—an interesting, and highly important, limitation of rationalism—and Galileo's utterances are indeed arguments in appearance only. For Galileo uses *propaganda*. He uses *psychological tricks* in addition to whatever intellectual reasons he has to offer. These tricks are very successful; they lead him to victory. But they obscure the new attitude toward experience that is in the making, and postpone for centuries the possibility of a reasonable philosophy. They obscure the fact that the experience on which Galileo wants to base the Copernican view is nothing but the result of his own fertile imagination, that it has been *invented*. They obscure this fact by insinuating that the new results which emerge are known and conceded by all, and need only be called to our attention to appear as the most obvious expression of the truth.

Galileo "reminds" us that there are situations in which the nonoperative character of shared motion is just as evident and as firmly believed as the *idea* of the operative character of all motion is in other circumstances (this latter idea is therefore not the only natural interpretation

of motion). The situations are events in a boat, in a smoothly moving carriage, and in any other system that contains an observer and permits him to carry out some simple operations.

Sagredo: There has just occurred to me a certain fantasy which passed through my imagination one day while I was sailing to Aleppo, where I was going as consul for our country . . . If the point of a pen had been on the ship during my whole voyage from Venice to Alexandretta and had had the property of leaving visible marks of its whole trip, what trace—what mark—what line would it have left?

Simplicio: It would have left a line extending from Venice to there; not perfectly straight—or rather, not lying in the perfect arc of a circle—but more or less fluctuating according as the vessel would now and again have rocked. But this bending in some places a yard or two to the right or left, up or down, in length of many hundreds of miles, would have made little alteration in the whole extent of the line. These would scarcely be sensible, and without an error of any moment it could be called part of a perfect arc.

Sagredo: So that if the fluctuation of the waves were taken away and the motion of the vessel were calm and tranquil, the true and precise motion of that pen point would have been an arc of a perfect circle. Now if I had had that same pen continually in my hand, and had moved it only a little sometimes this way or that, what alteration should I have brought into the main extent of this line?

Simplicio: Less than that which would be given to a straight line a thousand yards long which deviated from absolute straightness here and there by a flea's eye.

Sagredo: Then if an artist had begun drawing with that pen on a sheet of paper when he left the port and had continued doing so all the way to Alexandretta, he would have been able to derive from the pen's motion a whole narrative of many figures, completely traced and sketched in thousands of directions, with landscapes, buildings, animals, and other things. Yet the actual, real, essential movement marked by the pen point would have been only a line; long, indeed, but very simple. But as to the artist's own actions, these would have been conducted exactly the same as if the ship had been standing still. The reason that of the pen's long motion no trace would remain except the marks drawn upon the paper is that the gross motion from Venice to Alexandretta was common to the paper, the pen, and everything else in the ship. But the small motions back and forth, to right and left, communicated by the artist's fingers to the pen but not to the paper, and belonging to the former alone, could thereby leave a trace on the paper which remained stationary to those motions.¹⁴³

Or:

Salviati: . . . imagine yourself in a boat with your eyes fixed on a point

of the sail yard. Do you think that because the boat is moving along briskly, you will have to move your eyes in order to keep your vision always on that point of the sail yard and follow its motion?

Simplicio: I am sure that I should not need to make any change at all; not just as to my vision, but if I had aimed a musket I should never have to move it a hairsbreadth to keep it aimed, no matter how the boat moved.

Salviati: And this comes about because the motion which the ship confers upon the sail yard, it confers also upon you and upon your eyes, so that you need not move them a bit in order to gaze at the top of the sail yard, which consequently appears motionless to you. (And the rays of vision go from the eye to the sail yard just as if a cord were tied between the two ends of the boat. Now a hundred cords are tied at different fixed points, each of which keeps its place whether the ship moves or remains still).¹⁴⁴

It is clear that these situations lead to a nonoperative concept of motion even within common sense.

On the other hand, common sense, and I mean seventeenth-century common sense, also contains the idea of the operative character of all motion. This latter idea arises when a limited object that does not contain too many parts moves in vast and stable surroundings, for example, when a camel trots through the desert, or when a stone descends from a tower.

Now, Galileo urges us to "remember" the conditions in which we assert the nonoperative character of shared motion in this case also, and to subsume the second case under the first.

Thus, the first of the two paradigms of nonoperative motion mentioned above is followed by the assertion that "it is likewise true that the earth being moved, the motion of the stone in descending is actually a long stretch of many hundred yards, or even many thousand; and had it been able to mark its course in motionless air or upon some other surface, it would have left a very long slanting line. But that part of all this motion which is common to the rock, the tower, and ourselves remains insensible and as if it did not exist. There remains observable only that part in which neither the tower nor we are participants; in a word, that with which the stone in falling measures the tower."¹⁴⁵

And the second paradigm precedes the exhortation to "transfer this argument to the whirling of the earth and to the rock placed on top of the tower, whose motion you cannot discern because in common with the rock you possess from the earth that motion which is required for following the tower; you do not need to move your eyes. Next, if you add to

the rock a downward motion which is peculiar to it and not shared by you, and which is mixed with this circular motion, the circular portion of the motion which is common to the stone and the eye continues to be imperceptible. The straight motion alone is sensible, for to follow that you must move your eyes downwards.”¹⁴⁶

This is strong persuasion indeed.

Yielding to this persuasion, we now quite automatically start confounding the conditions of the two cases and become relativists. This is the essence of Galileo’s trickery! As a result the clash between Copernicus and “the conditions affecting ourselves and those in the air above us”¹⁴⁷ dissolves into thin air, and we finally realize “that all terrestrial events from which it is ordinarily held that the earth stands still and the sun and the fixed stars are moving would necessarily appear just the same to us if the earth moved and the others stood still.”¹⁴⁸

Paradigm I: Motion of compact objects in stable surroundings of great spatial extension—deer observed by the hunter.		Paradigm II: Motion of objects in boats, coaches, and other moving systems.	
Natural Interpretation: All motion is operative		Natural Interpretation: Only relative motion is operative	
Falling stone proves ↓ Earth at rest	Motion of earth predicts ↓ Oblique motion of stone	Falling stone proves ↓ No relative motion between starting point and earth	Motion of earth predicts ↓ No relative motion between starting point and stone

Let us now look at the situation from a more abstract point of view. We start with two conceptual subsystems of ordinary thought (see the preceding diagram). One of them regards motion as an absolute process which always has effects, effects on our senses included. The description of this conceptual system which appears in the present paper may be somewhat idealized, but the arguments of the opponents of Copernicus which are quoted by Galileo himself, and which according to him were “very plausible,”¹⁴⁹ show that there was a widespread tendency to think in its terms, and that this tendency was a serious obstacle for the discussion of alternative ideas. Occasionally one finds even more primitive ways of thinking, where concepts such as “up” and “down” are used absolutely. Examples are the assertion “that the earth is too heavy to climb up over the sun and then fall headlong back down again,”¹⁵⁰ or the assertion that “after a short time the mountains, sinking downward with the rotation of the terrestrial globe, would get into such a position that whereas a little earlier one would have had to climb steeply to their peaks, a few hours later one would have to stoop and descend in order to get there.”¹⁵¹ Galileo, in his marginal notes, calls these “utterly childish reasons [which] suffice[d] to keep imbeciles believing in the fixity of the earth”¹⁵² and he thinks it unnecessary “to bother about such men as these, whose name is legion, or to take notice of their fooleries.”¹⁵³ Yet it is clear that the absolute idea of motion was “well entrenched,” and that the attempt to replace it was bound to encounter strong resistance.

The second conceptual system is built around the relativity of motion, and is also well entrenched in its own domain of application. Galileo aims at replacing the first system by the second in all cases, terrestrial as well as celestial. Naive realism with respect to motion is to be completely eliminated.

Now, we have seen that this naive realism is on occasions an essential part of our observational vocabulary. On these occasions (Paradigm I), the observation language contains the idea of the efficacy of all motion. Or, to express it in the material mode of speech, our experience in these situations is the experience of objects which move absolutely. Taking this into consideration, it is apparent that Galileo’s proposal amounts to a partial revision of our observation language or of our experience. An experience which partly contradicts the idea of the motion of the earth is turned into an experience that confirms it, at least as far as “terrestrial things” are concerned.¹⁵⁴ This is what actually happens. But Galileo wants to per-

suade us that no change has taken place, that the second conceptual system is already universally *known*, even though it is not universally used. Both Salviati, his representative in the dialogue, and his opponent Simplicio, and also Sagredo, the intelligent layman, connect Galileo's method of argumentation with Plato's theory of *anamnesis*¹⁵⁵—a clever tactical move, typically Galilean, one is inclined to say. Yet we must not allow ourselves to be deceived about the revolutionary development that is actually taking place.

The resistance against the assumption that shared motion is nonoperative was equated with the resistance which forgotten ideas exhibit toward the attempt to make them known. Let us accept this *interpretation* of the resistance! But let us not forget its existence. We must then admit that it restricts the use of the relativistic ideas, confining them to *part* of our everyday experience. Outside this part, and that means in interstellar space, they are "forgotten," and therefore not active. But outside this part there is not complete chaos. Other concepts are used, among them those very same absolutistic concepts which derive from the first paradigm. We not only use them, but must admit that they are entirely adequate. No difficulties arise as long as one remains within the limits of the first paradigm. "Experience," that is, the totality of all facts from all domains described with the concepts which are appropriate in these domains, cannot force us to carry out the change which Galileo wants to introduce. The motive for a change must come from a different source.

It comes, first, from the desire to see "the whole [correspond] to its parts with wonderful simplicity"¹⁵⁶ as Copernicus had already expressed himself. It comes from the "typically metaphysical urge" for unity of understanding and conceptual presentation. And the motive for a change is connected, secondly, with the intention to make room for the motion of the earth, which Galileo accepts and is not prepared to give up. The idea of the motion of the earth is closer to the first paradigm than to the second, or at least it was at the time of Galileo. This gave great strength to the Aristotelian arguments, and made them very plausible. To eliminate this plausibility, it was necessary to subsume the first paradigm under the second, and to extend the relative notions to all phenomena. The idea of *anamnesis* functions here as a psychological crutch, as a lever which smoothes the process of subsumption by concealing its existence. As a result we are now *ready* to apply the relative notions not only to boats, coaches, birds, but also to the "solid and well-established earth" as a whole.

And we have the impression that this readiness was in us all the time, although it took some effort to make it conscious. This impression is most certainly erroneous: it is the result of Galileo's propagandistic machinations. We would do better to describe the situation in a different way, as a change of our conceptual system. Or, because we are dealing with concepts which belong to natural interpretations, and which are therefore connected with sensations in a very direct way, we should describe it as a *change of experience* that allows us to accommodate the Copernican doctrine. The change corresponds perfectly to the pattern outlined in an earlier paper: an inadequate view, the Copernican theory, is supported by another inadequate view, the idea of the nonoperative character of shared motion, and both theories gain strength and give support to each other in the process. It is this change which constitutes the transition from the Aristotelian point of view to the epistemology of modern science.

For experience now ceases to be that unchangeable fundament which it is both in common sense and in the Aristotelian philosophy. The attempt to support Copernicus makes experience "fluid" in the very same manner in which it makes the heavens fluid, "so that each star roves around in it by itself."¹⁵⁷ An empiricist who starts from experience, and builds on it without ever looking back, now loses the very ground on which he stands. Neither the earth, "the solid, well-established earth," nor the facts on which he usually relies, can be trusted any longer. It is clear that a philosophy that uses such a fluid and changing experience needs new methodological principles which do not insist on an asymmetric judgment of theories by experience. *Classical physics* intuitively adopts such principles; at least the great and independent thinkers, such as Newton, Faraday, and Boltzmann, proceed in this way. But its *official doctrine* still clings to the idea of a stable and unchanging basis. The clash between this doctrine and the actual procedure is concealed by a tendentious presentation of the *results* of research that hides their revolutionary origin and suggests that they arose from a stable and unchanging source. These methods of concealment start with Galileo's attempt to introduce new ideas under the cover of *anamnesis*, and they culminate in Newton.¹⁵⁸ They must be exposed if we want to arrive at a better account of the progressive elements in science.

8. The Law of Inertia

Our discussion of the anti-Copernican argument is not yet complete. So

far, we have tried to discover what assumption will make a stone that moves alongside a moving tower appear to fall "straight down," instead of being seen to move in an arc. The assumption, which I shall call the *relativity principle*, that our senses notice only relative motion, and are completely insensitive to a motion which objects have in common, was seen to do the trick. What remains to be explained is *why the stone stays with the tower*, and why it is not left behind. In order to save the Copernican view, one must explain not only why a motion that preserves the relation among visible objects *remains unnoticed*, but also why a common motion of various objects does not affect their relation. That is, one must explain why such a motion is not a *causal agent*. Turning the question around in the manner explained in section 6, it is now apparent that the anti-Copernican argument of section 5 rests on two natural interpretations,¹⁵⁹ viz. the *epistemological assumption* that absolute motion is always noticed and the *dynamical principle* that objects (such as the falling stone) which are not interfered with move toward their natural place. The present problem is to supplement the relativity principle with a new law of inertia in such a fashion that the motion of the earth can still be asserted. One sees at once that the following law, the *principle of circular inertia*, as I shall call it, provides the required solution: An object that moves with a given angular velocity on a frictionless sphere around the center of the earth will continue moving with the same angular velocity forever. Combining the appearance of the falling stone with the relativity principle, the principle of circular inertia, and some simple assumptions concerning the composition of velocities, yields an argument which no longer endangers Copernicus's view, but can be used to give it partial support.

The relativity principle was defended in two ways. The first was by showing how it helps Copernicus; this defense is truly ad hoc. The second was by pointing to its function in common sense, and by surreptitiously generalizing that function (see section 7). No independent argument was given for its validity.¹⁶⁰ Galileo's method of support for the principle of circular inertia is of exactly the same kind. He introduces it, again not by reference to experiment or to independent observation, but by reference to what everyone is already supposed to know.

Simplicio: So you have not made a hundred tests, or even one? And yet you so freely declare it to be certain? . . .

Salviati: Without experiment, I am sure that the effect will happen as I tell you, because it must happen that way; and I might add that you yourself also know that it cannot happen otherwise, no matter how you may pretend not to know it . . . But I am so handy at picking people's brains that I shall make you confess this in spite of yourself.¹⁶¹

Step by step Simplicio is forced to admit that a body that moves without friction on a sphere concentric with the center of the earth will carry out a "boundless," a "perpetual" motion.¹⁶² We know, of course, especially after the analysis we have just completed of the nonoperative character of shared motion, that what Simplicio accepts is based neither on experiment nor on corroborated theory. It is a daring new suggestion involving a tremendous leap of the imagination. A little more analysis then shows that this suggestion is connected with experiments, such as the "experiments" of the *Discorsi*, by ad hoc hypotheses. (The amount of friction to be eliminated follows not from independent investigations—such investigations commence only much later, in the eighteenth century—but from the very result to be achieved, viz. the circular law of inertia.) Viewing natural phenomena in this way leads, as we have already said, to a complete reevaluation of all experience. We can now add that it leads to the invention of a *new kind of experience* that is not only more sophisticated but also far more speculative than is the experience of Aristotle or of common sense. Speaking paradoxically, but not incorrectly, one may say that *Galileo invented an experience that has metaphysical ingredients*.¹⁶³ It is by means of such an experience that the transition from a geostatic cosmology to the point of view of Copernicus and Kepler is achieved.

9. The Progressive Role of Ad Hoc Hypotheses

This is the place to briefly mention certain ideas which have been developed by Lakatos, and which throw new light on the problem of the growth of knowledge.

It is customary to assume that good scientists refuse to employ ad hoc hypotheses, and to assert that they are right in their refusal. New ideas, so it is thought, go far beyond the available evidence, and they must go beyond it in order to be of value. Ad hoc hypotheses are bound to creep in eventually, but they should be resisted and kept at bay. This is the customary attitude as it is expressed, for example, in the writings of K. R. Popper.

As opposed to this, Lakatos, in lectures, and now also in publications, has pointed out that “ad hocness” is neither despicable nor absent from the body of science. New ideas, he emphasizes, are usually almost entirely ad hoc, they cannot be otherwise. And they are reformed only in a piecemeal fashion, by gradually stretching them, so that they apply to situations lying beyond their starting point. Schematically:

Popper: new theories have, and must have, excess content which is, but should not be, gradually infected by ad hoc adaptations.

Lakatos: new theories are, and cannot be anything but, ad hoc. Excess content is, and should be, created in a piecemeal fashion, by gradually extending them to new facts and domains.

The historical material I have just analyzed (and the more extensive material presented in “Problems of Empiricism, Part II”) lends unambiguous support to the position of Lakatos. In what follows I shall try to show this in some detail.

First, *kinematic relativity* (cf. section 7, above):

Just like Newtonian physics, Aristotelian physics distinguishes between relative space and absolute space.¹⁶⁴ In addition, it allows one to “operationally” determine absolute places, directions, velocities. One may proceed in the following way: The center of the universe is found, for example, by backwardly elongating the direction of two flames, and it is tested by using a third flame. Flames function here as test bodies and not as reference bodies for relative motion. Distance from the center is determined by the strength of the upward motion of flames, or of suitable mixtures which may be enclosed in test capsules. Thus, space is traced out, in an entirely physical way, by using known physical laws. Direction, finally, is determined by determining the axis of rotation of the stellar sphere. This whole physical background is removed by Galileo. With it, we lose all means of testing for center, distance, and direction. The new relativistic principles (only relative motion is “operative”) are therefore metaphysical, and, because adapted to the tower experiment, also ad hoc.

Considering now *dynamical relativity* (section 8), one should remember, first of all, that the natural character of circular motion was not first asserted by Galileo. It was an old assumption, concerning all supralunar entities. The new assumption introduced by Galileo (and by Copernicus, in chapter VIII of *De revolutionibus*) is that circular motion is a natural motion for terrestrial objects also. On the one hand, this is an immediate

consequence of having made the earth a star: Stars move in circles. Hence, if the earth is a star, its natural motion will be circular, both its motion around the sun and “its motion with respect to itself,” as its rotation was described at the time. Now, does this particular assumption of the rotation of earth assert anything over and above what was known to happen at its surface at Galileo’s own time? My attitude, which is in accordance with Lakatos’s general theory, is that the answer must be no. The only consequence of the assertion is that it connects moving objects rigidly with the framework of the moving, i.e., rotating, earth. This leaves everything as it is, and it especially leaves the results of the tower experiment and the cannon experiment unchanged.¹⁶⁵ No further consequence was implied at the time. (It was different with the motion of the earth around the sun which led one to expect a sizable stellar parallax.) Even the later Newtonian argument that distant objects, moving with the same angular velocity, will hit the earth *ahead* of the tower cannot be used at this stage: it is not at all clear whether Galileo would want distant objects to move with the same angular velocity. (In the case of the planets he notices their *decreasing* angular velocity—the effect of Kepler’s third law—and he might have been inclined to treat bodies circulating around the earth in the same way. On the other hand, he calculates the time a stone takes to drop from the moon to the earth by assuming a constant acceleration all the way.¹⁶⁶)

Furthermore, I do not think that bringing in the tradition of the impetus theory will improve matters. For this theory is again ad hoc, this time not with respect to the tower, but with respect to the behavior of objects thrown (which continue to move, contrary to Aristotle’s law of inertia). When a circular law is asserted, as seems to be the case with Buridan, the problem is the same as for Galileo.¹⁶⁷ (Besides, the impetus theory is incompatible with Galileo’s idea of the nonoperative character of all motion.¹⁶⁸)

Finally, one must not argue against “ad hocness” by pointing to the fact that experiments were made in boats, with cannon balls, on towers, and so on.¹⁶⁹ These experiments did not lead to any decisive result. And they did not test any excess content of the law of circular inertia, but tried to establish the fact which the law then explains in ad hoc fashion. Reference to the experiments with the inclined plane is also beside the point. These experiments test, if that is the right word, the law of free

fall. But of course there still remains the task of subdividing *that* motion into an inertial motion and something else. However one looks at the matter, the best conjecture is that at the time in question the circular law of inertia, and to an even greater extent the idea of the relativity of motion, was an ad hoc hypothesis designed to get out of the trouble of the tower.

Now this is such an incredible situation that a little more argument seems to be required. We therefore take a brief look at Galileo's earlier work on mechanics and motion.

In *De motu* motions of spheres in the center of the universe, outside of it, homogeneous, nonhomogeneous, supported at the center of gravity, supported outside of it are discussed, and described as being either natural, or forced, or neither. But about the actual motion of such spheres we hear very little, and what we do hear is by implication only. Thus there appears the question¹⁷⁰ whether a homogeneous sphere made to move in the center of the universe would move forever. We read that "it seems that it should move perpetually," but an unambiguous answer is never given. A marble sphere supported on an axis through the center and set in motion is said to "rotate for a long time"¹⁷¹ in *De motu* while a perpetual motion is said to be "quite out of keeping with the nature of the earth itself to which rest seems to be more congenial than motion" in the *Dialogue on Motion*.¹⁷² Another argument against perpetual rotations is found in Benedetti's *Diverse Speculations*.¹⁷³ Rotations, says Benedetti, are "certainly not perpetual," for the parts of the sphere, wanting to move in a straight line, are constrained against their nature, "and so they come to rest naturally." Again, in *De motu*,¹⁷⁴ we find a criticism of the assertion that adding a star to the celestial sphere might slow it down by changing the relation between the force of the moving intelligences and the resistance of the sphere. This assertion, Galileo says, certainly applies to an excentric sphere. Adding weight to an excentric sphere means that a weight will occasionally be moved away from the center and be raised to a higher level. But "who would ever say that [a concentric sphere] was impeded by the weight, since the weight in its circular path would neither approach, nor recede from, the center."¹⁷⁵ Note that the original rotation is in this case said to be caused by an intelligence; it is not assumed to be taking place all by itself. This is in perfect agreement with Aristotle's general theory of motion¹⁷⁶ where a mover is postulated for every motion, and not just for violent motions. Galileo seems to accept this part of

the theory both when letting rotating spheres slow down and when accepting the "force of the intelligences" in the present argument (he also accepts impetus—see below). But in objecting to the idea that a new star will increase resistance he adopts the entirely different view that resistance occurs only when a motion is forced, and is absent otherwise. This is neither Aristotelian nor compatible with the version of the impetus theory he holds at the time which attributes any prolonged motion to an internal moving force similar to the force of sound that resides in a bell long after it has been struck,¹⁷⁷ and which is again supposed to "gradually diminish."¹⁷⁸

Looking at these few examples we see that Galileo ascribes a special position to motions which are neither violent nor forced. Such motions may last for a considerable time though they are not supported by the surrounding medium. But *they do not last forever, and they need an internal driving force* in order to even persist for a finite time.

Now if one wants to overcome the dynamical arguments against the motion of the earth (and we are here always thinking about its rotation rather than about its motion around the sun), then the two italicized principles must both be revised. It must be assumed that the "neutral" motions which Galileo discusses in his early dynamical writings may last forever, or at least for periods comparable to the age of historical records. And these motions must be regarded as "natural" in the entirely new and revolutionary sense that neither an outer nor an inner motor is needed to keep them going. The first assumption is necessary to allow the earth to rotate. The second assumption is necessary if we want to regard motion as a *relative* phenomenon, depending on the choice of a suitable coordinate system.¹⁷⁹ Copernicus, in his brief remarks on the problem,¹⁸⁰ makes both assumptions. Galileo never clearly resolves the problem. He formulates permanence along a horizontal line as a hypothesis in his *Discorsi*¹⁸¹ and he seems to make both assumptions in the *Dialogue*.¹⁸² Now my guess is that a *clear statement of permanent motion with(out) impetus developed in Galileo only together with his gradual acceptance of the Copernican view*. Galileo changed his view about the "neutral" motions—he made them permanent and "natural"—in order to make them compatible with the rotation of the earth and in order to evade the difficulties of the tower argument.¹⁸³ His new ideas concerning such motions are therefore at least partly ad hoc. Impetus in the old sense disappeared partly for methodological reasons (interest in the *how*, not

in the *why*—this development itself deserves careful study), partly because of the vaguely perceived inconsistency with the idea of the relativity of all motion. The wish to save Copernicus plays a role in either case. This hypothesis must of course be tested by an examination of Galileo's published writings and his correspondence between 1590 and, say, 1630. Considering what we know already we must admit that it has much plausibility.

Now, if we are right in assuming that Galileo framed an ad hoc hypothesis at this point, then we can also praise him for his methodological acumen. It is obvious that the moving earth demands a new dynamics. One test of the old dynamics consists in the attempt to establish the motion of the earth. Trying to establish the motion of the earth is the same as trying to find a refuting instance for the old dynamics. The motion of the earth, however, is inconsistent with the tower experiment *interpreted in accordance with the old dynamics*. Interpreting the tower experiment in accordance with the old dynamics therefore means trying to save the old dynamics in an ad hoc fashion. If one does not want to do this one must find a different interpretation for the phenomena of free fall. What interpretation should be chosen? One wants an interpretation that turns the motion of the earth into a refuting instance of the old dynamics, without lending ad hoc support to the motion of the earth itself. The first step toward such an interpretation is to establish contact, however vague, with the "phenomena," i.e., with the falling stone, and to establish it in such a manner that the motion of the earth is not obviously contradicted. The most primitive element of this first step is to frame an ad hoc hypothesis with respect to the rotation of the earth. The next step would then be to elaborate the hypothesis, so that additional predictions become possible. Copernicus and Galileo take the first and most primitive step. Their procedure looks contemptible only if one forgets that the aim is to test older views rather than to prove new ones, and if one also forgets that developing a good theory is a complex process that has to start modestly and that takes time. But why, an impatient methodologist might ask, did it take so long before additional phenomena were added? It took so long because *the domain of possible phenomena had first to be circumscribed by the further development of the Copernican hypothesis*. It is much better to remain ad hoc for a while, and in the meantime to develop heliocentrism in all its astronomical ramifications which can then be used as guidelines for a further elaboration of dynamics.

Therefore: Galileo *did* use ad hoc hypotheses. It was good that he used them. Had he not been ad hoc, he would have been ad hoc anyway, but this time with respect to an older theory. Hence, as one cannot help being ad hoc, it is better to be ad hoc with respect to a new theory, for a new theory, like all new things, will give a feeling of freedom, excitement, and progress. Galileo is to be applauded because he preferred protecting an interesting hypothesis to protecting a dull one.

10. Summary of Analysis of Tower Argument

I repeat and summarize: An argument is proposed that refutes Copernicus by observation. The argument is inverted in order to discover those natural interpretations which are responsible for the contradiction. The offensive interpretations are replaced by others. Propaganda and appeal to distant and highly theoretical parts of common sense are used to defuse old habits and to enthrone new ones. The new natural interpretations which are also formulated explicitly as auxiliary hypotheses are established partly by the support they give to Copernicus and partly by plausibility considerations and ad hoc hypotheses. An entirely new "experience" arises in this way. Independent evidence is as yet entirely lacking, but this is no drawback as it is to be expected that independent support will take a long time appearing. For what is needed is a theory of solid objects, aerodynamics, hydrodynamics, and all these sciences are still hidden in the future. *But their task is now well defined*, for Galileo's assumptions, his ad hoc hypotheses included, are sufficiently clear and simple to prescribe the direction of future research. Let it be noted, incidentally, that Galileo's procedure drastically reduces the content of dynamics. Aristotelian dynamics was a general theory of change comprising locomotion, qualitative change, generation, and corruption, and it provided a theoretical basis for witchcraft also. Galileo's dynamics and its successors deal with locomotion only, and here again only with the locomotion of matter. The other kinds of motion are pushed aside with the promissory note, due to Democritus, that locomotion will eventually be capable of explaining all motion. Thus, a comprehensive empirical theory of motion is replaced by a much narrower theory¹⁸⁴ plus a metaphysics of motion, just as an "empirical" experience is replaced by an experience that contains strange and speculative elements. *Counterinduction*, however, is now justified both for theories and for facts. It clearly plays an important role in

the advancement of science. This concludes the considerations which started in section 2. For details and further examples the reader is again referred to my "Problems of Empiricism, Part II."

11. Discovery and Justification; Observation and Theory

Let us now use the material of the preceding sections to throw light on the following features of contemporary empiricism: first, the distinction between a context of discovery and a context of justification; second, the distinction between observational terms and theoretical terms; third, the problem of incommensurability.

One of the objections which may be raised against the preceding discussion is that it has confounded two contexts which are essentially separate, viz. a context of discovery and a context of justification. *Discovery* may be irrational and need not follow any recognized method. *Justification*, on the other hand, or, to use the Holy Word of a different school, *criticism*, starts only after the discoveries have been made and proceeds in an orderly way. Now, if the example given here and the examples I have used in earlier papers show anything, then they show that the distinction refers to a situation that does not arise in practice at all. And, if it does arise, it reflects a temporary stasis of the process of research. Therefore, it should be eliminated as quickly as possible.

Research at its best is an *interaction* between new theories which are stated in an explicit manner and older views which have crept into the observation language. It is not a one-sided *action* of the one upon the other. Reasoning within the context of justification, however, presupposes that one side of this pair, viz. observation, has frozen, and that the principles which constitute the observation concepts are preferred to the principles of a newly invented point of view. The former feature indicates that the discussion of principles is not carried out as vigorously as is desirable; the latter feature reveals that this lack of vigor may be due to some unreasonable and perhaps not even explicit preference. But is it wise to be dominated by an inarticulate preference of this kind? Is it wise to make it the *raison d'être* of a distinction that separates two entirely different modes of research? Or should we not rather demand that our methodology treat explicit and implicit assertions, doubtful and intuitively evident theories, known and unconsciously held principles, in exactly the same way, and that it provide means for the discovery and the criticism of the latter? Abandoning the distinction between a context of discovery

and a context of justification is the first step toward satisfying this demand.

Another distinction which is clearly related to the distinction between discovery and justification is the distinction between *observational terms* and *theoretical terms*. It is now generally admitted that the distinction is not as sharp as it was thought to be only a few decades ago. It is also admitted, in complete agreement with Neurath's original views, that *both* theories and observation statements are open to criticism. Yet the distinction is still held to be a useful one and is defended by almost all philosophers of science. But what is its point? Nobody will deny that the sentences of science can be classified into long sentences and short sentences, or that its statements can be classified into those which are intuitively obvious and others which are not. But nobody will put particular weight on these distinctions, or will even mention them, for they do not now play any role in the business of science. (This was not always so. Intuitive plausibility, for example, was once thought to be a most important guide to the truth; but it disappeared from methodology the very moment intuition was replaced by experience.) Does experience play such a role in the business of science? Is it as essential to refer to experience as it was once thought essential to refer to intuition? Considering what has been said in section 4, I think that these questions must be answered in the negative. True—much of our thinking arises from experience, but there are large portions which do not arise from experience at all but are firmly grounded on intuition, or on even deeper lying reactions. True—we often test our theories by experience, but we equally often *invert* the process; we *analyze* experience with the help of more recent views and we *change* it in accordance with these views (see the preceding discussion of Galileo's procedure). Again, it is true that we often rely on experience in a way that suggests that we have here a solid foundation of knowledge, but such reliance turns out to be just a psychological quirk, as is shown whenever the testimony of an eyewitness or of an expert crumbles under cross-examination. Moreover, we equally firmly rely on general principles so that even our most solid *perceptions* (and not only our *assumptions*) become indistinct and ambiguous when they clash with these principles. The symmetry between observation and theory that emerges from such remarks is perfectly reasonable. Experience, just as our theories, contains natural interpretations which are abstract and even metaphysical ideas. For example, it contains the idea of an observer-independent exist-

ence. It is incontestable that these abstractions, these speculative ideas, are connected with sensations and perceptions. But, first of all, this does not give them a privileged position, unless we want to assert that perception is an infallible authority. And, secondly, it is quite possible to altogether *eliminate* perception from all the essential activities of science (see above, section 6 as well as the appendix). All that remains is that some of our ideas are accompanied by strong and vivid psychological processes, "sensations," while others are not. This, however, is just a peculiarity of human existence which is as much in need of examination as is anything else.

Now, if we want to be "truly scientific" (dreaded words!), should we then not regard the theses "experience is the foundation of our knowledge" and "experience helps us to discover the properties of the external world" as (very general) hypotheses? And must these hypotheses not be examined just like any other hypothesis, and perhaps even more vigorously, as so much depends on their truth? Furthermore, will not such an examination be rendered impossible by a method that either justifies or criticizes "on the basis of experience"? These are some of the questions which arise in connection with the customary distinctions between observation and theory, discovery and justification. None of them is really new. They are known to philosophers of science, and are discussed by them at length. But the inference that the distinction between theory and observation has now ceased to be relevant either is not drawn or is explicitly rejected.¹⁸⁵ Let us take a step forward, and let us abandon this last remainder of dogmatism in science!

12. Rationality Again

Incommensurability, which I shall discuss next, is closely connected with the question of the rationality of science. Indeed, one of the most general objections, either against the use of incommensurable theories or even against the idea that *there* are such theories to be found in the history of science, is the fear that they would severely restrict the efficacy of traditional, nondialectical *argument*. Let us, therefore, look a little more closely at the critical *standards* which, according to some people, constitute the content of a "rational" argument. More especially, let us look at the standards of the Popperian school with whose ratiomania we are here mainly concerned.

Critical rationalism is either a meaningful idea or a collection of slo-

gans (such as "truth"; "professional integrity"; "intellectual honesty") designed to intimidate yellow-bellied opponents (who has the fortitude, or even the insight, to declare that Truth might be unimportant, and perhaps even undesirable?).

In the former case it must be possible to produce rules, standards, restrictions which permit us to separate critical behavior (thinking, singing, writing of plays) from other types of behavior so that we can *discover* irrational actions and correct them with the help of concrete suggestions. It is not difficult to produce the standards of rationality defended by the Popperian school.

These standards are standards of *criticism*: rational discussion consists in the attempt to criticize, and not in the attempt to prove, or to make probable. Every step that protects a view from criticism, that makes it safe, or "well founded," is a step away from rationality. Every step that makes it more vulnerable is welcome. In addition it is recommended that ideas which have been found wanting be abandoned, and it is forbidden to retain them in the face of strong and successful criticism unless one can present a suitable counterargument. Develop your ideas so that they can be criticized; attack them relentlessly; do not try to protect them, but exhibit their weak spots; and eliminate them as soon as such weak spots have become manifest—these are some of the rules put forth by our critical rationalists.

These rules become more definite and more detailed when we turn to the philosophy of science, and especially to the philosophy of the natural sciences.

Within the natural sciences criticism is connected with experiment and observation. The content of a theory consists in the sum total of those basic statements which contradict it; it is the class of its potential falsifiers. Increased content means increased vulnerability; hence theories of large content are to be preferred to theories of small content. Increase of content is welcome; decrease of content is to be avoided. A theory that contradicts an accepted basic statement must be given up. Ad hoc hypotheses are forbidden—and so on and so forth. A science, however, that accepts the rules of a critical empiricism of this kind will develop in the following manner.

We start with a *problem* such as the problem of the planets at the time of Plato. This problem is not merely the result of *curiosity*, it is a *theoretical result*, it is due to the fact that certain *expectations* have been

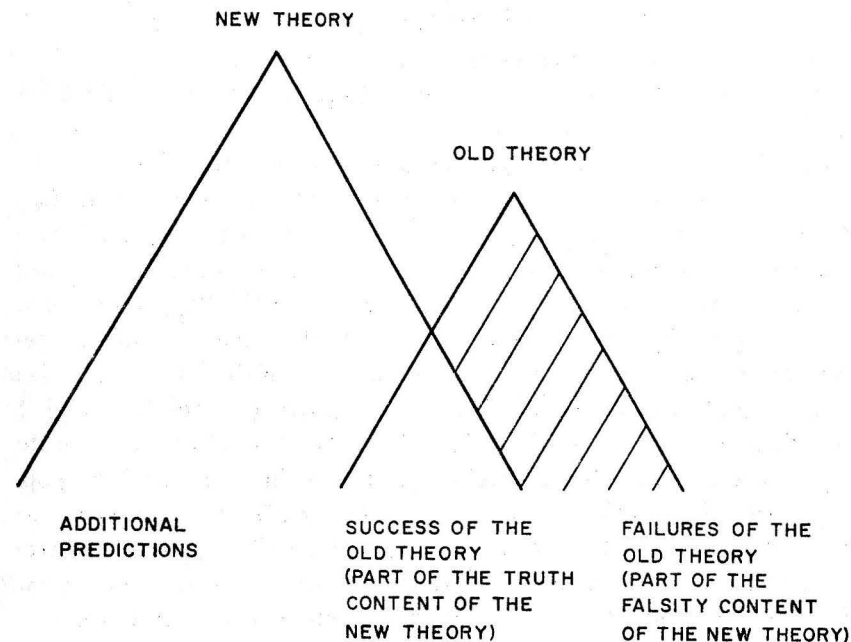
disappointed: On the one hand it seemed to be clear that the stars must be divine; hence one expects them to behave in an orderly and lawful manner. On the other hand one cannot find any easily discernible regularity. The planets, to all intents and purposes, move in a quite chaotic fashion. How can this fact be reconciled with the expectation and with the principles that underlie the expectation? Does it show that the expectation is mistaken? Or have we failed in our analysis of the facts? This is the problem.

It is important to see that the elements of the problem are not simply given. The "fact" of irregularity, for example, is not accessible without further ado. It cannot be discovered by just anyone who has healthy eyes and a good mind. It is only through a certain expectation that it becomes an object of our attention. Or, to be more accurate: this fact of irregularity exists because there is an expectation of regularity. After all, the term "irregularity" makes sense only if we have a rule. In our case the rule (which is a more specific part of the expectation that has not yet been mentioned) asserts circular motion with constant angular velocity. The fixed stars agree with this rule and so does the sun if we trace its path relative to the fixed stars. The planets do not obey the rule, neither directly, with respect to the earth, nor indirectly, with respect to the fixed stars.

(In the case just discussed the rule is formulated explicitly, and it can be discussed. This need not be the case. Recognizing a color as red is made possible by deep-lying assumptions concerning the structure of our surroundings and recognition does not occur when these assumptions cease to be available.)

To sum up this part of the Popperian doctrine: Research starts with a problem. The problem is the result of a conflict between an expectation and an observation which in turn is constituted by the expectation. It is clear that this doctrine differs from the doctrine of inductivism where objective facts mysteriously enter a passive mind and leave their traces there. It was prepared by Kant, by Dingler, and, in a very different manner, by Hume.

Having formulated a problem one tries to solve it. Solving a problem means inventing a theory that is relevant, falsifiable (to a larger degree than any alternative solution), but not yet falsified. In the case mentioned above (planets at the time of Plato) the problem was to find circular



motions of constant angular velocity for the purpose of saving the planetary phenomena. It was solved by Eudoxos.

Next comes the *criticism* of the theory that has been put forth in the attempt to solve the problem. Successful criticism removes the theory *once and for all* and creates a new problem, viz. to explain (a) why the theory has been successful so far; (b) why it failed. Trying to solve this problem we need a new theory that produces the successful consequences of the older theory, denies its mistakes, and makes additional predictions not made before. These are some of the *formal conditions* which a *suitable successor of a refuted theory* must satisfy. Adopting the conditions one proceeds, by conjectures and refutations, from less general theories to more general theories and expands the content of human knowledge. More and more facts are *discovered* (or constructed with the help of expectations) and are then *connected* in a reasonable manner. There is no guarantee that man will solve every problem and replace every theory that has been refuted with a successor satisfying the formal conditions. The invention of theories depends on our talents and other fortuitous circumstances, such as a satisfactory sex life. But as long as these talents hold out the accompanying scheme is a correct account of the growth of a knowledge that satisfies the rules of critical rationalism.

Now, at this point we may raise two questions:

1. Is it *desirable* to live in accordance with the rules of a critical rationalism?

2. Is it *possible* to have both a science as we know it and these rules?

As far as I am concerned the first question is far more important than the second. True—science and other depressing and narrow-minded institutions play an important part in our culture and they occupy the center of interest of most philosophers. Thus the ideas of the Popperian school were obtained by generalizing solutions for methodological and epistemological problems. Critical rationalism arose from the attempt to solve Hume's problem and to understand the Einsteinian revolution, and it was then extended to politics, and even to the conduct of one's private life (Habermas and others therefore seem to be justified in calling Popper a positivist). Such a procedure may satisfy a *school philosopher* who looks at life through the spectacles of his own specific problems and recognizes hatred, love, happiness only to the extent to which they occur in these problems. But if we consider the interests of *man* and, above all, the question of his freedom (freedom from hunger, despair, from the tyranny of constipated systems of thought, *not* the academic "freedom of the will"), then we are proceeding in the worst possible fashion.

For is it not possible that science as we know it today (the science of critical rationalism that has been freed from all inductive elements) or a "search for the truth" in the style of traditional philosophy will create a monster? Is it not possible that it will harm man, turn him into a miserable, unfriendly, self-righteous mechanism without charm and without humor? "Is it not possible," asks Kierkegaard, "that my activity as an objective [or a critico-rational] observer of nature will weaken my strength as a human being?"¹⁸⁶ I suspect the answer to all these questions must be affirmative and I believe that a reform of the sciences that makes it more anarchistic and more subjective (in Kierkegaard's sense) is therefore urgently needed. But this is not what I want to discuss in the present essay. Here I shall restrict myself to the second question and I shall ask: is it possible to have both a science as we know it and the rules of a critical rationalism as just described? And to *this* question the answer seems to be a resounding *no*.

To start with we have seen, though rather briefly,¹⁸⁷ that the actual development of institutions, ideas, practices, and so on often does not start from a problem but rather from some irrelevant activity, such as play-

ing, which, as a side effect, leads to developments which later on can be interpreted as solutions to unrealized problems. Are such developments to be excluded? And if we do exclude them, will this not considerably reduce the number of our adaptive reactions and the quality of our learning process?

Secondly, we have seen, in sections 4ff, that a strict principle of falsification, or a "naive falsificationism" as Imre Lakatos calls it, combined with the demand for maximum testability and non-adhocness would wipe out science as we know it, and would never have permitted it to start. This has been realized by Lakatos who has set out to remedy the situation.¹⁸⁸ His remedy is not mine, it is not anarchism. His remedy consists in slight modification of the "critical standards" he adores. (He also tries to show, with the help of amusing numerological considerations, that it is already foreshadowed in Popper.)

According to naive falsificationism, a theory is judged, i.e., either accepted or condemned, as soon as it is introduced into the discussion. Lakatos gives a theory time, he permits it to develop, to show its hidden strength, and he judges it only "in the long run." The "critical standards" he employs provide for an interval of hesitation. They are applied "with hindsight." If the theory gives rise to interesting new developments, if it engenders "progressive problem shifts," then it may be retained despite its initial vices. If on the other hand the theory leads nowhere, if the ad hoc hypotheses it employs are not the starting point but the end of all research, if the theory seems to kill the imagination and to dry up every resource of speculation, if it creates "degenerating problem shifts," i.e., changes which terminate in a dead end, then it is time to give it up and to look for something better.

Now it is easily seen that standards of this kind have practical force only if they are combined with a *time limit*. What looks like a degenerating problem shift may be the beginning of a much longer period of advance, so—how long are we supposed to wait? But if a time limit is introduced, then the argument against the more conservative point of view, against "naive falsificationism," reappears with only a minor modification. For if you can wait, then why not wait a little longer? Besides there are theories which for centuries were accompanied by degenerating problem shifts until they found the right defenders and returned to the stage in full bloom. The heliocentric theory is one example. The atomic theory is another. We see that the new standards which Lakatos wants to de-

hend either are vacuous—one does not know when and how to apply them—or else can be criticized on grounds very similar to those which led to them in the first place.

In these circumstances one can do one of the following two things. One can stop appealing to permanent standards which remain in force throughout history, and govern every single period of scientific development and every transition from one period to another. Or one can retain such standards as a *verbal ornament*, as a memorial to happier times when it was still thought possible to run a complex and catastrophic business like science by a few simple and “rational” rules. It seems that Lakatos wants to choose the second alternative.

Choosing the second alternative means abandoning permanent standards *in fact*, though retaining them *in words*. *In fact* Lakatos’s position now is identical with the position of Popper as summarized in the marvelous (because self-destructive) Appendix i/15 of the fifth edition of the *Open Society*.¹⁸⁹ According to Popper, we do not “need any . . . definite frame of reference for our criticism,” we may revise even the most fundamental rules and drop the most fundamental demands if the need for a different measure of excellence should arise.¹⁹⁰ Is such a position irrational? Yes and no. Yes, because there no longer exists a single set of rules that will guide us through all the twists and turns of the history of thought (science), either as participants or as historians who want to reconstruct its course. One can of course *force* history into a pattern, but the results will always be poorer and less interesting than were the actual events. No, because each particular episode is rational in the sense that some of its features can be explained in terms of reasons which were either accepted at the time of its occurrence or invented in the course of its development. Yes, because even these local reasons which change from age to age are never sufficient to explain *all* the important features of a particular episode. One must add accidents, prejudices, material conditions, e.g., the existence of a particular type of glass in one country and not in another for the explanation of the history of optics, the vicissitudes of married life (Ohm!), superficiality, pride, oversight, and many other things, in order to get a complete picture. No, because, transported into the climate of the period under consideration and endowed with a lively and curious intelligence, we might have had still more to say; we might have tried to overcome accidents, and to “rationalize” even the most

whimsical sequence of events. But, and now I come to a decisive point for the discussion of incommensurability, how is the transition from certain standards to other standards to be achieved? More especially, what happens to our standards, as opposed to our theories, during a period of revolution? Are they changed in the manner suggested by Mill, by a critical discussion of alternatives, or are there processes which defy a rational analysis? Well, let us see!

That standards are not always adopted on the basis of argument has been emphasized by Popper himself. Children, he says, “learn to imitate others . . . and so learn to look upon standards of behavior as if they consisted of fixed, ‘given’ rules . . . and such things as sympathy and imagination may play an important role in this development.”¹⁹¹ Similar considerations apply to those grownups who want to continue learning, and who are intent on expanding both their knowledge and their sensibility. This we have already discussed in section 1. Popper also admits that new standards may be discovered, invented, accepted, imparted to others in a very irrational manner. But, he points out, one can criticize them *after* they have been adopted, and it is *this* possibility which keeps our knowledge rational. “What, then, are we to trust?” he asks after a survey of possible sources for standards.¹⁹² “What are we to accept? The answer is: whatever we accept we should trust only tentatively, always remembering that we are in possession, at best, of partial truth (or rightness), and that we are bound to make at least some mistake or misjudgment somewhere—not only with respect to facts but also with respect to the adopted standards; secondly, we should trust (even tentatively) our intuition only if it has been arrived at as the result of many attempts to use our imagination; of many mistakes, of many tests, of many doubts, and of searching criticism.”

Now this reference to tests and to criticism, which is supposed to guarantee the rationality of science, and, perhaps, of our entire life, may be either to *well-defined procedures* without which a criticism or test cannot be said to have taken place, or to a purely *abstract* notion, so that it is left to us to fill it now with this, and now with that concrete content. The first case has just been discussed. In the second case we have again but a verbal ornament. The questions asked in the last paragraph but one remain unanswered in either case.

In a way even this situation has been described by Popper, who says that “rationalism is necessarily far from comprehensive or self-contained.”¹⁹³

But our present inquiry is not whether *there are* limits to our reason; the question is *where* these limits are situated. Are they outside the sciences so that science itself remains entirely rational; or are irrational changes an essential part even of the most rational enterprise that has been invented by man? Does the historical phenomenon "science" contain ingredients which defy a rational analysis, although they may be described with complete clarity in psychological or sociological terms? Can the abstract aim to come closer to the truth be reached in an entirely rational manner, or is it perhaps inaccessible to those who decide to rely on argument only? These are the problems which were raised, first by Hegel and then, in quite different terms, by Kuhn. They are the problems I wish to discuss.

In discussing these further problems, Popper and Lakatos reject considerations of sociology and psychology, or as Lakatos expresses himself, "mob psychology," and assert the rational character of *all* science. According to Popper, it is possible to arrive at a judgment as to which of two theories is closer to the truth, even if the theories should be separated by a catastrophic upheaval such as a scientific or other revolution. (A theory is closer to the truth than another theory if the class of its true consequences, its truth content, exceeds the truth content of the latter without an increase of falsity content.) According to Lakatos, the apparently unreasonable features of science occur only in the material world and in the world of (psychological) thought; they are absent from the "world of ideas, from Plato's and Popper's 'third world.'" It is in this third world that the growth of knowledge takes place, and that a rational judgment of all aspects of science becomes possible.

Now in regard to this convenient flight into higher regions, it must be pointed out that the scientist is, unfortunately, dealing with the world of matter and of psychological (i.e., subjective) thought also. It is *mainly* this material world he wants to change and to influence. And the rules which create order in the third world will most likely be entirely inappropriate for creating order in the brains of living human beings (unless these brains and their structural features are put in the third world also, a point that does not become clear from Popper's account¹⁹⁴). The numerous deviations from the straight and rather boring path of rationality which one can observe in actual science may well be necessary if we want to achieve progress with the brittle and unreliable material (instruments; brains; assistants; etc.) at our disposal.

However, there is no need to pursue this objection further. There is no need to argue that science as we know it may differ from its third-world shadow *in precisely those respects* which make progress possible.¹⁹⁵ For the Popperian model of an approach to the truth breaks down even if we confine ourselves to ideas entirely. It breaks down because there are incommensurable theories.

13. Incommensurability

Scientific investigation, says Popper, starts with a problem, and it proceeds by *solving* it.

This characterization does not take into account that problems may be wrongly formulated, that one may inquire about properties of things or processes which later research declares to be nonexistent. Problems of this kind are not *solved*, they are *dissolved* and removed from the domain of legitimate inquiry. Examples are the problem of the absolute velocity of the earth, the problem of the trajectory of an electron in an interference pattern, or the important problem whether incubi are capable of producing offspring or whether they are forced to use the seeds of men for that purpose.¹⁹⁶

The first problem was dissolved by the theory of relativity which denies the existence of absolute velocities. The second problem was dissolved by the quantum theory which denies the existence of trajectories in interference patterns. The third problem was dissolved, though much less decisively so, by modern (i.e., post-sixteenth century) psychology and physiology as well as by the mechanistic cosmology of Descartes.

Now changes of ontology such as those just described are often accompanied by conceptual changes.

The discovery that certain entities do not exist may force the scientist to redescribe the events, processes, observations which were thought to be manifestations of them and were therefore described in terms assuming their existence. Or, rather, it may force him to use new concepts as the older words will remain in use for a considerable time. Thus the term "possessed" which was once used for giving a causal description of the behavioral peculiarities connected with epilepsy was retained, but it was voided of its devilish connotations.

An interesting development occurs when the faulty ontology is *comprehensive*, that is, when its elements are thought to be present in every process in a certain domain. In this case every description inside the do-

main must be changed and must be replaced by a different statement (or by no statement at all). Classical physics is a case in point. It has developed a comprehensive terminology for describing the most fundamental mechanical properties of our universe, such as shapes, speeds, and masses. The conceptual system connected with this terminology assumes that the properties *inhere* in objects and that they change only if one interferes with the objects, not otherwise. The theory of relativity teaches us, at least in one of its interpretations, that there are no such inherent properties in the world, neither observable, nor unobservable, and it produces an entirely new conceptual system for description inside the domain of mechanics. This new conceptual system does not just deny the existence of classical states of affairs, it does not even permit us to *formulate statements* expressing such states of affairs (there is no arrangement in the Minkowski diagram that corresponds to a classical situation). It does not, and cannot, share a single statement with its predecessor. As a result the formal conditions for a suitable successor of a refuted theory (it has to repeat the successful consequences of the older theory, deny its false consequences, and make additional predictions) cannot be satisfied in the case of relativity versus classical physics and the Popperian scheme of progress breaks down. It is not even possible to connect classical statements and relativistic statements by an *empirical hypothesis*.¹⁹⁷ Formulating such a connection would mean formulating statements of the type "whenever there is possession by a demon there is discharge in the brain" which perpetuate rather than eliminate the older ontology. Comprehensive theories of the kind just mentioned are therefore completely disjointed, or *incommensurable*. The existence of incommensurable theories provides another difficulty for critical rationalism (and, a fortiori, for its more positivistic predecessors). We shall discuss this difficulty by discussing and refuting objections against it.

It was pointed out that progress may lead to a complete replacement of statements (and perhaps even of descriptions) in a certain domain. More especially, it may replace certain natural interpretations by others. This case has already been discussed (see above, section 6). Galileo replaces the idea of the operative character of all motion by his relativity principle in order to accommodate the new views of Copernicus. It is entirely natural to proceed in this way. A cosmological theory such as the heliocentric theory, or the theory of relativity, or the quantum theory (though

the last one only with certain restrictions) makes assertions about the world as a whole. It applies to observed and to unobserved (unobservable, 'theoretical') processes. It can therefore demand to be used always, and not only on the theoretical level. Now such an adaptation of observation to theory, and this is the gist of the *first objection*, removes conflicting observation reports and saves the theory in an ad hoc manner. Moreover, there arises the suspicion that observations which are interpreted in terms of a new theory can no longer be used to refute that theory. It is not difficult to reply to these points.

As regards the objection we point out, in agreement with what has been said before (toward the end of section 4), that an inconsistency between theory and observation may reveal a fault of our *observational terminology* (and even of our sensations) so that it is quite natural to change this terminology, to adapt it to the new theory, and to see what happens. Such a change gives rise, and should give rise, to new auxiliary subjects (hydrodynamics, theory of solid objects, optics in the case of Galileo) which may more than compensate for the empirical content lost by the adaptation. And as regards the suspicion we must remember that the predictions of a theory depend on its postulates, the associated grammatical rules, as well as on initial conditions while the meaning of the "primitive" notions depends on the postulates (and the associated grammatical rules) only.¹⁹⁸ In those rare cases, however, where a theory entails assertions about possible initial conditions¹⁹⁹ we can refute it with the help of *self-inconsistent observation reports* such as "object A does not move on a geodesic" which, if analyzed in accordance with the Einstein-Infeld-Hoffmann account reads "singularity *a* which moves on a geodesic does not move on a geodesic."

The *second objection* criticizes the interpretation of science that brings about incommensurability. To deal with it we must realize that the question "are two particular comprehensive theories, such as classical celestial mechanics (CM) and the special theory of relativity (SR) incommensurable?" is not a complete question. Theories can be interpreted in different ways. They will be commensurable in some interpretations, incommensurable in others. Instrumentalism, for example, makes commensurable all those theories which are related to the same observation language and are interpreted on its basis. A realist, on the other hand, wants to give a unified account, both of observable and of unobservable matters, and he will use the most abstract terms of whatever theory he is contem-

plating for that purpose.²⁰⁰ This is an entirely natural procedure. SR, so one would be inclined to say, does not just invite us to rethink unobserved length, mass, duration; it would seem to entail the relational character of all lengths, masses, durations, whether observed or unobserved, observable or unobservable.

Now, and here we only repeat what was said not so long ago, 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 either of earlier theories or of the available alternatives. These earlier theories and alternatives will then all become incommensurable with T. The relation between SR and CM is a case in point. The concept of length as used in SR and the concept of length as presupposed in CM are different concepts. Both are *relational* concepts, and very complex relational concepts at that (just consider determination of length in terms of the wave length of a specified spectral line). But relativistic length, or relativistic shape, involves an element that is absent from the classical concept and is in principle excluded from it.²⁰¹ It involves the relative velocity of the object concerned in some reference system. It is of course true that the relativistic scheme very often yields numbers which are practically identical with the numbers obtained from CM, but this does not make the concepts more similar. Even the case $c \rightarrow \infty$ (or $v \rightarrow 0$) which yields identical predictions cannot be used as an argument for showing that the concepts must coincide, at least in this special case. Different magnitudes based on different concepts may give identical values on their respective scales without ceasing to be different magnitudes. The same remark applies to the attempt to identify classical mass with relativistic rest mass.²⁰² This conceptual disparity, if taken seriously, infects even the most "ordinary" situations. The relativistic concept of a certain shape, such as the shape of a table, or of a certain temporal sequence, such as my saying "Yes," will differ from the corresponding classical concept also. It is therefore futile to expect that sufficiently long derivations may eventually return us to the older ideas.²⁰³ The consequence classes of SR and CM are not related in any way. A comparison of content and a judgment of verisimilitude cannot be made.²⁰⁴

The situation becomes even clearer when we use the Marzke-Wheeler interpretation of SR. For it can be easily shown that the methods of measurement provided by these authors, while perfectly adequate in a relativ-

istic universe, either collapse or give nonsensical results in a classical world (length, for example, is no longer transitive, and in some coordinate systems it may be impossible to assign a definite length to any object²⁰⁵).

We are now ready to discuss the second and most popular objection against incommensurability. This objection proceeds from the version of realism described above. "A realist," we said, "will want to give a unified account, both of observable and of unobservable matters, and he will use the most abstract terms of whatever theory he is contemplating for his purpose." He will use such terms in order either to give meaning to observation sentences or else to *replace* their customary interpretation. (For example, he will use the ideas of SR in order to replace the customary CM-interpretation of everyday statements about shapes, temporal sequences, and so on.) Against this, it is pointed out that theoretical terms receive their interpretation by being connected with a preexisting observation language, or with another theory that has already been connected with such an observation language, and that they are devoid of content without such connection. Thus Carnap asserts²⁰⁶ that "[t]here is no independent interpretation for L_T [the language in terms of which a certain theory, or a certain world view, is formulated]. The system T [the axioms of the theory and the rules of derivation] is in itself an uninterpreted postulate system. [Its] terms . . . obtain only an indirect and incomplete interpretation by the fact that some of them are connected by the [correspondence] rules C with observation terms . . ." Now, if theoretical terms have no "independent interpretation," then surely they cannot be used for correcting the interpretation of the observation statements, which is the one and only source of their meaning. It follows that realism as described here is an impossible doctrine.

The guiding idea behind this very popular objection is that new and abstract languages cannot be introduced in a direct way, but must be first connected with an already existing, and presumably stable, observational idiom.²⁰⁷

This guiding idea is refuted at once by noting the way in which children learn to speak and in which anthropologists and linguists learn the unknown language of a newly discovered tribe.

The first example is instructive for other reasons also, for incommensurability plays an important role in the early months of human development. As has been suggested by Piaget and his school²⁰⁸ the child's perception develops through various stages before it reaches its relatively stable adult

form. In one stage objects seem to behave very much like afterimages,²⁰⁹ and they are treated as such. In this stage the child follows the object with his eyes until it disappears, and he does not make the slightest attempt to recover it, even if this would require but a minimal physical (or intellectual) effort, an effort, moreover, that is already within the child's reach. There is not even a tendency to search; and this is quite appropriate, "conceptually" speaking. For it would indeed be nonsensical to "look for" an afterimage. Its "concept" does not provide for such an operation.

The arrival of the concept and of the perceptual image of material objects changes the situation quite dramatically. There occurs a drastic re-orientation of behavioral patterns, and, so one may conjecture, of thought. Afterimages, or things somewhat like them, still exist, but they are now difficult to find and must be discovered by special methods. (The earlier visual world therefore *literally disappears*.) Such special methods proceed from a new conceptual scheme (afterimages occur in humans, not in the outer physical world, and are tied to them) and cannot lead back to the exact phenomena of the previous stage (these phenomena should therefore be called by a different name, such as "pseudo-afterimages"). Neither afterimages nor pseudo-afterimages are given a special position in the new world. For example, they are not treated as "evidence" on which the new notion of a material object is supposed to rest. Nor can they be used to explain this notion: afterimages arise together with it, and are absent from the minds of those who do not yet recognize material objects. And pseudo-afterimages disappear as soon as such recognition takes place. It is to be admitted that every stage possesses a kind of observational "basis" to which one pays special attention and from which one receives a multitude of suggestions. However, this basis (i) changes from stage to stage; and (ii) is part of the conceptual apparatus of a given stage; it is not its one and only source of interpretation.

Considering developments such as these, one may suspect that the family of concepts centering upon "material object" and the family of concepts centering upon "pseudo-afterimage" are incommensurable in precisely the sense that is at issue here. Is it reasonable to expect that conceptual and perceptual changes of this kind occur in childhood only? Should we welcome the fact, if it is a fact, that an adult is stuck with a stable perceptual world and an accompanying stable conceptual system which he can modify in many ways, but whose general outlines have forever become immobilized? Or is it not more realistic to assume that fun-

damental changes, entailing incommensurability, are still possible, and that they should be encouraged lest we remain forever excluded from what might be a higher stage of knowledge and of consciousness? (Cf. on this point again section 1, especially on the role of scientific and other revolutions in bringing about such a higher stage.) Besides, the question of the mobility of the adult stage is at any rate an empirical question, which must be attacked by research and which cannot be settled by methodological fiat. The attempt to break through the boundaries of a given conceptual system and to escape the reach of "Popperian spectacles" (Lakatos) is an essential part of such research (and should be an essential part of any interesting life).²¹⁰

Looking now at the second element of the refutation, anthropological field work, we see that what is anathema here (and for very good reasons) is still a fundamental principle for the contemporary representatives of the philosophy of the Vienna Circle. According to Carnap, Feigl, Nagel, and others, the terms of a theory receive their interpretation in an indirect fashion, by being related to a different conceptual system which is either an older theory or an observation language.²¹¹ This older theory, this observation language, is not adopted because of its theoretical excellence. It cannot possibly be: the older theories are usually refuted. It is adopted because it is "used by a certain language community as a means of communication."²¹² According to this method, the phrase "having much larger relativistic mass than . . ." is partially interpreted by first connecting it with some prerelativistic terms (classical terms, common-sense terms), which are "commonly understood" (presumably, as the result of previous teaching in connection with crude weighing methods), and it is used only after such connection has given it a well-defined meaning.

This is even worse than the once quite popular demand to clarify doubtful points by translating them into Latin. For while Latin was chosen because of its precision and clarity, and also because it was conceptually richer than the slowly evolving vulgar idioms,²¹³ the choice of an observation language or of an older theory as a basis for interpretation is justified by saying that they are "antecedently understood": the choice is based on sheer popularity. Besides, if prerelativistic terms which are pretty far removed from reality (especially in view of the fact that they come from an incorrect theory implying a nonexistent ontology) can be taught ostensively, for example, with the help of crude weighing methods (and one must assume that they can be so taught, or the whole scheme collapses),

then why should one not introduce the *relativistic* terms *directly*, and *without* assistance from the terms of some other idiom? Finally, it is but plain common sense that the teaching or the learning of new and unknown languages must not be contaminated by external material. Linguists remind us that a perfect translation is never possible, even if one is prepared to use complex contextual definitions. This is one of the reasons for the importance of *field work* where new languages are learned *from scratch*, and for the rejection, as inadequate, of any account that relies on 'complete' or 'partial' translation. Yet just what is *anathema in linguistics* is taken for granted by *logical empiricism*, a mythical "observation language" replacing the English of the translators. Let us commence field work in this domain also, and let us study the language of new theories not in the definition factories of the double language model, but in the company of those metaphysicians, theoreticians, playwrights, courtesans who have constructed new world views! This finishes my discussion of the guiding principle behind the second objection against realism and the possibility of incommensurable theories.

Another point that is often made is that there exist *crucial experiments* which refute one of two allegedly incommensurable theories and confirm the other (example: the Michelson-Morley experiment, the variation of the mass of elementary particles, the transverse Doppler effect, are said to refute CM and confirm SR). The answer to this problem is not difficult either: adopting the point of view of relativity, we find that the experiments, *which of course will now be described in relativistic terms*, using the relativistic notions of length, duration, speed, and so on,²¹⁴ are relevant to the theory. And we also find that they support the theory. Adopting CM (with, or without an ether), we again find that the experiments, which are now described in the very different terms of classical physics, i.e., roughly in the manner in which Lorentz described them, are relevant. But we also find that they *undermine* CM, i.e., the conjunction of classical electrodynamics and of CM. Why should it be necessary to possess terminology that allows one to say that it is the *same* experiment which confirms one theory and refutes the other? But did we not ourselves use such terminology? Well, for one thing it should be easy though somewhat laborious to express what was just said *without* asserting identity. Secondly, the identification is of course not contrary to our thesis, for we are now not using the terms of either relativity or classical physics, as is done in a test, but are *referring* to them and their relation to the physi-

cal world. The language in which *this* discourse is carried out can be classical, or relativistic, or ordinary. It is no good insisting that scientists act as if the situation were much less complicated. If they act that way, then they are either instrumentalists (see above) or mistaken (many scientists are nowadays interested in *formulas*, while the subject here is *interpretations*). It is also possible that being well acquainted with both CM and SR, they change back and forth between these theories with such speed that they seem to remain within a single domain of discourse.

It is also said that by admitting incommensurability into science we can no longer decide whether a new view explains what it is supposed to explain, or whether it does not wander off into different fields.²¹⁵ For example, we would not know whether a newly invented physical theory is still dealing with problems of space and time or whether its author has not by mistake made a biological assertion. But there is no need to possess such knowledge. For once the fact of incommensurability has been admitted, the question which underlies the objection does not arise. Conceptual progress often makes it impossible to ask certain questions and to explain certain things; thus we can no longer ask for the absolute velocity of an object, at least as long as we take relativity seriously. Is this a serious loss for science? Not at all! Progress was made by the very same "wandering off into different fields" whose undecidability now so greatly exercises the critic: Aristotle saw the world as a super *organism*, as a *biological* entity, while one essential element of the new science of Descartes, Galileo, and their followers in medicine and in biology is its exclusively *mechanistic* outlook. Are such developments to be forbidden? And if they are not, what, then, is left of the complaint?

A closely connected objection starts from the notion of *explanation* or *reduction* and emphasizes that this notion presupposes continuity of concepts; other notions could be used for starting exactly the same kind of argument. (Relativity is supposed to explain the valid parts of classical physics; hence it cannot be incommensurable with it!) The reply is again obvious. As a matter of fact it is a triviality for anyone who has only the slightest acquaintance with the Hegelian philosophy: why should the relativist be concerned with the fate of classical mechanics except as part of a historical exercise? There is only one task we can legitimately demand of a theory, and it is that it should give us a correct account of the world, i.e., of the totality of facts as seen *through its own concepts*. What have the principles of explanation got to do with this demand? Is it not rea-

sonable to assume that a point of view such as the point of view of classical mechanics that has been found wanting in various respects, that gets in difficulty *with its own facts* (see above, on crucial experiments), and must therefore be regarded as self-inconsistent (another application of Hegelian principles!), cannot have entirely adequate concepts? Is it not equally reasonable to try replacing its concepts with those of a more promising cosmology? Besides, why should the notion of explanation be burdened by the demand for conceptual continuity? This notion has been found to be too narrow before (demand of derivability), and it had to be widened so as to include partial and statistical connections. Nothing prevents us from widening it still further and admitting, say, "explanations by equivocation."

Incommensurable theories, then, can be refuted by reference to their own respective kinds of experience, i.e., by discovering the *internal contradictions* from which they are suffering (in the absence of commensurable alternatives these refutations are quite weak, however²¹⁶). Their *content* cannot be compared, nor is it possible to make a judgment of *verisimilitude* except within the confines of a particular theory. None of the methods which Popper (or Carnap, or Hempel, or Nagel) want to use for rationalizing science can be applied, and the one that can be applied, refutation, is greatly reduced in strength. What remains are esthetic judgments, judgments of taste, and our own subjective wishes.²¹⁷ Does this mean that we are ending up in subjectivism? Does this mean that science has become arbitrary, that it has become an element of the general relativism which so much exercises the conscience of some philosophers? Well, let us see.

14. The Choice between Comprehensive Ideologies

To start with, it seems to me that an enterprise whose human character can be seen by all is preferable to one that looks "objective" and impervious to human actions and wishes.²¹⁸ The sciences, after all, are our own creation, including all the severe standards they seem to impose on us. It is good to be constantly reminded of this fact. It is good to be constantly reminded of the fact that science as we know it today is not inescapable, and that we can construct a world in which it plays no role whatever. (Such a world, I venture to suggest, would be more pleasant to behold than the world we live in today, both materially and intellectually.) What better reminder is there than the realization that the choice between theo-

ries which are sufficiently general to yield a comprehensive world view and which are empirically disconnected may become a matter of taste? *That the choice of a basic cosmology may become a matter of taste?*

Secondly, matters of taste are not completely beyond the reach of argument. Poems, for example, can be compared in grammar, sound structure, imagery, rhythm, and can be evaluated on such a basis (cf. Ezra Pound on progress in poetry²¹⁹). Even the most elusive mood can be analyzed *and should be analyzed* if the purpose is to present it in a manner that either can be enjoyed or increases the emotional, cognitive, perceptual, etc., inventory of the reader. Every poet who is worth his salt compares, improves, argues until he finds the correct formulation of what he wants to say.²²⁰ Would it not be marvelous if this free and entertaining²²¹ process played a role in the sciences also?

Finally, there are more pedestrian ways of explaining the same matter which may be somewhat less repulsive to the tender ears of a professional philosopher of science. One may consider the *length* of derivations leading from the principles of a theory to its observation language, and one may also draw attention to the number of *approximations* made in the course of the derivation. All derivations must be standardized for this purpose so that unambiguous judgments of length can be made. (This standardization concerns the form of the derivation, it does not concern the *content*.) Smaller length and smaller number of approximations would seem to be preferable. It is not easy to see how this requirement can be made compatible with the demand for simplicity and generality which, so it seems, would tend to increase both parameters. However that may be, there are many ways open to us once the fact of incommensurability is understood, and taken seriously.

15. Conclusion

The idea that science can and should be run according to some fixed rules, and that its rationality consists in agreement with such rules, is both unrealistic and vicious. It is *unrealistic*, since it takes too simple a view of the talents of men and of the circumstances which encourage, or cause, their development. And it is *vicious*, since the attempt to enforce the rules will undoubtedly erect barriers to what men might have been, and will reduce our humanity by increasing our professional qualifications. We can free ourselves from the idea and from the power it may possess over us (i) by a detailed study of the work of revolutionaries such as Galile-

leo, Luther, Marx, or Lenin; (ii) by some acquaintance with the Hegelian philosophy and with the alternative provided by Kierkegaard; (iii) by remembering that the existing separation between the sciences and the arts is artificial, that it is a side effect of an idea of professionalism one should eliminate, that a poem or a play can be intelligent as well as informative (Aristophanes, Hochhuth, Brecht), and a scientific theory pleasant to behold (Galileo, Dirac), and that we can change science and make it agree with our wishes. We can turn science from a stern and demanding mistress into an attractive and yielding courtesan who tries to anticipate every wish of her lover. Of course, it is up to us to choose either a dragon or a pussycat as our companion. So far mankind seems to have preferred the latter alternative: "The more solid, well defined, and splendid the edifice erected by the understanding, the more restless the urge of life . . . to escape from it into freedom." We must take care that we do not lose our ability to make such a choice.

Appendix. Science without Experience

1. One of the most important properties of modern science, at least according to some of its admirers, is its *universality*: any question can be attacked in a scientific way leading either to an unambiguous answer or else to an explanation of why an answer cannot be had. Let us therefore ask whether the *empirical hypothesis* is correct, i.e., whether experience can be regarded as a true source and foundation (testing ground) of knowledge.

2. Asking this question and expecting a scientific answer assumes that a science *without* experience is a *possibility*, that is, it assumes that the idea is neither absurd nor self-contradictory. It must be possible to imagine a natural science without sensory elements, and it should perhaps also be possible to indicate how such a science is going to work.

3. Now experience is said to enter science at three points: testing; assimilation of the results of test; understanding of theories.

A test may involve complex machinery and highly abstract auxiliary assumptions. But its final outcome has to be recognized by a human observer who looks at some piece of apparatus and notices some observable change. Communicating the results of a test also involves the senses: we hear what somebody says to us; we read what somebody has written down. Finally, the abstract principles of a theory are just strings of signs, without relation to the external world unless we know how to connect them

with experiment and that means, according to the first item on the list, with experience, involving simple and readily identifiable sensations.

4. It is easily seen that experience is needed at none of the three points just mentioned.

To start with, experience does not need to enter the process of *test*: we can put a theory into a computer, provide the computer with suitable instruments directed by him (her, it) so that relevant measurements are made which return to the computer leading there to an evaluation of the theory. The computer can give a simple yes-no response from which a scientist may *learn* whether or not a theory has been confirmed without having in any way *participated* in the test (i.e., without having been subjected to some relevant *experience*).

5. Learning what a computer says means being informed about some simple occurrence in the macroscopic world. *Usually* such information travels through the senses giving rise to distinct sensations. But this is not always the case. Subliminal perception leads to reactions directly, and without sensory data. Latent learning leads to memory traces directly, and without sensory data. Posthypnotic suggestion leads to (belated) reactions directly, and without sensory data. In addition there is the whole unexplored field of telepathic phenomena. I am not asserting that the natural sciences as we know them today could be built on these phenomena alone and could be freed from sensations entirely. Considering the peripheral nature of the phenomena and considering also how little attention is given to them in our education (we are not trained to effectively use our ability for latent learning) this would be both unwise and impractical. But the point is made that sensations are not *necessary* for the business of science and that they occur for practical reasons only.

6. Considering now the objection that we understand our theories, that we can *apply* them only because we have been told how they are connected with experience, one must point out that experience arises *together* with theoretical assumptions, *not* before them, and that an experience without theories is just as uncomprehended as is (allegedly) a theory without experience: eliminate part of the theoretical knowledge of a sensing subject and you have a person who is completely disoriented and incapable of carrying out the simplest action. Eliminate further knowledge and his sensory world (his "observation language") will start disintegrating, even colors and other simple sensations will disappear until he is in a stage even more primitive than a small child. A small child, on the other

hand, does not possess a stable perceptual world which he uses for making sense of the theories put before him. Quite the contrary. He passes through various perceptual stages which are only loosely connected with each other (earlier stages disappear when new stages take over) and which embody all the theoretical knowledge achieved at the time. Moreover, the whole process (including the very complex process of learning up to three or four languages) gets started only because the child reacts correctly toward signals, interprets them correctly, because he possesses means of interpretation even before he has experienced his first clear sensation. Again we can imagine that this interpretative apparatus acts without being accompanied by sensations (as do all reflexes and all well-learned movements such as typing). The theoretical knowledge it contains certainly can be applied correctly, though it is perhaps not understood. But what do sensations contribute to our understanding? Taken by themselves, i.e., taken as they would appear to a completely disoriented person, they are of no use, either for understanding or for action. Nor is it sufficient to just link them to the existing theories. This would mean extending the theories by further elements so that we obtain longer expressions, i.e., longer series of events, not the understanding of the shorter expressions which we wanted. No—the sensations must be incorporated into our behavior in a manner that allows us to pass smoothly from them into action. But this returns us to the earlier situation where the theory was applied but allegedly not yet understood. Understanding in the sense demanded here thus turns out to be ineffective and superfluous. Result: sensations can be eliminated from the process of understanding also (though they may of course continue to accompany it, just as a headache accompanies deep thought).

NOTES

1. V. I. Lenin, *'Left Wing' Communism, an Infantile Disorder* (Peking: Foreign Language Press, 1965), p. 100 (the book was first published in 1919 in order to criticize certain puritanical elements in German communism). Lenin speaks of parties and the revolutionary vanguard rather than of scientists and methodologists. The lesson is, however, the same.
2. H. Butterfield, *The Whig Interpretation of History* (New York: Norton, 1965), p. 66.
3. *Ibid.*, p. 21.
4. *Ibid.*, p. 25.
5. Lenin, *'Left Wing' Communism*, p. 100. It is interesting to see how a few substitutions can turn a political lesson into a lesson for methodology which, after all, is part of the process by means of which we move from one historical stage to another. We

also see how an individual who is not intimidated by traditional boundaries can give useful advice to everyone, philosophers of science included. Cf. notes 27 and 33, 35, 38.

6. P. A. Schilpp, ed., *Albert Einstein, Philosopher-Scientist* (Evanston, Ill.: Tudor, 1948), p. 683.

7. D. Hume, *A Treatise of Human Nature* (Oxford: Oxford University Press, 1888), p. 180.

8. Popper and his followers distinguish between the socio-psychological process of science where errors abound and rules are constantly broken and a "third world" where knowledge is changed in a rational manner, and without interference from "mob psychology," as Lakatos expresses himself. For details and a criticism of this poor man's Platonism see the text to note 194 below.

9. E. H. Carr, *Michael Bakunin* (London: Macmillan, 1937), pp. 8–9.

10. Thus external pressure is replaced by bad conscience, and freedom remains restricted as before. Marx describes a similar development in the case of Luther in the following words: ". . . Luther eliminates external religiousness and turns religiousness into the inner essence of man . . . he negates the raving parish-priest outside the layman, for he puts him right into his heart." *Nationalökonomie und Philosophie*; quoted from Marx, *die Frühschriften*, ed. S. Landshut (Stuttgart: Kroner, 1953), p. 228.

Whatever remains of irrationality in history is suppressed by the quasi-historical and indeed quite mythological manner in which scientists describe the genesis of their discoveries, or of the discoveries of others. ". . . history is wholly subordinated to the needs of the present, and indeed only survives to such an extent, and in such form, as serves present needs." Among the present needs, however, the propagation of what is thought to be good science is the most important one. Hence, history is replaced by myths "which are to be consonant with what [one thinks] to be good physics, and they are to be internally consistent." Paul Forman, "The Discovery of the Diffraction of X-Rays by Crystals: A Critique of the Myths," *Archive for the History of the Exact Sciences*, 6 (1969), 68–69. Forman's paper presents an interesting example to illustrate this statement. Another example is the myths which have been invented to explain the origin of the special theory of relativity. For an excellent account with plentiful sources see G. Holton, "Einstein, Michelson, and the 'Crucial' Experiment," *Isis*, 60 (1969), 133–197.

11. "This unique prevalence of the inner logic of a subject over and above the outer influences is not . . . to be found at the beginning of modern science." H. Blumenberg, *Die Kopernikanische Wende* (Frankfurt: Suhrkamp, 1965), p. 8.

12. "Nothing is more dangerous to reason than the flights of the imagination . . ." Hume, *A Treatise of Human Nature*, p. 267.

13. An expert is a man or a woman who has decided to achieve excellence in a narrow field at the expense of a balanced development. He has decided to subject himself to standards which restrict him in many ways, his style of writing and the patterns of his speech included, and he is prepared to conduct most of his waking life in accordance with these standards (this being the case, it is likely that his dreams will be governed by these standards, too). He is not averse to occasionally venturing into different fields, to listen to fashionable music, to adopt fashionable ways of dressing (though the business suit still seems to be his favorite uniform, in this country and abroad), or to seduce his students. However, these activities are aberrations of his private life; they have no relation whatever to what he is doing as an expert. A love for Mozart, or for *Hair*, will not make his physics more melodious, or give it a better rhythm. Nor will an affair make his chemistry more colorful.

This separation of domains has very unfortunate consequences. Not only are special subjects voided of ingredients which make a human life beautiful and worth living, but these ingredients are impoverished, too, emotions become crude and thoughtless, just as thought becomes cold and inhumane. Indeed, the private parts of one's existence suffer much more than does one's official capacity. Every aspect of professionalism has its watchdogs; the slightest change, or threat of a change, is examined, broadcast, warn-

ings are issued, and the whole depressing machinery moves at once in order to restore the status quo. Who takes care of the quality of our emotions? Who watches those parts of our language which are supposed to bring people together more closely, which have the function of giving comfort, understanding, and perhaps a little personal criticism and encouragement? There are no such agencies. As a result professionalism takes over even here.

To mention some examples:

In 1610 Galileo reported for the first time his invention of the telescope and the observations he made with it. This was a scientific event of the first magnitude, far more important than anything we have achieved in our megalomaniac twentieth century. Not only was here a new and very mysterious instrument introduced to the learned world (it was introduced to the learned world, for the essay was written in Latin), but this instrument was at once put to a very unusual use: it was directed toward the sky; and the results, the astonishing results, quite definitely seemed to support the new theory which Copernicus had suggested over sixty years earlier, and which was still very far from being generally accepted. How does Galileo introduce his subject? Let us hear.

"About 10 months ago a report reached my ears that a certain Dutchman had constructed a spyglass by means of which visible objects, though very distant from the eye of the observer, were distinctly seen, as if nearby. Of this truly remarkable effect several experiences were related, to which some persons gave credence while others denied them. A few days later the report was confirmed to me in a letter from a noble Frenchman in Paris, Jacques Badovere, which caused me to apply myself wholeheartedly to enquire into the means by which I might arrive at the invention of a similar instrument . . ." Quoted from Stillman Drake, ed., *Discoveries and Opinions of Galileo* (New York: Doubleday Anchor Books, 1957), pp. 28-29.

We start with a personal story, a very charming story, which slowly leads us to the discoveries, and these are reported in the same clear, concrete, and colorful way: "There is another thing," writes Galileo, describing the face of the moon, "which I must not omit, for I beheld it not without a certain wonder; this is that almost in the center of the moon there is a cavity larger than all the rest, and perfectly round in shape. I have observed it near both the first and last quarters, and have tried to represent it as correctly as possible in the second of the above figures . . ." Quoted from Drake, ed., *Discoveries and Opinions of Galileo*, p. 36. Galileo's drawing attracts the attention of Kepler who was one of the first to read Galileo's essay. He comments: "I cannot help wondering about the meaning of that large circular cavity in what I usually call the left corner of the mouth. Is it a work of nature, or of a trained hand? Suppose that there are living beings on the moon (following the footsteps of Pythagoras and Plutarch I enjoyed toying with this idea, long ago . . .). It surely stands to reason that the inhabitants express the character of their dwelling place, which has much bigger mountains and valleys than our earth has. Consequently, being endowed with very massive bodies, they also construct gigantic projects . . ." Quoted from Kepler's *Conversations with Galileo's Sideral Messenger*, trans. Edward Rosen (New York: Johnson Reprint Corporation, 1965), pp. 27-28.

"I have observed"; "I have seen"; "I have been surprised"; "I cannot help wondering"; "I was delighted"—this is how one speaks to a friend or, at any rate, to a live human being.

The awful Newton who more than anyone else is responsible for the plague of professionalism from which we suffer today starts his first paper on colors in a very similar style: ". . . in the beginning of the year 1666 . . . I procured me a triangular glass prisme, to try therewith the celebrated phenomena of colours. And in order thereto having darkened my chamber, and made a small hole in my window shuts, to let in a convenient quantity of the sun's light, I placed my prisme at its entrance, that it might be thereby refracted to the opposite wall. It was at first a very pleasing divertisement, to view the vivid and intense colours produced thereby; but after a while applying myself to consider them more circumspectly, I became surprised to see them in an oblonge

form . . ." Quoted from *The Correspondence of Isaac Newton*, vol. I (Cambridge: Cambridge University Press, 1959), p. 92.

Remember that all these reports are about cold, objective, "inhuman" inanimate nature, they are about stars, prisms, lenses, the moon, and yet these are described in a most lively and fascinating manner, communicating to the reader an interest and an excitement which the discoverer felt when first venturing into strange new worlds.

Now compare with this the introduction to a recent book, a best seller even, *Human Sexual Response* by W. H. Masters and V. E. Johnson (Boston: Little, Brown, 1966). I have chosen the book for two reasons. First, because it is of general interest. It removes prejudices which influence not only the members of some profession, but the everyday behavior of a good many apparently "normal" people. Second, because it deals with a subject that is new and without special terminology. Also, it is about man rather than about stones and prisms. So one would expect a beginning even more lively and interesting than that of Galileo, or Kepler, or Newton. What do we read instead? Behold, oh patient reader: "In view of the perversicacious gonadal urge in human beings, it is not a little curious that science develops its sole timidity about the pivotal point of the physiology of sex. Perhaps this avoidance . . ." and so on. This is human speech no more. This is the language of the expert.

Note that the subject has completely left the picture. Not "I was very surprised to find" or, since there are two authors, "We were very surprised to find," but "It is surprising to find"—only not expressed in these simple terms. Note also to what extent irrelevant technical terms intrude and fill the sentences with antediluvian barks, grunts, squeaks, belches. A wall is erected between the writers and their readers not because of some lack of knowledge, not because the writers do not know their readers, but in order to make utterances conform to some curious professional ideal of objectivity. And this ugly, inarticulate, and inhuman idiom turns up everywhere, and takes over the function of the most simple and the most straightforward description.

Thus on page 65 of the book we hear that the female, being capable of multiple orgasm, must often masturbate after her partner has withdrawn in order to complete the physiological process that is characteristic for her. And, so the authors want to say, she will stop only when she gets tired. This is what they want to say. What they actually say is: "usually physical exhaustion alone terminates such an active masturbatory session." You don't just masturbate; you have an "active masturbatory session." On the next page the male is advised to ask the female what she wants or does not want rather than try to guess it on his own. "He should ask her"—this is what our authors want to convey. What is the sentence that actually lies there in the book? Listen: "The male will be infinitely more effective if he encourages vocalization on her part." "Encourages vocalization" instead of "asks her"—well, one might want to say, the authors want to be precise, and they want to address their fellow professionals rather than the general public and, naturally, they have to use a special lingo in order to make themselves understood. Now as regards the first point, precision, remember that they also say that the male will be "infinitely more effective" which, considering the circumstances, is not a very precise statement of the facts. And as regards the second point we must say that we are not dealing with the structure of organs, or with special physiological processes which might have a special name in medicine, but with an ordinary affair such as asking. Besides, Galileo and Newton could do without a special lingo although the physics of their time was highly specialized and contained many technical terms. They could do without a special lingo because they wanted to start afresh and because they were sufficiently free and inventive not to be dominated by words, but to be able to dominate them. Masters and Johnson find themselves in the same position, but they cannot speak straight any more, their linguistic talents and sensibilities have been distorted to such an extent that one asks oneself whether they will ever be able to speak normal English again.

The answer to this question is contained in a little pamphlet which came into my hands and which contains the report of an ad hoc committee formed for the purpose

of examining rumors of police brutality during some rather restless weeks in Berkeley (winter 1968–69). The members of the committee were all people of good will. They were interested not only in the academic quality of life on campus; they were even more interested in bringing about an atmosphere of understanding and of compassion. Most of them came from sociology and from related fields, that is, they came from fields which deal not with lenses, stones, stars, as did Galileo in his beautiful little book, but with humans. There was a mathematician among them who had devoted considerable time to setting up and defending student-run courses and who finally gave up in disgust—he could not change the “established academic procedures.” How do these nice and decent people write? How do they address those to whose cause they have devoted their spare time and whose lives they want to improve? Are they able to overcome the boundaries of professionalism at least on this occasion? Are they able to *speak*? They are not.

The authors want to say that policemen often make arrests in circumstances when people are bound to get angry. They say: “When arousal of those present is the inevitable consequence.” “Arousal”; “inevitable consequence”—this is the lingo of the laboratory, this is the language of people who habitually mistreat rats, mice, dogs, rabbits, and carefully notice the effects of their mistreatment, but the language they use is now applied to humans, too, to humans, moreover, with whom one sympathizes, or says one sympathizes, and whose aims one supports. They want to say that policemen and strikers hardly talk to each other. They say: “Communication between strikers and policemen is nonexistent.” Not the strikers, not the police, not people are at the center of attention, but an abstract process, “communication,” about which one has learned a thing or two and with which one feels more at ease than with living human beings. They want to say that more than 80 people took part in the venture, and that the report contains the common elements of what about 30 of them have written. They say: “This report tries to reflect a consensus from the 30 reports submitted by the 80 plus faculty observers who participated.” Need I continue? Or is it not already clear that the effects, the miserable effects, of professionalism are much deeper and much more vicious than one would expect at first sight? That some professionals have even lost the ability to *speak* in a civilized manner, that they have returned to a state of mind more primitive than that of an eighteen-year-old who is still able to adapt his language to the situation in which he finds himself, talking the lingo of physics in his physics class and quite a different language with his friends in the street (or in bed)?

Many colleagues who agree with my general criticism of science find this emphasis on language farfetched and exaggerated. Language, they say, is an *instrument* of thought that does not influence it to the extent I surmise. This is true as long as a person has different languages at his disposal, and as long as he is still able to switch from one to another as the situation demands. But this is not the case here. Here a single and rather impoverished idiom takes over all functions and is used under all circumstances. Does one want to insist that the thought that hides behind *this* ugly exterior has remained nimble and humane? Or must be not rather agree with V. Klemperer and others who have analyzed the deterioration of language in fascistic societies that “words are like small doses of arsenic: they are swallowed unawares, they do not seem to have any noticeable effect, and yet the poisonous influence will be there after some time. If someone frequently enough replaces words such as ‘heroic’ and ‘virtuous’ by ‘fanatical’ he will believe in the end that without fanaticism there is no heroism and no virtue.” *Die Unbewaeltigte Sprache* (Munich: Deutscher Taschenbuch Verlag, 1969), p. 23. Similarly the frequent use of abstract terms from abstract disciplines (“communication”; “arousal”) in subjects dealing with humans is bound to make people believe that a human being can be dissolved into a few bland processes and that things such as emotion and understanding are just disturbing elements, or, rather, misconceptions belonging to a more primitive stage of knowledge.

In their search for a bland and standardized language with uniform spelling, punctuation, standardized references, and so on experts receive increasing support from *publishers*. Idiosyncrasies of style and expression that have been overlooked by a referee will certainly be noticed by printers or editors, and much energy is wasted in quarrels over a phrase, or the position of a comma. It seems that language has ceased to be the property of writers and readers and has been purchased by publishing houses, so that authors are no longer allowed to express themselves as they see fit and to make their contribution to the growth of English.

14. John Stuart Mill, *On Liberty*, quoted from *The Philosophy of John Stuart Mill*, ed. Marshall Cohen (New York: Modern Library, 1961), p. 258.

15. *Ibid.*, p. 265.

16. Even in undetermined and ambiguous situations uniformity of action is soon achieved, and adhered to tenaciously. Cf. M. Sherif, *The Psychology of Social Norms* (New York: Harper Torchbooks, 1964).

17. *A Treatise of Human Nature*, p. xxii. The word “reason” has been replaced by “understanding” in order to establish coherence with the terminology of the German idealists.

18. The first part of the quotation, up to “appearing as,” is taken from *Differenz des Fichte’schen und Schelling’schen Systems der Philosophie*, ed. G. Lasson (Hamburg: Felix Meiner, 1962), p. 13. The second part is from the *Wissenschaft der Logik*, vol. I (Hamburg: Felix Meiner, 1965), p. 6.

19. Letter to Gert Micha Simon of October 11, 1949. Quoted from *Gottfried Benn, Lyrik und Prosa, Briefe und Dokuments* (Wiesbaden: Limes Verlag, 1962), p. 235.

20. For details and further literature see “Problems of Empiricism, Part II,” in *The Nature and Function of Scientific Theory*, ed. R. G. Colodny (Pittsburgh: University of Pittsburgh Press, 1970).

21. One of the few physicists to see and to understand this feature of the development of scientific knowledge was Niels Bohr: “. . . he would never try to outline any finished picture, but would patiently go through all the phases of the development of a problem, starting from some apparent paradox, and gradually leading to its elucidation. In fact, he never regarded achieved results in any other light than as starting points for further exploration. In speculating about the prospects of some line of investigation, he would dismiss the usual considerations of simplicity, elegance or even consistency with the remark that such qualities can only be properly judged *after* [my italics] the event . . .” L. Rosenfeld in S. Rozental, ed., *Niels Bohr, His Life and Work as Seen by His Friends and Colleagues* (New York: Interscience, 1967), p. 117.

One must of course realize that science does not achieve final results and that it is therefore always “before” the event, never “after” it. Simplicity, elegance, consistency are therefore never a *conditio sine qua non* of scientific knowledge.

Considerations like these are usually criticized by the childish remark that a contradiction entails every statement and that self-inconsistent views are therefore useless for science. I call the remark childish because it assumes that a self-consistent science is a realistic possibility, that the rule which leads to the result just mentioned is the only possible rule, and that the scientist is obliged to play the thinking games of the logician. There is of course no such obligation. Quite the contrary, the scientist can criticize the logician for providing him with inadequate instruments that make nonsense of the complex, delicate, and often self-inconsistent theories he uses.

For further information concerning Bohr’s philosophy see my essay “On a Recent Critique of Complementarity,” *Philosophy of Science*, 35 (1968), 309–331, and 36 (1969), 82–105. The essay also cites relevant literature.

22. Children “learn to imitate others . . . and so learn to look upon standards of behavior as if they consisted of fixed, ‘given’ rules . . . and such things as sympathy and imagination may play an important role in this development . . .” K. R. Popper, *The Open Society and Its Enemies* (New York: Harper Torchbooks, 1967), II, 390.

One should also compare the remainder of appendix i/15 which gives a clear account of the irrational elements in our knowledge.

23. In one of his numerous lucubrations in praise of Ordinary English ("Moore and Ordinary Language," in *The Philosophy of G. E. Moore*, ed. P. A. Schilpp, New York: Tudor, 1952, pp. 354ff) Malcolm makes the following comment: ". . . if a child who was learning the language were to say, in a situation where we were sitting in a room with chairs about, that it was 'highly probable' that there were chairs there, we should smile, and correct his language" (italics in the original). One can only hope that the children whom Malcolm addresses in this manner are not as gullible as are most of his students and that they will retain their intelligence, their imagination, and especially their sense of humor in the face of this and other "methods" of education.

24. Cf. below, text to note 208.

25. Commenting on his early education by his father, and especially on the explanations he received on matters of logic, J. S. Mill made the following observations: "The explanations did not make the matter at all clear to me at the time; but they were not therefore useless; they remained as a nucleus for my observations and reflections to crystallize upon; the import of his general remarks being interpreted to me, by the particular instances which came under my notice afterwards." *Autobiography* (London: Oxford University Press, 1963), p. 16. In "Problems of Empiricism, Part II" I have argued that the development of science exhibits phase differences of precisely this kind. A strange and incomprehensible new principle often serves as a "nucleus for observations and reflections to crystallize upon" until we obtain a theory that is understood even by the most uneducated empiricist. For a general discussion of the problem touched upon in this remark, see Hegel, *Wissenschaft der Logik*, I, 51-64. See also St. Augustine, *De doctrina Christiana*, 11/9: "The first . . . case is to know these books [i.e., the books of the old and of the new testament]. Altogether, we may not yet understand them, but by reading we can either memorize them, or become somehow acquainted with them." The way in which apparently aimless talk may lead to new ideas and to a new state of consciousness has been described, briefly, but exquisitely, by Heinrich von Kleist, "Ueber die allmaehliche Verfertigung der Gedanken beim Reden," in Hans Meyer, ed., *Meisterwerke Deutscher Literaturkritik* (Stuttgart: Golvers, Neue Bibliothek der Weltliteratur, 1962), 741-747.

26. "Recourse to direct action changed the whole tenor of the struggle, for the workers' self-confidence is enormously increased (and their knowledge transformed) once they act without delegating any of their power to political parties or trade unions. 'The factory is ours so do we need to start working for the bosses again?' This idea arose quite spontaneously, not by command, or under the aegis of the so-called vanguard of the proletariat [with its special methods, rules, prescriptions, and its special idea of rationality], but simply as a natural response to a concrete situation." D. Cohn-Bendit, *Obsolete Communism: The Left Wing Alternative*, trans. A. Pomerans (London: André Deutsch, 1968), p. 67. Cohn-Bendit's emphasis on "spontaneity . . . The chief enemy of all bureaucrats" (p. 154) agrees with the tenor of the present paper which wants to eliminate excessive bureaucracy not only from government, but also from the administration of knowledge (where it appears as an appeal to rationality). For the formation of natural responses to ambiguous situations, see also Sherif, *The Psychology of Social Norms*.

27. (A) K. R. Popper, whose views I have in mind when criticizing the omnipresence of argument, has admitted that "rationalism is necessarily far from comprehensive or self-contained." *The Open Society and Its Enemies*, II, 231. But the question I am asking is not whether there are limits to our reason. The question is where these limits are situated. Are they outside the sciences, so that science itself remains entirely rational (though the decision to become scientific may be an irrational decision); or are irrational changes an essential part of even the most rational enterprise that has been invented by man? Does the historical phenomenon 'science' contain ingredients which defy a rational analysis? Can the abstract goal of coming closer to the

truth be reached in an entirely rational fashion, or is it perhaps inaccessible to those who decide to rely on argument only? These are the questions to which I want to address myself in the present essay.

(B) Surprising insights into the limitations of methodological rules as well as into their dependence on a certain developmental stage of mankind are found in Lenin's and Mao's political writings and, of course, in Hegel's philosophy. It needs only a little imagination to turn the positive advice contained in these writings into advice for the scientist, or the philosopher of science.

Thus, we read on pp. 40ff of Lenin's *'Left Wing' Communism* (a book that is very useful as a theoretical basis for the criticism of contemporary left radicalism, campus radicals, leftist puritans, and other leftovers from the undialectical political stone age): "we can (and must) begin to build socialism, not with imaginary human material [as does the doctrine of critical rationalism], nor with human material specially prepared by us [as do all Stalinists, in politics as well as in the philosophy of science], but with the [quite specific] human material bequeathed to us by capitalism. True, that is very 'difficult,' but no other approach to this task is serious enough to warrant discussion." Replace "socialism" by "rationality of the future," "capitalism" by "critical rationalism," and our case is stated with perfect clarity.

It seems to me that such attention to the wider political context will free the philosopher of science from the Nagel-Carnap-Popper-Kuhn carrousel. The only philosopher who secretly imbibes the forbidden brew of Leninism is Lakatos—and the results are evident in his magnificent work. All that is required now is that he confess his vices openly so that others may learn to delight and enlighten us in a similar way.

(C) An excellent example of the need for moving forces in addition to argument is provided by the history of witchcraft in the thirteenth to seventeenth centuries. "No mere skepticism, no mere 'rationalism,' could have driven out the old cosmology," writes H. Trevor-Roper in his analysis (*The European Witch Craze*, New York: Harper Torchbooks, 1969, p. 181). "A rival faith had been needed . . ." Despite all the arguments against it "the intellectual basis of the witch craze remained firm all through the seventeenth century. No critic had improved on the arguments of Weyer; none had attacked the substance of the myth . . ." (pp. 160-161). Such attacks did not occur, and they could not have been effective. They could not have been effective, because the science of the schools was "empirically confirmed" (p. 191), because it "created its own evidence" (p. 166), because it was firmly rooted in common belief (p. 124), leading to strong experiences, to "illusions" which were "centralised . . . around" the main characters of the dominating myth such as for example "the devil" (p. 125), and because strong emotional forces were expressed by the myth as well. The existence of the empirical evidence made it difficult to argue against witchcraft in a "scientific" manner. The existence of the emotional force would have neutralized even an effective scientific counterargument. What was needed was not simply a formal criticism, or an empirical criticism; what was needed was a change of consciousness, a "rival faith" as Trevor-Roper expresses himself, and this rival faith had to be introduced against tremendous odds, and even in the face of reason. Now it is of course correct that a general and forceful education in the rules of rationalism, dogmatic, critical, or otherwise, will make it more easy for arguments to win the day—well-trained dogs heel more promptly than do their anarchistic counterparts—but the discussion of the value of argument will now be considerably more difficult, and perhaps entirely impossible. Besides, man was not meant to be just a rational animal. At any rate he was not meant to be castrated and cut apart. But whatever our position on that head, we shall have to admit that rational argument works with rational people only and that an appeal to rational argument is therefore discriminatory. Rational people are specially prepared, they have been conditioned in special ways, their freedom of action and of thought has been considerably restricted. If we oppose discrimination and mental restriction, then the omnipresence of reason can no longer be guaranteed and our assertion in the text holds. Cf. also Burr's letter to A. D. White, quoted from *George Lincoln Burr, His Life and*

Selected Writings (Ithaca, N.Y.: Cornell University Press, 1943), p. 56, my italics: "To my thought—and here I differ widely from both Buckle and Lecky . . . —it was not science, not reason that put an end to inhumanity in so many fields: the pedants were as cruel as the bigots. Reason came in only to sanction here reforms which had been wrought in spite of her. *The real antagonist of theology and of rationalism alike* [and it does not make any difference whether we speak here of dogmatic rationalists, or of skeptics, or of critical rationalists as is shown by the example of Glanville] was the unreasoning impulse of human kindness."

(D) The example of witchcraft shows that the wider context we need in order to see science, or the "search for truth," in perspective need not be politics. It can be religion, metaphysics, theology, or what have you. In "Classical Empiricism" (in R. E. Butts, ed., *The Methodological Heritage of Newton*, Toronto: University of Toronto Press, 1970), I have linked developments of science with developments in theology and I have commented on the wider perspective of the theologians when compared with that of the scientists. Today, of course, politics are much more popular. Besides, Professor Imre Lakatos, the secretary general of the slowly disintegrating Popperian party, is a politician first, and a theologian only much much later, and he knows Lenin better than he knows St. Thomas. This is why I have taken my extrascientific quotations from revolutionary politics, and not from revolutionary theology (besides, everyone has by now forgotten that St. Thomas was a revolutionary, too).

28. According to Popper we do not "need any . . . definite frame of reference for our criticism"; we may revise even the most fundamental rules and drop the most fundamental demands if the need for different measures of excellence should arise. *The Open Society and Its Enemies*, II, 390.

29. "No new progressive epoch has ever defined itself by its own limitations . . . In our case, however, watching the boundaries is regarded as more virtuous than transcending them." Speech of Milan Kundera at the IVth Congress of Czech Authors, Prague, June 1967. Quoted from *Reden Zum IV. Kongress des Tschechoslowakischen Schriftstellerverbandes* (Frankfurt: Suhrkamp, 1968), p. 17. "Our case" is of course also the case of revolutionary developments in science and methodology. In his introduction to the German translation of Burke's writings on the French Revolution, Gentz comments in a similar vein (quoted from P. G. Gooch, *Germany and the French Revolution*, London: Longmans, 1920, p. 95): ". . . the eulogist of new systems always finds opinion on his side [optimist!], while the defender of the old must [read: will] appeal to reason." The "opinion" of today is, of course, the "reason" of tomorrow which is already present in a naive, immediate, undeveloped form.

30. Leon Trotsky, *The Revolution Betrayed*, trans. M. Eastman (Garden City, N.Y.: Doubleday, 1937), pp. 86–87.

31. The priority of idea over behavior, problem over physical adaptation, brain over body—these are other versions of the ideology I am criticizing, and all of them have been refuted by more recent research. Thus the discovery of the australopithecines confronted us with a being that combines the brain of an ape with nearly human dentition, posture, and (possibly), behavior. Such a combination "was not anticipated in earlier speculation" (George G. Simpson et al., *Life: An Introduction to College Biology*, New York: Harcourt, Brace, 1957, p. 793) where it was assumed that it is the brain that is responsible for the remaining human features and not the other way around: man became erect, he started using his hands, because his brain told him so. Today, we must admit that a new posture leading to new tasks may "create" the brain needed for these tasks (this, essentially, was also Engels's conjecture in his little essay about the function of the hand in the humanization of our apelike ancestors).

It also seems that certain comprehensive features of early civilization such as domestication or agriculture did not arise as attempts to solve problems. Rather "man at play inadvertently discovered their practical use." F. Alexander, *Fundamentals of Psychoanalysis* (New York: International Universities Press, 1948), p. 113; cf. also G. Róheim, *The Origin and Function of Culture* (New York: Nervous and Mental Disease Mono-

graphs, 1943), pp. 40–47, on the origin of the economic activity of mankind, and *Psychoanalysis and Anthropology* (New York: International Universities Press, 1950), p. 437, on the reasons why parents take care of their children. This is most easily proved by the fact that wool in sheep, a surplus of milk in cows, an abundant amount of eggs laid by fowls are all a consequence of domestication and cannot have acted as a reason for it. Hahn (*Die Haustiere in ihrer Beziehung zur Gesellschaft des Menschen*, Leipzig: Johann Ambrosius Barth, 1896, pp. 79, 154, 300, paraphrased after R. H. Lowie, *The History of Ethnological Theory*, New York: Farrar and Reinhart, 1937, pp. 112ff) suggests that people kept poultry originally as alarm clocks, or for cockfights—both non-economic motives. He also suggests that early man was an idler, doing useful labor as a pastime, rather than with serious and problem-conscious intent. Q. H. Schultz ("Some Factors Influencing the Social Life of Primates in General and of Early Man in Particular," in S. L. Washburn, ed., *Social Life of Early Man*, Chicago: Aldine, 1961, p. 63) says: "It was no radical innovation for Dawn man to use their hands for picking up rocks or clubs as ready defence to overcome the lack of large teeth. Nearly every captive macaque delights in carrying new objects around its cage, and apes are entertained for hours by a blanket, or a bucket which they will not let out of their hands without a fight" (my italics).

Wherever we look we see a happy and playful activity leading to accidental solutions of unrealized problems. We do not see serious problem-conscious thinkers engaged in the attempt to intellectually discuss and then properly solve the problems they have set up. Later on the sequence is of course inverted by postulating either a divine inventor or a problem situation to which the minds of the contemporaries are supposed to have found the appropriate solution. Such an intellectualistic account is neither correct nor helpful for it prevents us from improving unknown faults of our situation in a spontaneous way and it also prevents us from recognizing such faults in retrospect, after their removal has made their substance clear. By all means, let us be rational: But let us not make the mistake of believing that man can and should improve his lot by reasoned planning only.

32. Cf. notes 22 and 25.

33. I cannot believe that a revolution such as the French Revolution occurred "in the full consciousness of [the] rights [which people possess] as men and citizens" as Wilhelm von Humboldt expresses himself (quoted from Gooch, *Germany and the French Revolution*, p. 109), or that a revolution such as the Copernican Revolution proceeded in the full consciousness of the ideas and methods, and with a full understanding of the instruments about (i.e., within the next 300 years) to be invented. In all these cases the element of action—unreasonable, nonsensical, mad, immoral action when seen from the point of view of a contemporary—is a necessary presupposition of whatever clarity one would like to possess, but can achieve only *after the event*, as the result of the actions performed. For material from the history of science see my "Problems of Empiricism, Part II," especially sections 7, 8, 11.

In politics and religion the point just made implies the need for (mass) action in addition to (party) doctrine, even if the doctrine should happen to contain definite and absolutely clear rules of procedure. For such rules, while clear and complete when compared with other rules, are always woefully inadequate vis-à-vis the ever changing multitude of social conditions. (In physics the situation is exactly the same: the formalism of the elementary quantum theory is a monster of beauty and precision. But it is very difficult to exactly specify experimental arrangements capable of measuring even the simplest observable. Here we must still rely on the correspondence principle.) But it is just to such conditions that their content must be referred and in the process 'anarchistic' action, i.e., action that is directly related neither to theory nor to the existing institutions, plays an essential part: "We cannot tell . . . what immediate cause will most serve to rouse [a revolution], kindle it, and impel very wide masses [of scientists, for example] who are at present dormant into the struggle . . . History gen-

erally, and the history of revolutions in particular, is always richer in content, more varied, more many-sided, more lively and 'subtle' than even the best parties and the most class conscious vanguards of the most advanced classes imagine . . . From this follow two very important practical conclusions: first, that in order to fulfil its task the revolutionary class must be able to master all forms, or sides of social activity without exception . . . second, that the revolutionary class must be ready to pass from one form to another in the quickest and most unexpected manner." Lenin, *'Left Wing' Communism*, p. 100. Cf. also the text to note 5. The application to science is quite straightforward if we keep the proper rules of translation (note 27(B)) in mind. Cohn-Bendit, *Obsolete Communism*, gives a vivid account of an anarchism of the kind. "Problems of Empiricism, Part II" applies the lesson to science. Cf. also notes 35 and 38.

[Addition in fall 1969: I now prefer the label of *Dadaism* to that of *anarchism*. There is not much difference between the two procedures *theoretically* (for partial argument see my essay "The Theatre as an Instrument of the Criticism of Ideologies," *Inquiry*, 10 (1967), 298-310, especially footnote 12 and text). But an anarchist is prepared to kill while a Dadaist would not hurt a fly. The only thing he does hurt is the "professional conscience" of the defenders of the status quo which at any rate must be exposed to discomfort if one wants to find its limits and if one wants to move beyond them. The necessity for mass action (interruption of "professional meetings," for example) is not denied—but it must be restricted by a dogmatic respect for human lives and by a somewhat less dogmatic respect for the views of the opposition.]

In *philosophy* this point implies the dependence of theoretical structure on individual action and individual decision: Kierkegaard's analysis of the Ethical applies to the sciences as well. See note 35.

34. The phrase "magical" is quite appropriate, for the inclusion of well-formed observational reports was demanded in books on magic, down to Agrippa's *De occulta philosophia*.

35. Our understanding of ideas and concepts, says Hegel (*Gymnasialreden*; quoted from K. Loewith and J. Riedel, eds., *Hegel, Studienausgabe*, vol. I, Frankfurt: Fischer Bücherei, 1968, p. 54), starts with "an uncomprehended knowledge of them" ("Es ist damit derselbe Fall wie mit anderen Vorstellungen und Begriffen, deren Verstehen gleichfalls mit einer unverständenen Kenntnis anfaengt . . ."). Cf. also *Logik*, I, 39-40. "It sometimes happens that at a new turning point of a movement, theoretical absurdities cover up some practical truth." Lenin, diary note at the Stuttgart Conference of the Second International, quoted from Bertram D. Wolfe, *Three Who Made Revolution* (Boston: Beacon, 1948), p. 599.

The ideas which are needed in order to explain and to justify a certain procedure in the sciences are often created only by the procedure itself and remain unavailable if the procedure is not carried out. This shows that the element of action and faith which some believe has been eliminated from the sciences is absolutely essential for it: "Even intellectual history, we now admit, is relative, and cannot be dissociated from the wider social context with which it is in constant interaction." Trevor-Roper, *The European Witch Craze*, p. 100. "We are here up against an extremely interesting historical and philosophical phenomenon," writes Ronchi in his discussion of Galileo and the telescope ("Complexities, Advances, and Misconceptions in the Development of the Science of Vision: What Is Being Discovered?" in A. C. Crombie, ed., *Scientific Change*, London: Heinemann, 1963, p. 552), "which illustrates the possible harm that can be caused by logic and reason [i.e., by the exclusive use of well-established ideas and rational methods] while pure faith—for all its unreasonableness—may bring about the most fruitful results."

It is also interesting to note to what extent Kierkegaard's ideas about the role of faith, passion, subjectivity apply to our scientific life (provided, of course, we are interested in fundamental discoveries, and not just in the preservation of the status quo, in methodology, and elsewhere). Cf. *Concluding Unscientific Postscript*, trans. David

F. Swensen and Walter Lowrie (Princeton, N.J.: Princeton University Press, 1941), especially chapter II: "Truth as Subjectivity." Kierkegaard emphasizes the process over the result. "While objective thought translates everything into results and helps all mankind to cheat, by copying these off and reciting them by rote, subjective thought puts everything in process and omits the result; partly, because this belongs to him who has the way, partly because as an existing individual he is constantly in process of coming to be which holds true of every human being who has not permitted himself to be deceived into becoming objective, inhumanly identifying himself with speculative philosophy in the abstract [for example, with the rules of critical rationalism]" (p. 68). One might add that the results of objective thought which are supposed to give reason to everything emerge only at the end of a long process which therefore will have to occur without reason and will have to be passed through on faith only: The "rationality" of the early Royal Society, to take but one example, was entirely a matter of faith.

Kierkegaard's thought has had a decisive influence on Bohr (for material see M. Jammer, *The Conceptual Development of Quantum Mechanics*, New York: McGraw-Hill, 1966, pp. 172ff). It could be used, in conjunction with material from the history of science, to help us construct a new methodology which takes into consideration the role of the individual thinker, not just because he is there, and because his fate should be of interest to us, but because even the most dehumanized and "objective" form of science could not exist without his unreasonable and humorless passionate efforts. Cf. also note 27.

36. H. Marcuse, *Reason and Revolution* (London: Oxford University Press, 1941), p. 130. The quotation is about Hegel's logic.

37. Cf. note 18.

38. "It would be absurd to formulate a recipe or general rule . . . to serve all cases. One must use one's own brains and be able to find one's bearings in each separate case." Lenin, *'Left Wing' Communism*, p. 64. Cf. also note 27(B).

The reader should remember that despite all my praise for Marxism and its various proponents I am defending its *anarchistic* elements only and that I am defending those elements only insofar as they can be used for a criticism of epistemological and moral rules. I quote Lenin because of his insight into the complexity of historical conditions (which is incomparably superior to the insight of scientists and of philosophers of science) and because he recommends an appropriately complex method. I recommend Luxemburg because in elaborating her method she has always the individual before her eyes (one cannot say the same about Sir Karl Popper). I quote Mao because he is prepared to abandon doctrine, to experiment, even in quite fundamental matters. However, I do not quote these authors because of their defense of a uniform society of the future, or because of their belief in inexorable laws of history (in the case of Lenin the latter belief is present in a more critical form, for it is connected with *potentialities* rather than with actual developments). Such a society, such laws, it seems to me, would be even less attractive than the "system" of today whose dogmatism has the advantage of being tempered by dishonesty, doubt, cowardice, and indolence.

Some of my friends have chided me for elevating a statement such as "anything goes" into a fundamental principle of epistemology. They did not notice that I was joking. Theories of knowledge as I conceive them *develop*, like everything else. We find new principles, we abandon old ones. Now there are some people who will accept an epistemology only if it has some stability, or "rationality" as they are pleased to express themselves. Well, they can have such an epistemology, and "anything goes" will be its only principle.

39. "Problems of Empiricism," in *Beyond the Edge of Certainty*, ed. R. G. Colodny (Englewood Cliffs, N.J.: Prentice-Hall, 1965), sections IVff, especially section VI. (The relevant material has been reprinted in P. H. Nidditch, ed., *The Philosophy of Science*, London: Oxford University Press, 1969, pp. 12ff, especially pp. 25-33.) "Realism and Instrumentalism," in *The Critical Approach to Science and Philosophy*, ed. M. Bunge (Glencoe, Ill.: Free Press, 1964). "Reply to Criticism," in *Boston Studies*

in the *Philosophy of Science*, vol. II, ed. R. S. Cohen and M. W. Wartofsky (New York: Humanities, 1965).

40. Looking back into history we see that progress, or what is regarded as progress today, has almost always been achieved by counterinduction. Thales' principle according to which there is unity behind the variety of appearances lies at the bottom of all science, ancient and modern. Yet it is contradicted by observations of the most primitive kind (change; the difference between air and iron, for example). The same applies, and to an even larger extent, to Parmenides' principle of the impossibility of all motion. (Even a rationalist like Popper now feels inclined to attack Parmenides on empirical grounds.) The modern interpretation of mental illness as being due not to the action of some external spiritual principle but to autonomous disturbances of the sick organism ran counter to numerous instances where the action of such a principle was both felt (split personality, hearing voices, forced movement, objective appearance of emotions and dreams, nightmares, etc.) and objectively observed (phantom pregnancy, disintegration of speech patterns). Denying the power of the devil in these times was almost as foolish as (or, considering the threat of hellfire, much more foolish than) denying the existence of material objects is regarded today. Then, Copernicus put forth his magnificent hypothesis and upheld it in the face of plain and indubitable experience (for literature see the reference in note 20). Even Newton, who explicitly advises against the use of alternatives for hypotheses which are not yet contradicted by experience and who invites the scientist not merely to guess, but to deduce his laws from "phenomena" (cf. his famous rule IV), can do so only by using as "phenomena" laws which are inconsistent with the observations at his disposal. (As he says himself: "In laying down . . . Phenomena, I neglect those small and inconsiderable errors." Sir Isaac Newton's *Mathematical Principles of Natural Philosophy and His System of the World*, trans. A. Motte, rev. F. Cajori, Berkeley: University of California Press, 1953, p. 405.) For a more detailed analysis of Newton's dogmatic philosophy and of his dialectical method see my paper "Classical Empiricism."

Yet all these lessons are in vain. Now as ever counterinduction is ruled out by methodology. "The Counterinductive rule," says W. Salmon in his essay "The Foundation of Scientific Inference" (*Mind and Cosmos*, ed. R. G. Colodny, Pittsburgh: University of Pittsburgh Press, 1966, p. 185), is "demonstrably unsatisfactory." He fails to explain how the application of a "demonstrably unsatisfactory" rule can lead to so many satisfactory results which could not have been obtained in any other way.

41. "Fantasy as encountered in many people today is split off from what the person regards as his mature, sane, rational, adult experience. We do not then see fantasy in its true function, but experienced merely as an intrusive, sabotaging, infantile nuisance." R. D. Laing, *The Politics of Experience* (New York: Ballantine, 1967), p. 31.

Laing restricts his discussion of experience and of fantasy to their effect upon interpersonal relations (p. 23: "here, however, I am concentrating upon what we do to ourselves and to each other"). Fantasy, for him, is "a particular way of relating to the [social] world" (p. 31), telling us of problems, abilities, wishes which have become suppressed. The domain of natural science, the physical universe, remains unaffected.

But why should we restrict ourselves to rebuilding man's perception of society and of his fellow men? Why should we be interested in social reform alone and consider only new pictures of society? Is the structure of our physical world to be taken for granted? Are we expected to meekly accept the fact that we are living in a lousy material universe, that we are alone in a great ocean of lifeless matter? Or should we not try to change our vision of this universe, too, by leaving the domain of orthodox physics and considering more charming cosmologies? (The only alternative is to become mechanical oneself—this is the path chosen by some scientists, astronauts, and other strange beings.) Proliferation (revival of astrology, witchcraft, magic, alchemy, elaboration of Leibnitz's *Monadology*, and so on) will be a powerful guide in these matters. Psychiatrists and sociologists, however, must not rest content with changing perception

and society. They must interfere with the physical world and contemplate its reform in terms of our fantasies.

42. Those who want to consider the psychological consequences of proliferation will have to distinguish between intraindividual proliferation (plurality of world views within one and the same individual) and interindividual proliferation (plurality of world views in society, each individual holding only a single view and developing it according to his talents and his drive).

Intraindividual proliferation may in extreme cases lead to multiple personality. If we believe the teaching of psychoanalysis then there are always at least two elements present, the ego and the ego ideal, and the latter is ambivalent, being the result of the Oedipus complex. Freud, *Das Ich und das Es* (Leipzig-Vienna-Zurich: Internationaler Psychoanalytischer Verlag, 1923), p. 40. It is this ambivalence which turns the elements against each other, contributes to the development of both, and creates the dynamics of the individual. (In an animal which is also guided by different principles, for example by different instincts, the principles are not in competition but work peacefully side by side, each becoming active in specific circumstances only: G. Róheim, *Psychoanalysis and Anthropology*, p. 430.) This participation of various elements in any particular human action explains the "increase of flexibility, as compared to the animal world"; it explains why man "is the only organism normally and inevitably subjected to psychological conflict" (J. Huxley, *The Uniqueness of Man*, London: The Mall, 1941, p. 22); but it also explains why human behavior always presents "a mild case of insanity" (Róheim, *Psychoanalysis and Anthropology*, p. 442). The situation is further complicated by teachers, deans, bosses, and other authorities "who perpetuate the role of the father, whose demands and restrictions have remained active in the ego-ideal, and now act as moral censors in the form of our conscience" (Freud, *Das Ich und das Es*, p. 44). Imposing such a multiplicity of demands with merciless insistence, with a great amount of moralistic grumbling, threatening, headshaking, is bound to lead to crises in the life of the individual so treated and to extreme actions. "There are . . . disastrous choices [such] as those which confronted young people who felt that the service of God demanded forswearing the world forever, as in the Middle Ages, or cutting off one's finger as a religious offering, as among the Plains Indians." M. Mead, *Coming of Age in Samoa* (New York: Morrow, 1961), p. 200. Are we forced to renounce pluralism in favor of happiness and a balanced development?

I do not think we are driven to those extremes. Proliferation produces crises only if the chosen alternatives are played against each other with a vengeance. "The organization of science," writes R. K. Merton ("Behavior Patterns of Scientists," *American Scholar*, 38 (Spring 1969), 220), "operates as a system of institutionalized vigilance, involving competitive cooperation. It affords both commitment and reward for finding where others have erred or have stopped before tracking down the implications of their results, or have passed over in their work what is there to be seen by the fresh eye of another. In such a system, scientists are at the ready to pick apart and appraise each new claim to knowledge. This unending exchange of critical judgment, of praise and punishment, is developed in science to a degree that makes the monitoring of children's behavior by their parents seem little more than child's play." 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, conventionalization of "a lack of deep feeling" (Mead, *Coming of Age in Samoa*, p. 7; cf. also p. 35: "and with this goes the continual demand that [one] should not be too efficient, too outstanding, too precocious. [One] must never excel his fellows by more than a little," my italics)—and a good deal of laziness—and we shall be able to have our cake: to have freedom of choice in practical as well as in intellectual matters—and to eat it: to have this freedom without too much mental or emotional strain. This is one of the reasons why I regard

the moralism of today, whether it is now found on the right, with the defenders of "The System," or on the left, with the "New Revolutionaries," whether it carries with itself the invitation to "search for the truth," or the admonition to pursue some practical aim, as one of the most vicious ideologies invented by man.

43. *Autobiography* (London: Oxford University Press, 1963), p. 215. Many people are inclined to call Mill a liberal and to dismiss him because of the weaknesses of the liberal creed they have perceived. This is somewhat unjust, for Mill is very different indeed from much that is called "liberalism" today. He is a radical in many ways. Even as a radical, however, he excels by his rationality and his humanity. Cf. R. Lichtman, "The Façade of Equality in Liberal Democratic Theory," *Inquiry*, 12 (1969), 170–208.

44. For one particular element of this plurality, see K. R. Popper, "Back to the Presocratics," *Conjectures and Refutations* (New York: Basic Books, 1962), p. 136.

45. "Coleridge," in Cohen, ed., *The Philosophy of John Stuart Mill*, p. 62. (Numbers in parentheses in the text are pages in this edition.) ". . . I had to learn that I would recognize the value of health even in sickness, the value of rest through exertion, the spiritual through deprivation of material things . . . through evil the value of good . . . I suppose all that I ever tried to teach is expressed in these words." Sybil Leek, *Diary of a Witch* (New York: Quadrangle, 1969), pp. 49, 122.

46. Cf. also my essay "Outline of a Pluralistic Theory of Knowledge and Action," in *Planning for Diversity and Choice*, ed. S. Anderson (Cambridge, Mass.: MIT Press, 1968), which establishes the connection with scientific method alluded to toward the end of the last section.

For the relation between idea and action see the text to note 31. Emphasis on action within a libertarian framework plays an important role in Cohn-Bendit, *Obsolete Communism*, especially chapter V, p. 254: "Every small action committee [in the customary political language of the West: every institution, however small], no less than every mass movement [every large institution, including government bodies, etc.] which seeks to improve the lives of all men must resolve: (i) to respect and guarantee the plurality and diversity of political currents [in the widest sense of including scientific theories and other ideologies] . . . It must accordingly grant minority groups [such as witches, to mention only one example] the right of independent action—only if the plurality of ideas is allowed to express itself in social practice does this idea have any real meaning." In addition Cohn-Bendit demands flexibility and a democratic base for all institutions: "all delegates are accountable to, and subject to immediate recall by those who have elected them . . ." For example, one must "oppose the introduction of specialists and specialization" and one must "struggle against the formation of any kind of hierarchy" including the hierarchies of our educational institutions, universities, schools of technology, and so on. As regards knowledge the task is "to ensure a continuous exchange of ideas, and to oppose any control of information and knowledge." It seems to me that the best starting point in our attempt to remove the still existing fetters to thought and action is a combination of Mill's general ideas and of a practical anarchism such as that of Cohn-Bendit. Such a combination produces an ideology and a people that refuses to be intimidated, or restricted, by specialist knowledge (including the specialist knowledge disseminated by our contemporary critical rationalists), that tries to reform the corresponding institutions, especially those graceless safe-deposit boxes of wisdom, our universities, and that encourages the free flow of individuals from position to position ("[N]o function must be allowed to petrify or become fixed . . . the commander of yesterday can become a subordinate tomorrow"—Bakunin, quoted after James Joll, *The Anarchists*, London: Eyre and Spottiswoode, 1964, p. 109), assuring at the same time that no *every position in society is equally attractive, and is treated with equal respect*. Let not one say that science, being purely theoretical, has nothing to do with action and politics. The scientist whose results are received with respect and even with fear by the rest of the community and whose "methods" are eagerly imitated lives in a peculiar and often quite constipated environment. It has its own style (cf. note 13), its own rules, its own silly jokes, its own standards of 'integrity' which are likely to poison the

whole republic unless special preventive measures (elimination of specialists from positions of power: careful supervision of the educational process so that personal or group idiosyncrasies do not become a national malaise; and absolute distrust of expert testimony and of expert morality) are taken. The connection between theory and politics must always be considered.

47. For the propagandistic function of medieval art, see Rosario Assunto, *Die Theorie des Schoenen im Mittelalter* (Cologne: DuMont Schauberg, 1963), especially pp. 21–22.

48. "Ideological struggle," says Mao Tse-Tung ("On the Correct Handling of Contradictions among the People," quoted from *Four Essays on Philosophy*, Peking: Foreign Language Press, 1966, p. 116), "is not like other forms of struggle. The only method to be used in this struggle is that of painstaking reasoning and not crude coercion." ". . . the growth of new things may be hindered in the absence of deliberate suppression simply through lack of discernment. It is therefore necessary to be careful about questions of right and wrong in the arts and sciences, to encourage free discussion and avoid hasty conclusions. We believe that such an attitude can help to ensure a relatively smooth development of the arts and sciences" (p. 114). "People may ask, since Marxism is accepted as the guiding ideology by the majority of the people in our country, can it be criticised? Certainly it can . . . Marxists should not be afraid of criticism from any quarter. Quite the contrary, they need to temper and develop themselves and win new positions in the teeth of criticism and in the storm and stress of struggle . . . What should our policy be towards non-Marxist ideas? . . . Will it do to ban such ideas and deny them any opportunity for expression? Certainly not. It is not only futile but very harmful to use summary methods in dealing with ideological questions among the people . . . You may ban the expression of wrong ideas, but the ideas will still be there. On the other hand, if correct ideas are pampered in hothouses without being exposed to the elements or immunized from disease, they will not win out against erroneous ones. Therefore, it is only by employing the method of discussion, criticism and reasoning that we can really foster correct ideas and overcome wrong ones, and that we can really settle issues" (pp. 111–118). The similarity to Mill, whom Mao read in his youth, is remarkable.

It is to be noted that this advice is not put forth generally, but "in the light of China's specific conditions, on the basis of the recognition that various kinds of contradictions still exist in socialist society, and in response to the country's urgent need to speed up its economic and cultural development" (p. 113; see also p. 69, i.e., "On contradiction": ". . . we must make a concrete study of the circumstances of each specific struggle of opposites, and should not arbitrarily apply the formula to everything. Contradiction and struggle are universal and absolute, but the methods of resolving contradictions, that is, the forms of struggle, differ according to the differences in the nature of the contradictions"). Cf. also note 89.

Nor is freedom of discussion granted to everyone: "As far as unmistakable counter-revolutionaries and saboteurs of the socialist cause are concerned, the matter is easy: we simply deprive them of their freedom of speech." *Four Essays on Philosophy*, p. 117. (Cf. H. Marcuse, "Repressive Tolerance," in R. P. Wolff, B. Moore, Jr., H. Marcuse, *A Critique of Pure Tolerance*, Boston: Beacon Press, 1965, p. 100. Marcuse's case is quite interesting. He demands that certain powerful elements be excluded from the democratic debate. This assumes that he has the power to suppress them and to prevent them from speaking out and making themselves heard. Now, if he has this power, then he certainly has the power to make his own views better known, and he has also the power to educate people in the art of critical thinking. One wonders why he prefers to use an imaginary power which he does not yet possess but which he (or his wife) would certainly like to have, for suppressing opponents rather than for education and a more balanced discussion of views. Does he perhaps realize that well-educated people would never follow him, no matter how omnipresent his slogans and how seductive his presentation?)

The restriction occurs already in Mill, though with different reasons, and expressed in different terminology: "It is, perhaps, hardly necessary to say that this doctrine is meant to apply only to human beings in the maturity of their faculties . . . The early difficulties in the way of spontaneous progress are so great that there is seldom any choice of means for overcoming them; and a ruler full of the spirit of improvement is warranted in the use of any expedients that will attain an end perhaps otherwise unattainable. Despotism is a legitimate mode of government in dealing with barbarians, provided the end be their improvement and the means justified by actually effecting that end. Liberty, as a principle, has no application to any state of things anterior to the time when mankind have become capable of being improved by free and equal discussion. . . ." *On Liberty*, pp. 197–198; cf. Lenin, *'Left Wing' Communism*, p. 40: "We can (and must) begin to build socialism not with imaginary human material . . . but with the human material bequeathed to us . . ." The difference between Mill and Popper, however, seems to lie in this. For Mill the (material and spiritual) welfare of the individual, the full development of his capabilities, is the primary aim. The fact that the methods used for achieving this aim also yield a scientific philosophy, a book of rules concerning the 'search for the truth,' is a side effect, though a pleasant one. For Popper the search for the truth seems to be much more important and it seems occasionally to even outrank the interests of the individual. In this issue my sympathies are firmly with Mill.

49. This and similar remarks make it clear that Mill (and Popper, who follows Mill in all the respects so far enumerated) is not "dedicated to a national religion of skepticism, to the suspension of judgement" and that he does not "deny] the existence . . . not only of a public truth, but of any truth whatever," as we can read in Willmore Kendall's bombastic but uninformed essay "The 'Open Society' and Its Fallacies," *American Political Science Review*, 54 (1960), 972ff, quoted from P. Radcliff, ed., *Limits of Liberty* (Belmont, Calif.: Wadsworth, 1966), pp. 38 and 32. To refute the charge of suspension of judgment we should also consider this passage: "No wise man ever acquired his wisdom in any mode but this; nor is it in the nature of human intellect to become wise in any other manner. The steady habit of correcting and completing his own opinion by collating it with those of others, so far from causing doubt and hesitation in carrying it into practice, is the only stable foundation for a just reliance on it; for, being cognizant of all that can, at least obviously, be said against him, and having taken up his position against all gainsayers—knowing that he has sought for objections and difficulties instead of avoiding them, and has shut out no limit which can be thrown upon the subject from any quarter—he has a right to think his judgment better than that of any person, or any multitude, who have not gone through a similar process" (p. 209; my italics). Nor is the insinuation correct that Mill's society is, "so to speak, a debating club" (p. 36, italics in the original). Just think of Mill's insistence on different "experiments of living" (p. 249). Of course, such attention to detail is not to be expected from a self-righteous conservative for whom any discussion of freedom, and any attempt to achieve it, is but "evil teaching" (p. 35).

The possibilities of Mill's liberalism can be seen from the fact that it provides room for any human desire, and for any human vice. There are no general principles apart from the principle of minimal interference with the life of individuals, or groups of individuals who have decided to pursue a common aim. For example, there is no attempt to make the sanctity of human life a principle that would be binding for all. Those among us who can realize themselves only by killing humans and who feel fully alive only when in mortal danger are permitted to form a subsociety of their own where human targets are selected for the hunt, and are hunted down mercilessly, either by a single individual or by specially trained groups (for a vivid account of such forms of life see the film *The Tenth Victim*). So whoever wants to live a dangerous life, whoever wants to taste human blood, will be permitted to do so within the domain of his own subsociety. But he will not be permitted to implicate others; for example, he will

not be permitted to force others to participate in a "war of national honor," or what have you. He will not be permitted to cover up whatever guilt he may feel by making a potential murderer out of everyone. It is very strange to see how the general idea of the sanctity of human life that frowns upon simple, innocent, and rational murders such as the murder of a nagging wife by a henpecked husband does not object to the general murder of people one has not seen and with whom one has no quarrel. Let us admit that we have different tastes, let those who want to wallow in blood receive the opportunity to do so without giving them the power to make "heroes" of the rest of society. As far as I am concerned a world in which a louse can live happily is a better world, a more instructive world, a more mature world than a world in which a louse must be wiped out. (For this point of view see the work of Carl Sternheim; for a brief account of Sternheim's philosophy, see Wilhelm Emrich's Preface to C. Sternheim, *Aus dem Buergerlichen Heldenleben*, Neuwied: Hermann Luchterhand, 1969, pp. 5–19.) Mill's essay is a first step in the direction of constructing such a world.

It also seems to me the United States is very close to a cultural laboratory in the sense of Mill where different forms of life are developed and different modes of human existence tested. There are still many cruel and irrelevant restrictions, and excesses of so-called lawfulness threaten the possibilities which this country contains. However, these restrictions, these excesses, these brutalities occur in the brains of human beings; they are not all found in the Constitution. Accordingly, they can be removed by propaganda, enlightenment, special bills, personal effort (Ralph Nader!), and numerous other legal means. Of course, if such enlightenment is regarded as superfluous, if one thinks it irrelevant, if one assumes from the very beginning that the existing possibilities for change are either insufficient or condemned to failure, if one is determined to use "revolutionary" methods (methods, incidentally, which real revolutionaries, such as Lenin, would have regarded as utterly infantile, and which must increase the resistance of the opposition rather than removing it), then, of course, the "system" will appear much harder than it really is. It will appear harder because one has hardened it oneself, and the blame falls back on the bigmouth who calls himself a critic of society. It is depressing to see how a system that has much inherent elasticity is increasingly made less responsive by fascists on the Right and extremists on the Left until democracy disappears without ever having had a chance. My criticism, and my plea for anarchism, is therefore directed both against the traditional puritanism in science and society and against the "new," but actually age-old, antediluvian, primitive puritanism of the "new" Left which is always based on anger, on frustration, on the urge for revenge, but never on imagination. Restrictions, demands, moral arias, generalized violence everywhere. A plague on both your houses!

50. For a different argument which is entirely in Mill's spirit, see my "Problems of Empiricism," p. 185. Today increase of testability can be added to the list of epistemological benefits presented by Mill ("Problems of Empiricism," section vi). This is not a real addition, however, but only a more detailed and more technical presentation of ideas already developed by him.

51. This quotation has been added mainly for the benefit of Professor Herbert Feigl who keeps making fun of me for adopting extreme positions. Extreme positions are of extreme value. They induce the reader to think along different lines. They break his conformist habits. They are strong instruments for the criticism of what is established and well received. On the other hand, the current infatuation with "syntheses" and "dialogues" which are defended in the spirit of tolerance and of understanding can only lead to an end of all tolerance and of all understanding. To defend a "synthesis" by reference to tolerance means that one is not prepared to tolerate a view that does not show an admixture of one's own pet prejudices. To invite to a "dialogue" by reference to tolerance means inviting one to state one's views in a less radical and therefore mostly less clear way. An author who can write, in the spirit of "dialogue," that "Christianity and Marxism are not contrary to each other" (Guenther Nanning, quoted from the *Newsletter of the American Institute for Marxist Studies*, vol. 6, no. 1 (January–Febru-

ary 1969), first page bottom) will hardly be prepared to accept the doctrines of a tough-minded Marxist who is interested in progress, not in peace of mind.

52. In a singularly pretentious, ignorant, and narrow-minded book, *The Poverty of Liberalism* (Boston: Beacon, 1968), R. P. Wolff objects to proliferation on the grounds that it does not follow from the happiness principle. This criticism is certainly irrelevant to the thesis of *On Liberty*. The purpose of *On Liberty* is not to establish a proposition, be it now by reference to happiness, or in any other way; the purpose is to set an example, to present, explain, defend a certain form of life and to show its consequences in special cases (this becomes crystal clear from the relevant pages of the *Autobiography*). True—Mill also wrote on the happiness principle; but he was free and inventive enough not to restrict himself to a single philosophy, but to pursue different lines of thought. As a result maximum happiness plays no role in *On Liberty*. What does play a role is the free and unrestricted development of an individual. One can understand, however, why the author concentrates on happiness. This gives him the opportunity to display his knowledge (if one can call it that) of some of the tools which analytic “philosophers” have constructed for the endless discussion of hedonism.

In addition to the complaint just mentioned—for one can hardly call it an argument—Wolff offers what amounts to a series of rhetorical questions. “It is hard to believe,” he says (p. 17), “that even the most dedicated liberal will call for the establishing of chairs of astrology in our astronomy departments or insist that medical schools allow a portion of their curriculum to the exposition of chiropractice in order to strengthen our faith in the germ theory of disease.” This is hard to believe indeed, for our “most dedicated liberals” are often moral and intellectual cowards who would not dream of attacking that prize exhibit of the twentieth century—science. Besides, who would think that increasing the number of university chairs is going to lead to a more critical point of view? Are university chairs the only things a contemporary “radical philosopher” (text on front flap of the book) can think on when considering the possibilities of intellectual improvement? Are the limits of a university also the limits of the imagination of our academic radicals? If so, then the attack against Mill collapses at once, for how can a person with such a restricted point of view hope to even comprehend the simple nonacademic message of Mill’s philosophy?

“Does anyone suppose,” Wolff continues his inquiry (p. 16), “that a bright young physicist must keep his belief in quantum mechanics alive by periodically rehearsing the crucial experiments which gave rise to it?” Yes sir, there are lots of people who suppose just that, among them the founders of the quantum theory. There are lots of people who point out that science was often advanced with the help of some historical piece of knowledge and who explain the boorishness of much of contemporary physics by the very same lack of perspective which our radical author takes as the basis of his criticism. Of course, “no material harm” (p. 16) will come from the suppression of history and of alternatives just as brothels do not suffer from the philosophical ignorance of the whores—they flourish, and continue flourishing. But a philosophical courtesan certainly is preferable to a common broad because of the added techniques she can develop; and a science with alternatives is preferable to the orthodoxy of today for exactly the same reasons.

It is interesting to see how conservative so-called “radicals” become when confronted with the apparently more solid and more difficult parts of the establishment, such as for example science. Which again shows that they are moral cowards who dare to sing their arias only when there is absolutely no danger of a serious intellectual fight and when they can be absolutely sure of the support of what they think are the “progressive” elements of society.

53. Later in the nineteenth century proliferation was defended by evolutionary arguments: Just as animal species improve by producing variations and weeding out the less competitive variants, science was thought to improve by proliferation and criticism. Conversely, “well-established” results of science and even the “laws of thought” were regarded as temporary results of adaptation; they were not given absolute validity. Ac-

ording to Boltzmann (*Populaere Schriften*, Leipzig: Johann Ambrosius Barth, 1905, pp. 398, 318, 258–259), the latter “error finds its complete explanation in Darwin’s theory. Only what was adequate was also inherited. . . . In this way the laws of thought obtained an impression of infallibility that was strong enough to regard them as supreme judges, even of experience . . . One believed them to be irrefutable and perfect. In the same way our eyes and ears were once assumed to be perfect, too, for they are indeed most remarkable. Today we know that we were mistaken—our senses are not perfect.” Considering the hypothetical status of the laws of thought, we must “oppose the tendency to apply them indiscriminately, and in all domains” (p. 40). This means, of course, that there are circumstances, not factually circumscribed or determined in any other way, in which we must introduce ideas that contradict them. We must be prepared to introduce ideas inconsistent with the most fundamental assumptions of our science even before these assumptions have exhibited any weakness. Even “the facts” are incapable of restricting proliferation, for “there is not a single statement that is pure experience” (pp. 286, 222). Proliferation is important not only in science but in other domains too: “We often regard as ridiculous the activity of the conservatives, of those pedantic, constipated, and stiff judges of morality and good taste who anxiously insist on the observance of every and any ancient custom and rule of behavior; but this activity is beneficent and it must be carried out in order to prevent us from falling back into barbarism. Yet petrification does not set in, for there are also those who are emancipated, relaxed, the *hommes sans gêne*. Both classes of people fight each other and together they achieve a well-balanced society” (p. 322).

But Boltzmann does not always carry his ideas through to the end. Occasionally he relies on a more simplistic empiricism such as when he says that “a well-determined fact remains unchanged forever” (p. 343), or when he regards “my waking sensations [as] the only elements of my thought” (p. 173) so that “we infer the existence of objects from the impressions made on our senses” (p. 19), or when he declares, more than once, that the task of science is “to adapt our thoughts, ideas, and concepts to the given rather than subjecting the given to the judgment of the laws of thought” (p. 354; cf. with this the assertion, on p. 286, that “the simplest words such as yellow, sweet, sour, etc. which seem to represent mere sensations already stand for concepts which have been obtained by abstracting from numerous facts of experience”). He also warns us not to “go too far beyond experience.” This vacillation between a sound scientific philosophy and a bad positivistic conscience is characteristic of almost all so-called “realists” from Boltzmann up to, and including, Herbert Feigl. Reasons for this phenomenon are found in Lenin’s *Materialism and Empirio-Criticism* (New York: International Publishers, 1927). Popper’s theory of falsification which tells us why we can and should go as far beyond experience as possible has considerably improved the situation. All that is needed now is a little dialectics and attention to specific historical conditions (cf., for example, note 27(B)).

54. Popper, for example, takes it for granted that the subject cannot enter the domain of science, and he also uses a rather simple form of mechanical materialism in his attack on Bohr. For details see part II of “On a Recent Critique of Complementarity,” especially section 9. All these principles are used by him dogmatically, and without the shred of an argument. No Hegelian would ever proceed in such a simpleminded manner.

55. Cf. below, sections 12 and 13.

56. “Verhaeltnis des Skeptizismus zur Philosophie,” quoted from *Hegel, Studienausgabe*, I, 113; cf. also p. 112.

57. *Differenz des Fichte’schen und Schelling’schen Systems*, p. 13.

58. “Process becomes converted back to praxis, the patient becomes an agent.” Laing, *The Politics of Experience*, p. 35. There is a good deal of similarity between Hegel’s attempt to set concepts in motion and the attempts of some contemporary psychiatrists to return to the individual the control of some of the defense and projection mechanisms he has himself invented.

59. *Logik*, II, 61.

60. "Reflective reason . . . is nothing but the understanding which uses abstraction, separates, and insists that the separation be maintained and taken seriously." *Logik*, I, 26.

61. *Logik*, I, 82.

62. Cf. *Differenz*, p. 14.

63. Cf. the Carnap quotation, text to note 206.

64. *Logik*, I, 25.

65. *Encyclopaedie der Philosophischen Wissenschaften*, ed. G. Lasson (Leipzig: Teubner, 1920), pp. 72–73. In the original the reference is to Kant, not to scientific empiricism.

66. *Logik*, I, 25.

67. *Logik*, II, 211.

68. *Differenz*, p. 14. Cf. Lenin's comments on a similar passage in his notes on Hegel's *Logic*, quoted in V. I. Lenin, *Aus Dem Philosophischen Nachlass* (Berlin, 1949), pp. 136ff, especially p. 142.

69. Cf. also "Skepticismus," Hegel, *Studienausgabe*, p. 117: "that scepticism is intrinsically connected with every true philosophy." Also p. 118: "Where can we find a more perfect and independent document and system of true scepticism than in Plato's . . . Parmenides? Which embraces and destroys the whole domain of a knowledge achieved by the concepts of our understanding."

70. *Differenz*, p. 25.

71. "It is my aim to read Hegel in a materialistic fashion . . ." Lenin, *Nachlass*, p. 20. The same is true of Professor D. Bohm.

72. Cf. the note on the limit and the ought, *Logik*, I, 121–122: "Even a stone, being something, is differentiated into its being for itself and its Being and so it, too, transcends its limit . . . If it is a basis for acidification, then it can be oxidized, neutralized, and so on. In the process of oxidation, neutralization, etc. its limit, i.e., only to be a basis, is removed . . . and it contains the ought to such an extent that only force can prevent it from ceasing to be a basis . . ."

73. *Logik*, I, 71.

74. "Everything that exists is linked in this way to everything else: to the total process of the universe. This linkage is either direct, by means of a single quantum, or else indirect, through a series of such linkages." This is how Bohm describes (*Scientific Change*, ed. Crombie, p. 478) the situation created by the quantum theory. The similarity to Hegel is no accident. Bohm has studied Hegel in detail, and he has taken the *Logic* especially as the point of departure for some of his scientific views: ". . . may we not try to understand the world as a total process, in which all parts (for example, the system under observation, observing apparatus, man, etc.) are aspects or sides whose relationships are determined by the way in which they are generated in the process? Of course, in physics, man can, in an adequate approximation, probably be left out of the totality, because he obtains his information from a piece of apparatus on the large-scale level, which is influenced in a negligible way by his looking at it. But at a quantum mechanical level of accuracy, the apparatus and the system under observation must be recognized to be linked indivisibly. Should not the theory be formulated so as to say that this is so . . . ? In a total process of the kind that I am talking about, an observation is regarded as a particular kind of movement, in which some aspects of the process are, as it were, 'projected' into certain large-scale results . . . This process of projection is . . . an integral part of the total process that is being projected" (p. 482).

75. *Logik*, II, 53.

76. *Logik*, I, 67. Cf. also the physical model for this identity in I, 78–79, according to which neither "pure light" nor "pure darkness" gives rise to (the perception of) objects which are recognized and "distinguished only in the determined light . . . which is turbid light."

77. Bohm will therefore not be able to keep contradiction out of his ideas as he occasionally seems to believe (e.g., in *Scientific Change*, p. 482, second paragraph). He agrees in other places but tries to circumvent particular contradictions by moving to a different level of reality. Cf. his *Causality and Chance in Modern Physics* (New York: Harper Torchbooks, 1961).

78. Lenin, *Nachlass*, p. 27.

79. *Logik*, I, 115.

80. *Jenenser Logik, Metaphysik und Naturphilosophie*, ed. G. Lasson (Hamburg: Felix Meiner, 1967), p. 31.

81. In German the statement is more impressive: "Die Wahrheit [des] Seins der endlichen Dinge ist ihr Ende."

82. *Logik*, I, 117.

83. *Ibid.*

84. *Ibid.*, p. 36.

85. *Ibid.*

86. Cf. below, section 13, as well as footnote 116 of "Problems of Empiricism, Part II."

87. *Logik*, I, 36; cf. also II, 54, 58ff.

88. *Logik*, I, 117.

89. F. Engels, *Anti-Duehring* (Chicago: Charles H. Kerr, 1935), pp. 144–145; my italics. I am quoting Engels, Lenin, Mao, and similar thinkers rather than the usual bunch of Hegelian or anti-Hegelian scholars as they have still kept the freshness of mind that is necessary to interpret and to concretely apply the Hegelian philosophy. The same is true of such physicists as Bohm, Vigier, and even Bohr who may occasionally be regarded as an unconscious Hegelian. Cf. the remarks on subject and object below. Cf. also note 38.

90. *Logik*, I, 107.

91. Mathematics was for a long time regarded as lying outside the domain of dialectics. The examples used by Hegel and Engels and especially the example of the differential calculus, so it was thought, only showed the immaturity of the mathematics of the time and the limitations of even the greatest philosophers. One should not have been quite so generous, however. What Hegel says of mathematics applies to informal mathematics and, insofar as informal mathematics is the source of the rest, to all of mathematics. That a dialectical study of mathematics can lead to splendid discoveries, even today, is shown by Lakatos's *Proofs and Refutations* (first published in the *British Journal for the Philosophy of Science*, 1963–64). One must praise Lakatos for having made such excellent use of his Hegelian upbringing. On the other hand one must perhaps also criticize him for not revealing his source of inspiration in a more straightforward manner but giving the impression that he is indebted to a much less comprehensive and much more mechanical school of thought. Or has his temporary membership in this school made him lose his sense of perspective? So that he prefers being mistaken for a Wittgensteinian to being classified with the dialectical tradition to which he belongs? Cf. also note 27(B).

92. *Anti-Duehring*, pp. 143–144.

93. *Ibid.*, pp. 138–139.

94. *Ibid.*, p. 144; my italics. Epistemologically these laws belong to the Aristotelian rather than to the Newtonian tradition.

95. *Encyclopaedie der Philosophischen Wissenschaften, ergaenzet durch Vortraege und Kollegienhette*, ed. L. Henning et al. (Berlin, 1840), pp. 395–396; cf. also Lenin, *Nachlass*, p. 102. Or, to use Bohm's terminology: "as long as, by our customary habits of thinking, we try to say that in an experiment, some part of the world is observed [and described], with the aid of some other part, we introduce an element of confusion into our thought process. Indeed, even the very word 'observation' is misleading, as it generally implies a separation between the observing apparatus and the object under observation, of a kind that does not actually exist." *Scientific Change*, pp. 482–483.

The reader should go on and consider the beautiful example of the observation of a mirror image.

96. *Logik*, II, 224.
97. *Ibid.*, p. 227.
98. *Ibid.*, p. 408.
99. *Ibid.*, p. 225.
100. *Ibid.*, p. 408.
101. Lenin, *Nachlass*, p. 114.
102. *Logik*, II, 410.
103. *Ibid.*, pp. 408–409.
104. *Ibid.*, p. 228. "Knowledge is the eternal infinite approach of thought and object. The mirroring of nature in human thought is not 'dead,' it is not 'abstract,' it is not without motion, not without its contradictions but is to be conceived as an eternally moving process that gives rise to contradictions and removes them." Lenin, *Nachlass*, p. 115.
105. *Logik*, II, 228. The whole introduction to the *Subjective Logik*, i.e., II, 213–234, can be used for a criticism of what has become known as Tarski's theory of truth. If I remember correctly, this criticism is similar to a criticism voiced by the late Professor Austin in his lectures in Berkeley in 1959. Which shows that even an Oxford philosopher occasionally stumbles upon The Truth.
106. The *Assayer*, quoted from S. Drake and C. D. O'Malley, eds., *The Controversy on the Comets of 1618* (London: Oxford University Press, 1960), pp. 184–185.
107. *Dialogue concerning the Two Chief World Systems*, trans. S. Drake (Berkeley: University of California Press, 1953), p. 328.
108. D. Brouwer and G. M. Clemence, *Methods of Celestial Mechanics* (New York: Academic, 1961), p. v. Cf. also R. H. Dicke, "Remarks on the Observational Basis of General Relativity," in Hong-Yee Chiu and W. F. Hoffmann, eds., *Gravitation and Relativity* (New York: Benjamin, 1964), pp. 1–16. For a more detailed discussion of some of the difficulties of classical celestial mechanics see chapters IV and V of J. Chazy, *La Théorie de la Relativité et la Mécanique Céleste*, vol. I (Paris: Gauthier-Villars, 1928).
109. Cf. section 22 of Jammer, *The Conceptual Development of Quantum Mechanics*. For an analysis see the paper by Lakatos referred to in note 188 of the present essay.
110. H. A. Lorentz studied Miller's work for many years and could not find the trouble. It was only in 1955, 25 years after Miller had finished his experiments, that a satisfactory account of his results was found. See R. S. Shankland, "Conversations with Einstein," *American Journal of Physics*, 31 (1963), 47–57, especially p. 51, as well as footnotes 19 and 34. See also the inconclusive discussion at the "Conference on the Michelson-Morley Experiment," *Astrophysical Journal*, 68 (1928), 341ff. For general relativity see Chazy, *La Théorie de la Relativité*, I, 228ff.
111. For arguments see my essay "In Defence of Classical Physics" in the first issue of the *Studies in the History and Philosophy of Science*, Spring 1970, pp. 59–85.
112. This has been pointed out by K. R. Popper, for example in his paper "Rationality and the Search for Invariants" (Opening Address to the International Colloquium for the Philosophy of Science, London, 1965).
113. W. Heisenberg, "Der gegenwärtige Stand der Theorie der Elementarteilchen," *Naturwissenschaften*, 42 (1955), 640ff. For a comprehensive account of Heisenberg's philosophy, see Herbert Hörz, *Werner Heisenberg und die Philosophie* (Berlin: Deutscher Verlag der Wissenschaften, 1966).
114. *Physics*, book VI; *De coelo*, 303a3ff; *De generatione et corruptione*, 316a. Aristotle's theory of the continuum seems to be closely connected with his empiricism. In Aristotle the empirical doctrine is not just a philosophical dogma, it is a cosmological hypothesis that is clearly formulated (one hears, for a change, what kind of process experience is supposed to be) and leads to a solution of problems which arose in other,

and more 'metaphysical,' traditions. The problem of the continuum seems to be one of these problems.

115. Cf. A. Grünbaum, "A Consistent Conception of the Extended Linear Continuum as an Aggregate of Unextended Elements," *Philosophy of Science*, 19 (1952), 288ff.
116. Sir Isaac Newton, *Opticks* (New York: Dover, 1952), p. 266.
117. The rule is enunciated in Kepler's *Ad Vitellionem Paralipomena, Johannes Kepler, Gesammelte Werke herausgegeben im Auftrage der Deutschen Forschungsgemeinschaft und der Bayerischen Akademie der Wissenschaften*, vol. II (Munich: C. H. Beck'sche Verlagsbuchhandlung, 1939), p. 72. For a detailed discussion of Kepler's rule and its influence see Vasco Ronchi, *Optics: The Science of Vision* (New York: New York University Press, 1957), sections 43ff.
118. *Lectiones XVIII Cantabrigiae in Scholis publicis habitae in quibus Opticorum Phenomenon genuinae Rationes investigantur ac exponentur* (London, 1669), pp. 125–126. The passage is used by Berkeley in his attack on the traditional, 'objectivistic' optics. An *Essay towards a New Theory of Vision*, vol. I, Works, ed. A. C. Fraser (London, 1901), pp. 137ff.
119. Assuming M to be the observed mass of the charged particle, we obtain for its acceleration at time t the value

$$b(t) = b(0) \cdot \exp[3/2 \cdot Mc^2/e^2] \cdot t.$$
 Cf. D. K. Sen, *Fields and/or Particles* (New York: McGraw-Hill, 1968), p. 10.
120. G. Källén, *Helvetica Physica Acta*, 25 (1952), 417, as well as Sen, *Fields and/or Particles*, pp. ix and 73. ". . . this treatment illustrates how we can extract sensible numbers that can be compared with observation despite the divergence difficulties inherent in the present form of field theory." J. J. Sakurai, *Advanced Quantum Mechanics* (Reading, N.Y.: Addison-Wesley, 1967), p. 72.
121. The difficulty was realized by Bohr in his thesis. Bohr also pointed out that the velocity changes due to the change of the external field would equalize after the field is established so that no magnetic effect could arise. Cf. J. L. Heilbron and T. S. Kuhn, "The Genesis of the Bohr Atom," *Historical Studies in the Physical Sciences*, 1 (1969), 221.
- The argument in the text is taken from vol. II of *The Feynman Lectures* (Reading, N.Y.: Addison-Wesley, 1965), chapter 34.6. For a somewhat clearer account see R. Becker, *Theorie der Elektrizität*, vol. II (Leipzig: Teubner, 1949), p. 132.
122. Cf. my translation of Ehrenhaft's lectures on singular magnetic poles which can be obtained from me at the drop of a postcard.
123. Example: the theory of Eudoxus was misunderstood for a considerable time until Schiaparelli made it comprehensible through calculations of his own. For details see N. Herz, *Geschichte der Bahnbestimmung von Planeten und Kometen*, vol. I, *Die Theorien des Altertums* (Leipzig: Teubner, 1887), pp. 18ff. This is one of the reasons why even obscure or refuted theories should not be abandoned but be made available to all, so that some sympathetic and intelligent guy may pick them up and demonstrate their hidden virtue.
124. "The ephemerides are calculated in accordance with the Newtonian law of gravitation, modified by the theory of general relativity." *Explanatory Supplement to the Astronomical Ephemeris and the American Ephemeris and Nautical Almanack* (London: Her Majesty's Stationery Office, 1961), p. 11. "In the theory of relativity the law of attraction can be formulated rigorously only for the movement of an infinitely small mass under the influence of a fixed spherical mass; this movement is determined by the geodesics of the ds^2 of Schwarzschild . . . if we now want to pass in the study of planetary movements from the Newtonian theory to the theory of relativity, then it suffices . . . to add to the Newtonian perturbations the advancements of the perihelia . . . obtained from the ds^2 of Schwarzschild." J. Chazy, *La Théorie de la Relativité et la Mécanique Céleste*, I, 228. "This mixture of the theories of

Newton and Einstein is intellectually repellent, since the two theories are based upon such different fundamental concepts. The situation will be made clear only when the many-body problem has been handled relativistically in a rational and mathematically satisfactory way." J. L. Synge, *Relativity, the General Theory* (Amsterdam: North-Holland, 1964), pp. 296-297.

125. One might be inclined to deny this statement by referring to the numerous "derivations" of classical mechanics from the general theory of relativity, some of them dealing quite explicitly with the n-body problem. Now, such derivations are but formal exercises unless it is shown that not only momentary effects but also long-term effects are excluded, and this for the whole period for which useful astronomical observations are available (more than 3000 years!): one would have to show that the minute deviations neglected in the usual approximations have no cumulative effect which might endanger the stability of the planetary system. This is precisely what is missing in the derivation of the ds^2 of Schwarzschild that is given in J. Chazy, *La Théorie de la Relativité et la Mécanique Céleste*, vol. II (Paris: Gauthier-Villard, 1930), chapters IX to XI. Planets are here quite properly embedded in the solar system, and the basic equations of relativity are used to show that the combination, referred to in note 124, of Newtonian perturbation theory and the ds^2 of Schwarzschild is valid to the degree of approximation used. However, Chazy's statement (p. 182) that "this method has thereby been shown to be legitimate" cannot be accepted, for cumulative effects have been omitted from the calculation. Considering the difficulties of the relativistic many-body problem it is not likely that they will soon be taken into account. And even if they are some day, we must still concede the existence of periods in the history of science which, from a sternly methodological point of view, are close to madness, but whose elimination is bound to wipe out science.

126. "The complete, or almost complete mistakes and failures are usually forgotten, by the prophets as well as by the faithful," says a "modern man" and decided opponent about astrology (Franz Boll and Carl Bezold, *Sternglaube und Sterndeutung*, Leipzig: Teubner, 1931, pp. 74, 72). It is clear that the judgment applies to the so-called "sciences" as well.

127. For details see again "Problems of Empiricism, Part II." The fact that science, or any historically grown subject, contains components of different age and different sophistication which hinder each other has been seen and described in a political context by Lenin, Trotsky, and others: "The gist of the matter lies in this," writes Trotsky ("The School of Revolutionary Strategy," Speech at a General Party Membership Meeting of the Moscow Organization, July 1921, quoted from *The First Four Years of the Communist International*, vol. II, New York: Pioneer Publishers, 1923, p. 5): "that the different aspects of the historical process—economics, politics, the growth of the working class—do not develop simultaneously along parallel lines." Cf. also Lenin, 'Left Wing' Communism, p. 59, as well as the quotation in note 38 of the present essay.

The same is true of the relation between observation, auxiliary sciences, theories, and so on.

An excellent example of the phase difference between different parts of the historical process is provided by the history of witchcraft. Witchcraft persecutions were at their peak at the beginning of the seventeenth century and later on, when Galileo reported his telescopic discoveries, Kepler found the laws of planetary motion (and had to defend his own mother against the accusation of witchcraft), when Descartes developed his rationalism and his materialistic physics, 80 years after Copernicus, 40 to 50 years after Montaigne, and they continued into the age of Newton. And the belief was very often held by people who were otherwise perfect examples of the new "scientific spirit." In these times "in which science and art were reborn . . . when people were painting and sculpting anew and once more turned towards investigation and writing, the making of new discoveries and new inventions, when the old classical world and bookprinting seemed to recast the face of Western civilization—in those very days humanity

stood in one respect on a lower level of mental development than do some of the primitive races of today." C. Binz, *Doctor Johann Weyer* (Bonn: Landesverlag, 1895), p. 3.

128. In what follows the reader is advised to always consult his Hegel and to compare my statements with Hegel's own dialectical formulations. The reader will also realize that my analysis at once invalidates the direct and naively empirical "refutations" of Marxism by Bernstein, Popper, and others. Things are not quite that simple! Cf. also the next section.

129. *Dialogue concerning the Two Chief World Systems*, p. 126.

130. *Ibid.*, p. 125.

131. *Ibid.*, p. 256.

132. "Problems of Empiricism," pp. 204ff.

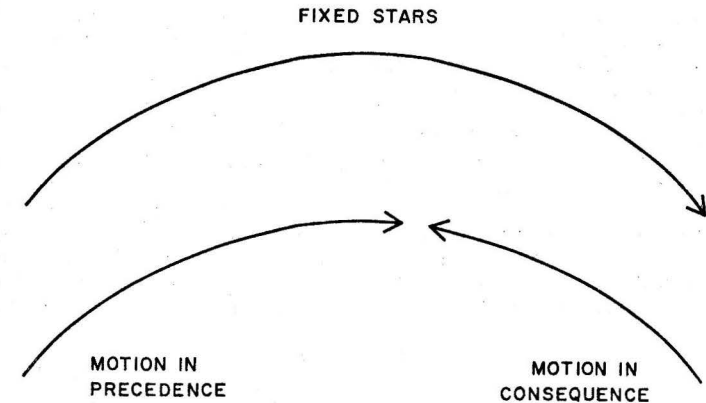
133. Bacon, *The New Organon*, Introduction.

134. *Dialogue concerning the Two Chief World Systems*, p. 255. My italics.

135. *Ibid.*, p. 256.

136. *Ibid.*, p. 248.

137. *Ibid.*, p. 171. Only one example to support this thesis: In the Middle Ages there existed two theories of planetary motion, one asserting a motion in consequence, with Saturn the slowest planet and the moon the fastest, the other asserting a motion



THEORY ii

in precedence, i.e., from east to west, with Saturn the fastest (but not as fast as the celestial sphere) and the moon the slowest. The first theory is held by Plato (*Laws*, 822a), by all the followers of Ptolemy; it occurs in the *Sphere of Sacrobosco* (Lynn Thorndike, *The "Sphere" of Sacrobosco and Its Commentators*, Chicago: University of Chicago Press, 1949, p. 120, Latin text p. 79), in the German *Sphere* of Conrad von Megenberg, and in many encyclopedias and textbooks (Vitruvius, Isidore, Bede, Hrabanus Maurus, and others). The second theory occurs in Democritus, in Wolfram von Eschenbach's *Parzival* (cf. G. K. Bauer, *Sternkunde und Sterndeutung der Deutschen im 9.-14. Jahrhundert*, Berlin, 1937, pp. 27-28). Both theories are compared in the book *De solis affectibus* (Jacques Paul Migne, *Patrologia Latina*, vol. 172, p. 108): "Utrique sententiae, sive contra firmamentum vadunt planetae, seu cum firmamentum potest opponi." Yet we have here a perfect example of relative motion. An even better

THEORY i

example of the operative interpretation of motion is provided by the habit of interpreting Bible passages concerning motion as dealing with absolute motion. Altogether the interpreters of the Bible disregard appearances and regard terms such as "move," "to be at rest," as absolute terms referring to objective situations having unique consequences. This in turn is the result of a naive realism of fantastic proportions. Thus St. Augustine (*De Genesi ad litteram*, II, chapter XVI; Migne, *Patrologia Latina*, vol. 134, p. 277) rejects the idea of fixed stars bigger than the sun on the basis of the duo luminaria magna of Genesis 1:16. The persistence of the belief in witchcraft is at least partly due to this instinctive naive realism that was reluctant to declare as illusory what one had experienced so plainly. Cf. Gregory Zilboorg, *The Medical Man and the Witch during the Renaissance* (Baltimore: Johns Hopkins Press, 1935). Cf. also note 40.

Finally, it must not be overlooked that the impetus theory which Galileo accepts in his early writings on mechanics (*De motu*; *De motu dialogus*) and which had been the *opinio communis* since the fifteenth century demands an absolute view of motion. For if the motive force resides in a moving object in the same way in which heat resides in a piece of iron, or sound in a bell that has just been struck (for these examples see *De motu* as translated by I. E. Drabkin in Galileo Galilei, *On Motion and On Mechanics*, ed. S. Drake and I. E. Drabkin, Madison: University of Wisconsin Press, 1960, p. 77, and memorabilia on motion as translated by I. E. Drabkin in Drake and Drabkin, eds., *Mechanics in 16th Century Italy*, Madison: University of Wisconsin Press, 1969, p. 379), then the necessary effect of such a force, i.e., motion, cannot depend on the relation of the object to an arbitrarily chosen coordinate system: the impetus theory entails the absolute, or operative, view of all motion.

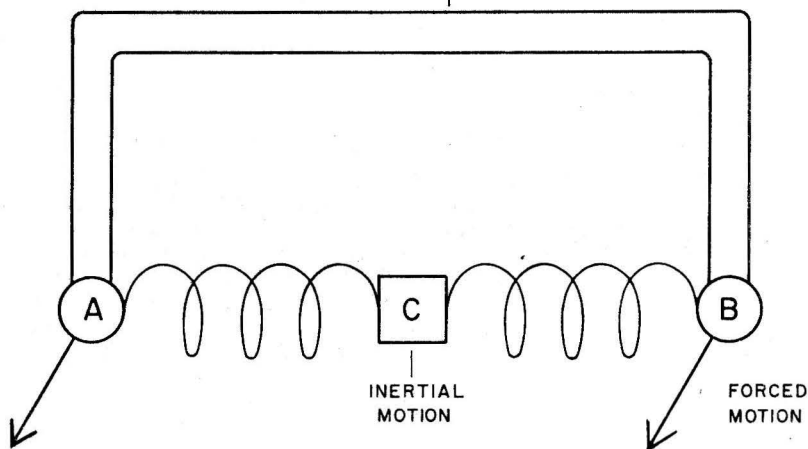
138. Cf. "Problems of Empiricism," pp. 204ff.

139. Cf. Hegel, *Vorlesungen über die Geschichte der Philosophie*, part I, ed. C. L. Michelet (Berlin: Duncker und Humblot, 1840), p. 289.

140. *Dialogue concerning the Two Chief World Systems*, p. 171.

Galileo's relativism with respect to motion is far from being satisfactory, or even consistent. He proposes the view, (i), expressed in the quotation in the text, that shared motion has no effect whatever. "Motion," he says (*Dialogue*, p. 116), "insofar as it is and acts as motion, to that extent exists relatively to things that lack it; and among things which all share equally in any motion, it does not act and is as if it did not exist." "Whatever motion comes to be attributed to the earth must necessarily remain im-

RIGID CONNECTION



perceptible . . . so long as we look only at terrestrial objects" (p. 114). ". . . motion that is common to many moving things is idle and inconsequential to the relation of these movables among themselves . . ." (p. 116).

On the other hand, (ii), there is the assertion (cf. *Dialogue*, p. 19) that "nothing . . . moves in a straight line by nature. The motion of all celestial objects is in a circle; ships, coaches, horses, birds, all move in a circle around the earth; the motions of the parts of animals are all circular; in sum—we are forced to assume that only *gravia deorsum* and *levia sursum* move apparently in a straight line; but even that is not certain as long as it has not been proven that the earth is at rest."

Now, if (ii) is adopted, then (i) cannot be correct. For assume that two objects, A and B, being rigidly connected, move in a straight line and that a third object, C, is fastened to them by a spring. Clearly C, being moved forcibly, will tend to assume its natural circular motion and will therefore change its relation to A and B, thus contradicting the assertion, inherent in (i), that common motion does not affect the relation between things. It is this inconsistency which has forced me to split the argument in the text into two steps, one dealing with the relativity of motion (only relative motion it noticed), the other dealing with inertial laws (and only inertial motion leaves the relation between the parts of a system unaffected—assuming, of course, that neighboring inertial motions are approximately parallel). For the two steps of the argument see the beginning of section 8.

It is also important to realize that accepting the relativity of motion even for inertial paths means giving up the impetus theory (cf. the last part of note 137). This Galileo seems to have done by now, for his argument for the existence of "boundless" or "perpetual" motions which he outlines on pp. 147ff of the *Dialogue* appeals to motions which are neutral, i.e., neither natural nor forced and which may therefore(?) be assumed to go on forever.

141. J. L. Austin, *Sense and Sensibilia* (New York: Oxford University Press, 1964), p. 74.

142. For details see the Appendix at the end of this paper.

143. *Dialogue concerning the Two Chief World Systems*, pp. 171–172.

144. *Ibid.*, pp. 249–250.

145. *Ibid.*, pp. 172–173.

146. *Ibid.*, p. 250.

147. Ptolemy, *Syntaxis*, i.7.

148. *Dialogue concerning the Two Chief World Systems*, p. 416. Cf. the *Dialogues concerning Two New Sciences*, trans. Henry Crew and Alfonso de Salvio (London, 1914; New York: Dover, 1958), p. 164: "The same experiment which at first glance seemed to show one thing, when more carefully examined, assures us of the contrary."

149. *Dialogue concerning the Two Chief World Systems*, p. 131.

150. *Ibid.*, p. 327.

151. *Ibid.*, p. 330.

The idea that there is an absolute direction in the universe has a very interesting history. It rests on the structure of the gravitational field on the surface of the earth or of that part of the earth which the observer knows, and generalizes the experiences made there. The generalization is only rarely regarded as a separate hypothesis; it rather enters the "grammar" of common sense and gives the terms "up" and "down" an absolute meaning. (This is a natural interpretation, in precisely the sense that was explained in the text above.) Lactantius, a church father of the fourth century, appeals to this meaning when he asks (*Divinae Institutiones*, III, de falsa sapientia): "Is one really going to be so confused as to assume the existence of humans whose feet are above their heads? Or of regions where the objects which fall with us rise instead? Where trees and fruit grow not upward, but downwards?" The same use of language is presupposed by that "mass of untutored men" who raise the question why the antipodes are not falling off the earth (Pliny, *Natural History*, II, 161–166; cf. also Ptolemy,

Syntaxis, i.7). The attempts of the Presocratics, Thales, Anaximenes, Xenophanes, to find support for the earth which prevents it from falling "down" (Aristotle, *De coelo*, 294a2ff) show that almost all early philosophers with only the exception of Anaximander have shared in this way of thinking. (For the Atomists who assume that the atoms originally fall "down," see M. Jammer, *Concepts of Space*, Cambridge, Mass.: Harvard University Press, 1953, p. 11). Even Galileo, who thoroughly ridicules the idea of the falling antipodes (*Dialogue concerning the Two Chief World Systems*, p. 331) occasionally speaks of the "upper half of the moon" (p. 65), meaning that part of the moon "which is invisible to us." And let us not forget that some linguistic philosophers of today "who are too stupid to recognize their own limitations" (p. 327) want to revive the absolute meaning of "up-down" at least locally. Thus the power over the minds of his contemporaries of a primitive conceptual frame assuming an anisotropic world, which Galileo had also to fight, must not be underestimated. For an examination of some aspects of common sense at the time of Galileo, including astronomical common sense, the reader is invited to consult E. M. W. Tillyard, *The Elizabethan World Picture* (London: Penguin, 1963). The agreement between popular opinion and the central-symmetrical universe is frequently asserted by Aristotle. See, for example, *De coelo*, 308a23f.

152. *Dialogue concerning the Two Chief World Systems*, p. 327.

153. *Ibid.*, p. 327; italics added.

154. *Ibid.*, pp. 132, 416.

155. Cf. footnote 137 of "Problems of Empiricism, Part II."

156. *Dialogue concerning the Two Chief World Systems*, p. 341. Galileo here quotes part of Copernicus's address to Pope Paul III in *De revolutionibus*. Cf. also the *Narratio Prima* (quoted from E. Rosen, *Three Copernican Treatises*, New York: Dover, 1959, p. 165): "For all these phenomena appear to be linked most nobly together, as by a golden chain; and each of the planets, by its position, and order, and every inequality of its motion, bears witness that the earth moves and that we who dwell upon the globe of the earth, instead of accepting its changes of position, believe that the planets wander in all sorts of motions of their own." Note that empirical reasons are absent from the argument, and they have to be, for Copernicus himself admits (*Commentariolus*, Rosen, *Three Copernican Treatises*, p. 57) that the Ptolemaic theory is "consistent with the numerical data."

157. *Dialogue concerning the Two Chief World Systems*, p. 120.

In their book *Geschichte der Hexenprozesse*, vol. I (Stuttgart: Cotta, 1880), p. 64, W. G. Soldan and H. Heppel comment on the fluidity of concepts such as striga, empusa, Lamia, and they continue: "it must not be forgotten that no physiology has been written for the domain of superstition and that there remained, despite the existence of certain essential elements, sufficient leeway for variety in the particulars, according to age, locality, or the fantasy of the individual poet." Cf. also the material assembled by J. Frank, "Geschichte des Wortes Hexe," in J. Hansen, *Quellen und Untersuchungen zur Geschichte des Hexenwahns und der Hexenverfolgungen im Mittelalter* (Bonn: Olbers, 1901), chapter VII. My analysis of Galileo shows that such fluidity is a characteristic of science also and that it takes possession not only of the accidental elements of a concept, but of its very essence. Moreover, it is a precondition of scientific progress. The stability of concepts is not the differentia specifica that separates science from witchcraft (magic, poetry, and so on).

158. Cf. "Classical Empiricism."

159. Cf. note 140.

160. *Dialogue concerning the Two Chief World Systems*, p. 145.

161. *Ibid.*, p. 147.

162. Cf. note 140.

163. Charles B. Schmitt, in an interesting and very important article ("Experience and Experiment: A Comparison of Zabarella's View with Galileo's *De motu*," *Studies*

in the Renaissance, 16 (1969), 80–138) discusses the various notions of experience which were active in the sixteenth and seventeenth centuries and tries to determine Galileo's own position during his years in Pisa. Galileo then regarded experience as a "useful device to resolve a particular dispute. By merely observing the world around us we can sometimes decide either for or against a particular opinion which has been brought forth. Therefore, Aristotle can sometimes be criticized for holding positions which are not in conformity with experience. On the other hand, Aristotle sometimes relies too much upon experience, to the extent that he does not allow a sufficient role to *rationes*; but according to Galileo it is through *rationes* that demonstration takes place. That is to say: demonstration and proof depend upon 'objects of thought' rather than 'objects of experience' (pp. 111–112). Accordingly, "with the young Galileo . . . experience is not always so carefully selected and, more often than not, proves to be deceptive or, at least, not capable of resolving the problem at hand" (p. 124). In addition Galileo seems to distrust experience because of its occult overtones (p. 135): there was a tradition, in the sixteenth and seventeenth centuries, when experience went hand in hand with the study of magic and of the occult, being a source of knowledge in cases which could not be reached by reason: "There are hidden forces," writes Cornelius Agrippa in his *Occult Philosophy* (I, 10), "whose causes are inaccessible because reason cannot thoroughly explore them. Therefore philosophers have studied the greater number of these causes by experience rather than by thought" (cf. Schmitt, pp. 86ff, and the literature there). Now our hypothesis is (a) that later on Galileo neither rejected experience nor relied on it to the exclusion of everything else, but that he changed it so that 'objects of thought' became perceptible, and (b) that his belief in the Copernican theory played an essential part in the transformation. This hypothesis will of course have to be supported by a more detailed study than I have given here. Schmitt's article shows that the situation is less settled than is usually assumed and that many popular views concerning Galileo (including those put forth more recently by Geymonat—see Schmitt, footnote 133) are oversimplified, to say the least. "What are some of the broader implications of our investigation?" asks Schmitt at the end of his paper (p. 136). "Although it seems obvious that the 'experimental method' which emerged in the 17th century was in some way or another an outgrowth of the observational and experientialist tradition of the preceding centuries, it is not at all clear precisely how. To see it as merely an outgrowth of technological practice on the one hand, or of Aristotelian empiricism on the other, seems to oversimplify the situation to the point of distortion. At the same time, one must be careful not to dismiss entirely the significance of observation and experience and to make the 'scientific revolution' merely a conceptual revolution in which a Platonic view of the universe replaced an Aristotelian one. It seems clearly to have been a more complex process than either of these interpretations would seem to suggest. It is the belief of the present writer," Schmitt continues, "that more light could be shed on this subject through a detailed study of sixteenth century writings on natural philosophy, both scholastic, and non-scholastic." I would add that one must also study the manner in which the wish to make Copernicus true has influenced the procedures of some thinkers, and especially of Galileo.

164. Cf. *Physics*, 208b10ff.

165. Galileo seems to have been aware of this situation. He silently abandons the idea of the nonoperative character of circular motion in his attempt to prove the motion of the earth from the tides. Cf. H. L. Burstin, "Galileo's Attempt to Prove That the Earth Moves," *Isis*, 53 (1962), 161–185, and the literature cited there.

166. Such a stone, says Galileo (*Dialogue concerning the Two Chief World Systems*, p. 233), would arrive ahead of the tower.

167. One might be inclined to assume that the phenomenon of projectile motion which in the fourteenth century had led to various criticisms of the Aristotelian point of view was a clear empirical basis for a law such as Galileo wanted to establish. A

little consideration shows that this cannot have been the case. All one knew about projectile motion was that it proceeded for some time and then came to a halt. Galileo's circular law is in no way determined by this phenomenon. It is determined by his wish to preserve the Copernican view. Cf. also notes 137, 140, and the text below. Buridan, incidentally, rejected the rotation of the earth.

168. Cf. note 140.

169. For an enumeration of such experiments see A. Armitage, "The Deviation of Falling Bodies," *Annals of Science*, 5 (1941-47), 342-351. For further material and discussion see A. Koyré, *Metaphysics and Measurement* (Cambridge, Mass.: Harvard University Press, 1968). For a comprehensive survey see G. Hagen, *La Rotation de la Terre* (Rome, 1911). It is interesting to see how the experiments ceased after their first inconclusive results, and how they were resumed when Newton made a new prediction concerning their outcome. Cf. Armitage, "The Deviation of Falling Bodies," p. 346.

170. *De motu*, trans. Drabkin, p. 73.

171. *Ibid.*, p. 78.

172. Drabkin translation, p. 338. Drake in footnote 10 of the same page comments that "Galileo was not a Copernican when he wrote this."

173. Quoted from Drake and Drabkin, eds., *Mechanics in 16th Century Italy*, p. 228.

174. *De motu*, pp. 73-74.

175. *Ibid.*, p. 74.

176. *Physics*, VII, 1; 241b34-36.

177. *De motu*, p. 79. Cf. also notes 137 and 140.

178. *De motu*, chapter XVIII in Drabkin's subdivision.

179. Cf. notes 137 and 140.

180. *De revolutionibus*, i.8.

181. *Dialogue concerning Two New Sciences*, pp. 215, 250.

182. Pp. 147ff. Cf., however, the inconsistency described in note 137 of the present essay.

183. According to Anneliese Maier, *Die Vorläufer Galileis im 14. Jahrhundert* (Rome: Edizioni di Storia e Letteratura, 1949), pp. 151ff, Galileo replaces impetus by inertia in order to explain the "fact" that "neutral" motions go on forever. Now, to start with, there is no such fact. Secondly, Galileo initially does not believe, and rightly so, that there is such a fact. This we have just seen. There is therefore no need for him "to explain certain newly detected phenomena" (p. 151). The need was purely theoretical: to accommodate, to "save," not a phenomenon, but a new world view.

184. The so-called scientific revolution led to astounding discoveries and it considerably extended our knowledge of physics, physiology, and astronomy. This was achieved by pushing aside and regarding as irrelevant, and often as nonexistent, those facts which had supported the older philosophy. Thus all the evidence for witchcraft, demonic possession, all the empirical phenomena one had been able to cite in favor of the existence of the devil, were pushed aside together with the "superstitions" they once confirmed. The result was that "towards the close of the Middle Ages science was forced away from human psychology, so that even the great endeavour of Erasmus and his friend Vives, as the best representatives of humanism, did not suffice to bring about a rapprochement, and psychopathology had to trail centuries behind the developmental trend of general medicine and surgery ["The hatred and jealousy of the doctors," says von Gleichen, "when they persecute, [is] as dangerous as that of the priests"]. As a matter of fact . . . the divorcement of medical science from psychopathology was so definite that the latter was always totally relegated to the domain of theology and ecclesiastic and civil law—two fields which naturally became further and further removed from medicine . . ." Zilboorg, *The Medical Man and the Witch*, pp. 3-4 as well as 70ff. ("Dr. Zilboorg," says H. Sigerist in his introduction to the book, "recognised that witchcraft is the central problem in the development of occidental psychiatry. In the changing attitude towards witchcraft modern psychiatry was born as a medical disci-

pline.") Astronomy advanced, but our knowledge of man slipped back into an earlier, more primitive stage. Cf. note 127.

Another example is *astrology*. "In the early stages of the human mind," writes A. Comte (*Philosophie Positive*, Paris: Littré, 1836, III, 273-280), "these connecting links between astronomy and biology were studied from a very different point of view, but at least they were studied and not left out of sight, as is the common tendency in our own time, under the restricting influence of a nascent and incomplete positivism. Beneath the chimaerical belief of the old philosophy in the physiological influence of the stars, there lay a strong though confused recognition of the truth that the facts of life were in some way dependent on the solar system. Like all primitive inspirations of man's intelligence this feeling needed rectification by positive science, but not destruction; though unhappily in science, as in politics, it is often hard to reorganise without some brief period of overthrow."

185. "Neurath fails to give . . . rules [which distinguish empirical statements from others] and thus unwittingly throws empiricism overboard." K. R. Popper, *The Logic of Scientific Discovery* (New York: Basic Books, 1959), p. 97.

186. *Papirer*, ed. P. A. Heiberg (Copenhagen, 1909), VII, part I, see A, Nr. 182. Cf. also sections 7ff of my forthcoming paper "Abriss einer anarchistischen Erkenntnislehre."

187. Cf. note 31 and text.

188. "Criticism and the Methodology of Scientific Research Programs," in *Criticism and the Growth of Knowledge*, ed. I. Lakatos and A. Musgrave (Amsterdam: North-Holland, 1969). Quotations are from the typescript of the paper which Lakatos distributed liberally before its publication. In this typescript the reference is mostly to Popper. Had Lakatos been as careful with acknowledgments as he is when the Spiritual Property of the Popperian Church is concerned, he would have pointed out that his liberalization which sees knowledge as a process is indebted to Hegel.

189. Popper, *The Open Society and Its Enemies*, pp. 388ff.

190. *Ibid.*, p. 390. Cf. also note 28.

191. *Ibid.* Cf. note 22 and the corresponding text.

192. *Ibid.*, p. 391.

193. *Ibid.*, p. 231.

194. I am referring here to the following two papers: "Epistemology without a Knowing Subject," in Bob Van Rootselaar and J. F. Staal, eds., *Logic, Methodology and a Knowledge of Science*, vol. III (Amsterdam: North-Holland, 1968), as well as "On the Theory of the Objective Mind." In the first paper, *birdnests* are assigned to the "third world" (p. 341) and an interaction is assumed between them and the remaining worlds. They are assigned to the third world because of their function. But then stones and rivers can be found in this third world too, for a bird may sit on a stone, or take a bath in a river. As a matter of fact, everything that is noticed by some organism will be found in the third world, which will therefore contain the whole material world and all the mistakes mankind has made. It will also contain "mob psychology."

195. Cf. again "Problems of Empiricism, Part II."

196. Cf. *Malleus Maleficarum*, trans. Montague Summers (London: Pushkin Press, 1928), part II, question I, chapter IV: "Here follows the way whereby witches copulate with those Devils known as Incubi," second item, as to the acts, "whether it is always accompanied with the injection of semen received from some other man." The theory goes back to St. Thomas Aquinas.

197. It is of course possible to establish correlations between the sentences of the two theories, but one must realize that the elements of the correlation, when interpreted, cannot be both meaningful, or both true: if relativity is true, then classical descriptions are either always false or are always nonsensical. Continued use of classical sentences must therefore be regarded as an abbreviation for sentences of the following kind: "Given conditions C, the classical sentence S was uttered by a classical physicist whose sense organs are in order, and who understands his physics"—and sentences of this kind,

if taken together with certain psychological assumptions, can be used for a test of relativity. However, the statements which are expressed by these sentences are part of the relativistic framework, for they use relativistic terms. This situation is overlooked by Lakatos who argues as if classical terms and relativistic terms can be combined at will and who infers from this assumption the nonexistence of incommensurability.

198. This became clear to me in a discussion with Mr. L. Briskman, in Professor Watkins's seminar at the London School of Economics.

199. This seems to occur in certain versions of the general theory of relativity. Cf. A. Einstein, L. Infeld, and B. Hoffmann, "The Gravitational Equations and the Problem of Motion," *Annals of Mathematics*, 39 (1938), 65, and Sen, *Fields and/or Particles*, pp. 19ff.

200. This consideration has been raised into a principle by Bohr and Rosenfeld, *Kgl. Danske Videnskab. Selskab, Mat.-Fys. Medd.*, 12, no. 8 (1933), and, more recently, by Robert F. Marzke and John A. Wheeler, "Gravitation as Geometry I," in Chiu and Hoffmann, eds., *Gravitation and Relativity*, p. 48: "every proper theory should provide in and by itself its own means for defining the quantities with which it deals. According to this principle, classical general relativity should admit to calibrations of space and time that are altogether free of any reference to [objects which are external] to it such as rigid rods, inertial clocks, or atomic clocks [which involve] the quantum of action."

201. It is possible to base space-time frames on this new element entirely, and to avoid contamination by earlier modes of thought. All one has to do is to replace distances by light times and to treat time intervals in the relativistic fashion, for example, by using the *k*-calculus (Cf. chapter II of J. L. Synge, "Introduction to General Relativity," in *Relativity, Groups, and Topology*, ed. C. M. DeWitt and B. B. DeWitt, New York: Gordon, 1964. For the *k*-calculus see H. Bondi, *Assumption and Myth in Physical Theory*, London: Cambridge University Press, 1967, pp. 28ff, as well as D. Bohm, *The Special Theory of Relativity*, New York: Benjamin, 1965, chapter XXVI.) The resulting concepts (of distance, velocity, time, etc.) are a necessary part of relativity, in the sense that all further ideas, such as the idea of length as defined by the transport of rigid rods, must be changed and adapted to them. They therefore suffice for explaining relativity. Following their own principle as described in note 200 Marzke and Wheeler have given an account of relativistic terms that does not involve any objects external to the theory (this account goes back to Robert F. Marzke, "The Theory of Measurement in General Relativity," A.B. senior thesis, Princeton University, 1959; the article by Marzke and Wheeler is the only published report available so far). All intervals, whether spatial or temporal, are expressed in terms of some (spatial or temporal) standard interval. There is no difference between the units used for intervals of distance and intervals of time. The construction which leads to measurement in terms of the standard interval is carried out with the help of light and mass points only and involves neither rigid rods nor clocks whose construction would have to be explained in nonrelativistic terms. "The importance of light rays and the light cone in the intrinsic geometry of physics comes more directly to the surface. The true function of the speed of light is no longer confused with the trivial task of relating two separate units of interval, the meter and the second, of purely historic and accidental origin" (Marzke and Wheeler, "Gravitation as Geometry I," p. 56). The difference between such terms and classical terms is now very obvious and the assertion of incommensurability is made much more precise. Cf. also note 205.

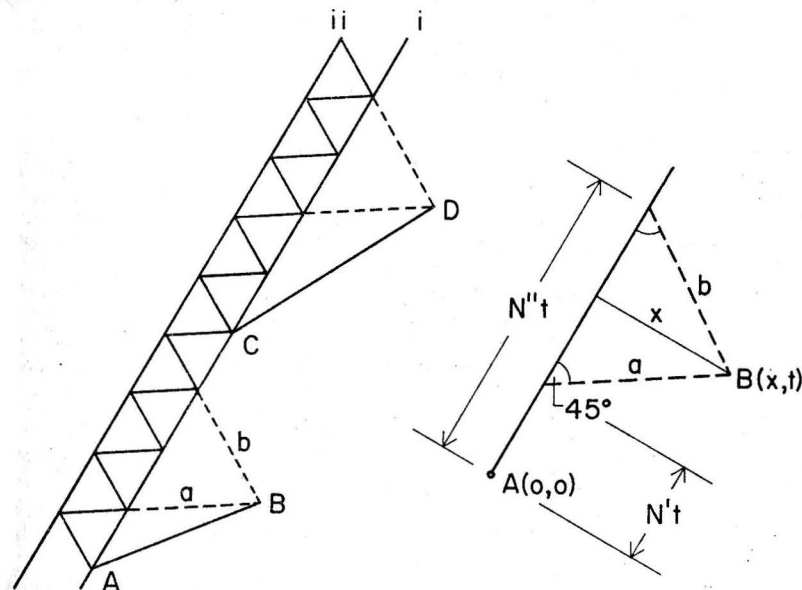
202. For this point and further arguments see A. S. Eddington, *The Mathematical Theory of Relativity* (Cambridge: Cambridge University Press, 1963), p. 33. The more general problem of concepts and numbers has been treated by Hegel, *Logik*, I, Das Mass.

203. This takes care of an objection which Professor J. W. N. Watkins has raised on various occasions.

204. For further details, especially concerning the concept of mass, the function of "bridge laws" or "correspondence rules," and the two-language model, see section iv of "Problems of Empiricism." It is clear that, given the situation described in

the text, we cannot derive classical mechanics from relativity, not even approximately. For example, we cannot derive the classical law of mass conservation from relativistic laws. The possibility of connecting the formulas of the two disciplines in a manner that might satisfy a pure mathematician, or an instrumentalist, is, however, not excluded. For an analogous situation in the case of quantum mechanics see section 3 of my paper "On a Recent Critique of Complementarity." See also section 2 of the same article for more general considerations.

205. Marzke and Wheeler measure length in the following way (for details see "Gravitation as Geometry I"): First, flatness of space is ascertained to the degree of precision desired. Next, a method is devised for constructing a parallel to any straight line in space-time (the method uses inertial trajectories and light rays only, thus eliminating all nonclassical space-time notions). Third, a "geodesic clock" is constructed by



letting a pulse of light be reflected back and forth between two parallels: the intersections of the pulse with one of the lines are the "ticks" of the clock. Finally, two arbitrary intervals, AB and CD, are compared, thus: A trajectory, *i*, is found that connects A and C. A parallel, *ii*, is drawn to *i*. A geodesic clock is constructed between *i* and *ii*. A light ray, *a*, is sent from *i* toward B and reflected from it back to *i* (*b*). $N't$ and $N''t$ are the times of departure and arrival of this light ray, counted from A. Assuming *c* equal to 1 we see that

$$N't = t - x$$

$$N''t = t + x, \text{ so that}$$

$$N'tN''t = t^2 - x^2 = (t - 0)^2 - (x - 0)^2 = (t_B - t_A)^2 - (x_B - x_A)^2 = (\Delta s_{AB})^2,$$

hence

$$(\Delta s_{AB}) = AB = \tau \sqrt{N'tN''t}$$

$$(\Delta s_{CD}) = CD = \tau \sqrt{N'''tN''''t}, \text{ so that}$$

$$\frac{CD}{AB} = \sqrt{\frac{N'''tN''''t}{N'tN''t}}$$

which gives the numerical values resulting from the comparison.

Now this method clearly works only if we can assume that it gives the same results in all inertial systems, for example, if we can assume that c has the same value in all inertial systems. If there exists a preferred system, or if the classical principle of velocity addition is still assumed to be valid, then the method no longer leads to useful numbers, and transitivity of length cannot be guaranteed for all inertial systems (just assume three systems, A, B, and C, B moving with $c/2$ relatively to A and C with $c/2$ relatively to B, then length measured in C will always be 0, assuming A is the rest system).

206. R. Carnap, "The Methodological Character of Theoretical Concepts," *Minnesota Studies in the Philosophy of Science*, vol. I, ed. H. Feigl and M. Scriven (Minneapolis: University of Minnesota Press, 1956), p. 47.

207. An even more conservative principle is sometimes used when discussing the possibility of languages with a logic different from our own: "Any allegedly new possibility must be capable of being fitted into, or understood in terms of, our present conceptual or linguistic apparatus." B. Stroud, "Conventionalism and the Indeterminacy of Translations," *Synthese*, 1968, p. 173.

208. As an example the reader is invited to consult J. Piaget, *The Construction of Reality in the Child* (New York: Basic Books, 1954).

209. *Ibid.*, pp. 5ff.

210. For the condition of research formulated in the last sentence see section 8 of "Reply to Criticism," *Boston Studies in the Philosophy of Science*, vol. II, ed. Cohen and Wartofsky. For the role of observation see section 7 of the same article. For the application of Piaget's work to physics and, more especially, to the theory of relativity see the appendix of Bohm, *The Special Theory of Relativity*. Bohm and Schumacher have also carried out an analysis of the various informal structures which underlie our theories. One of the main results of their work is that Bohr and Einstein argued from incommensurable points of view. Seen in this way the case of Einstein, Podolsky, and Rosen cannot refute the Copenhagen Interpretation and it cannot be refuted by it either. The situation is, rather, that we have two theories, one permitting us to formulate EPR, the other not providing the machinery necessary for such a formulation. We must find independent means for deciding which one to adopt. For further comments on this problem see section 9 of my "On a Recent Critique of Complementarity."

211. For what follows cf. also my review of Nagel's *Structure of Science* on pp. 237-249 of the *British Journal for the Philosophy of Science*, 6 (1966), 237-249.

212. Carnap, "The Methodological Character of Theoretical Concepts," p. 40. Cf. also C. G. Hempel, *Philosophy of Natural Science* (Englewood Cliffs, N.J.: Prentice-Hall, 1966), pp. 74ff.

213. It was for this reason that Leibniz regarded the German of his time and especially the German of the artisans as a perfect observation language, while Latin, for him, was already too much contaminated by theoretical notions. See his "Unvorgreifliche Gedanken, betreffend die Ausübung und Verbesserung der Teutschen Sprache," published in *Wissenschaftliche Beihefte zur Zeitschrift des allgemeinen deutschen Sprachvereins*, IV, 29 (Berlin: F. Berggold, 1907), pp. 292ff.

214. For examples of such descriptions see the article of Synge referred to in note 201.

215. This objection was raised at a conference by Prof. Roger Buck.

216. For this point see section I of "Reply to Criticism," as well as the corresponding sections in "Problems of Empiricism."

217. That the choice between comprehensive theories rests on one's interests entirely and reveals the innermost character of the one who chooses has been emphasized by Fichte in his "Erste Einleitung in die Wissenschaftslehre." Fichte discusses the opposition between idealism and materialism which he calls dogmatism. He points out that there are no facts and no considerations of logic which can force us to adopt either the one or the other position. ". . . we are here faced," he says (*Erste und Zweite Einleitung in die Wissenschaftslehre*, Hamburg: Felix Meiner, 1961, p. 19), "with an

absolutely first act that depends on the freedom of thought entirely. It is therefore determined in an arbitrary manner [durch Willkür] and, as an arbitrary decision must have a reason nevertheless, by our inclination and our interest. The final reason for the difference between the idealist and the dogmatist is therefore the difference in their interests."

218. Here once more the familiar problem of alienation arises: what is the result of our own activity becomes separated from it, and assumes an existence of its own. The connection with our intentions and our wishes becomes more and more opaque so that in the end we, instead of leading, follow slavishly the dim outlines of our shadow whether this shadow manifests itself objectively, in certain institutions, or subjectively, in what some people are pleased to call their "intellectual honesty," or their "scientific integrity." (" . . . Luther eliminates external religiousness and turns religiousness into the inner essence of man . . . he negates the raving parish-priest outside the layman because he puts him into the very heart of the layman." Marx, *Nationalökonomie und Philosophie*; quoted from Marx, *die Frühschriften*, ed. Landshut, p. 228.

In the economic field the development is very clear: "In antiquity and in the Middle Ages exploitation was regarded as an obvious, indisputable, and unchangeable fact by both sides, by the free as well as by the slaves, by the feudal lords as well as by their bondsmen. It was precisely because of this knowledge on the part of both parties that the class structure was so transparent; and it was precisely because of the dominance of agriculture that the exploitation of the lower classes could be seen in the strict sense of the word. In the Middle Ages the serf worked, say, four days and a half per week on his own plot of land and one day and a half on the land of his master. The place of work for himself was distinctly separated from the place of serfdom . . . Even the language was clear, it spoke of 'bondsmen' ['Leibeigene,' i.e., those whose bodies are owned by someone else] . . . of 'compulsory service' ['Fronarbeit'] and so on. Thus the class distinctions could not only be seen, they could also be heard. Language did not conceal the class structure, it expressed it in all desirable clarity. That was true in Egypt, Greece, the European Middle Ages, in Asiatic as well as in European languages. It is no longer true in our present epoch . . . Workers in early capitalism spent their whole time in the factory. There was neither a spatial nor a temporal separation between the period they worked for their own livelihood and the period they slaved for the capitalist. This led to the phenomenon I have called . . . the 'sociology of repression.' The fact of exploitation was no longer admitted and the repression was facilitated because exploitation could no longer be seen." Fritz Sternberg, *Der Dichter und die Ratio; Erinnerungen an Bertolt Brecht* (Göttingen: Sachse und Pohl, 1963), pp. 47ff. Exactly the same development occurred between Galileo and, say, Laplace. Science ceased to be a variable human instrument for exploring and changing the world and became a solid block of "knowledge," impervious to human dreams, wishes, expectations. At the same time the scientists themselves became more and more remote, "serious," greedy for recognition, and incapable and unwilling to express themselves in a way that could be understood and enjoyed by all. Einstein and Bohr, and Boltzmann before them, were notable exceptions. But they did not change the general trend. There are only a few physicists now who share the humor, the modesty, the sense of perspective, and the philosophical interests of these extraordinary people. All of them have taken over their physics, but they have thoroughly ruined it.

It is even worse in the philosophy of science. For some details, see my papers "Classical Empiricism" and "On the Improvement of the Sciences and the Arts, and the Possible Identity of the Two," in *Boston Studies in the Philosophy of Science*, vol. III, ed. R. S. Cohen and M. W. Wartofsky (Dordrecht: Reidel, 1968).

219. Popper has repeatedly asserted, both in his lectures and in his writings, that while there is progress in the sciences there is no progress in the arts. He bases his assertion on the belief that the content of succeeding theories can be compared and that a judgment of verisimilitude can be made. The refutation of this belief eliminates an important difference, and perhaps the only important difference, between science and

the arts, and makes it possible to speak of styles and preferences in the first, and of progress in the second.

220. Cf. B. Brecht, "Ueber das Zerpfuecken von Gedichten," *Uber Lyrik* (Frankfurt: Suhrkamp, 1964). In my lectures on the theory of knowledge I usually present and discuss the thesis that finding a new theory for given facts is exactly like finding a new production for a well-known play. For painting see also E. Gombrich, *Art and Illusion* (New York: Pantheon, 1960).

221. "The picture of society which we construct we construct for the river-engineers, for the gardeners . . . and for the revolutionaries. All of them we invite into our theater, and we ask them not to forget their interest in *entertainment* when they are with us, for we want to turn over the world to their brains and hearts so that they may change it according to their wishes." Brecht, "Kleines Organon für das Theater," *Schriften zum Theater* (Frankfurt: Suhrkamp, 1964), p. 20; my italics.

A Picture Theory of Theory Meaning

The standard view of scientific theories that Herbert Feigl has pictorially set out here is very helpful in some respects. It is a representation of a scientific theory and the role that it plays with respect to the over-all scientific activity in terms of the syntactical calculus of a theory, the observation statements, and the correspondence rules (the coordinating definitions). What has sometimes been characterized as the ideal language conception of the function of a scientific theory and, on the other hand, what has sometimes been characterized as the black box conception of a scientific theory—both stand out in very clear logical relief in terms of this sort of representation. Historically, there are two extremely good cases, one of which would correspond to the ideal language representation and the other to the black box representation.

The ideal language case is the ideal fluid mechanics of Euler and Bernoulli. Fluid mechanics as represented in the post-Newtonian work of these men was a remarkable theory. They saw to it that at the theoretical level the definitions were so sharp and so idealized that the remainder of the algorithm which was generated was very elegant indeed. From the mathematical point of view it is quite a beautiful theory. All that is required is that the subject matter, in this case fluids, be ideal. That is to say, inviscid, irrotational, incompressible, and obeying something called the sine square law. Of course, it turns out that when one makes the algorithm go as beautifully as Euler and Bernoulli did—and to achieve this they had to set out some very clear axioms or definitions by which it could be made a virtual Euclidean instantiation—then a slightly awkward business comes up at the level of the correspondence rules, or the coordinating activities. As it turns out, there are no ideal fluids. In point of fact, the theory doesn't apply to a damned thing, unless one is prepared to bring in lots of additional qualifications at this level. It becomes, then, a sort of general approximation, in terms of which one can have a kind of gestalt for what types of subject matter fluids really are. One must