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On The Social Control Of Science

Glenn Gerard Griener

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THIS DISSERTATION
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ON THE SOCIAL CONTROL OF SCIENCE

by

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Submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy

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ABSTRACT

Increases in science's ability to affect individual's lives bring about calls for more direct social control over scientific research. Defenders of the scientific tradition usually counter these calls by suggesting (a) that any external control would mean the end of the scientific enterprise, and (b) that such control would prove impractical. This dissertation investigates whether these suggestions can be developed into conclusive arguments.

One would expect that if sound arguments against social control are to be found, they will be found in the varied methodologies of science which litter the philosophical field. A scrutiny of several prominent methodologists' writings shows that this expectation is in vain. Moreover, it uncovers reasons why a conclusive argument against such control cannot be found in the mainstream.

Modern methodologies oscillate between the poles of prescriptive and descriptive. Descriptive methodologies can only paint pictures of how science is done. They may discover that external control is seldom found in research, but they must prescind from any evaluation of such findings. Only a prescriptive methodology, one which prescribes how science ought to be performed, can hope to present strong reasons against control. But it could do so only if it could show both that the goal of science is a worthwhile one, and that only a science free of external control can expect to attain it. None of the standard methodologies in the field even attempt the first task.

The dissertation then turns from the mainstream to consider the Jurgen Habermas's attempt to establish what the goals of the sciences
should be. Habermas tries to show that these goals can be rationally grounded in deep-seated human needs which cannot be gainsaid. Unfortunately, at the current stage of its development his argument remains inconclusive.

No argument has been found to show that social control over science is illegitimate. But can a means of control be found which is effective yet selective? Two methods are suggested in the writings of those writers considered here: Feyérabendian anarchism and Habermasian control by professionals. The advantages and disadvantages of each proposal are briefly considered.

In the end it is concluded that while no one has proven that control over scientific research is illegitimate, no one has offered a clear picture of how such control could be exercised.
Acknowledgments

Tradition demands one who reaches the resting places of intellectual journeys acknowledge those who have helped him get so far. This is a demand I gladly meet because it is right.

I owe very special debts to three people.

Jim Leach supervised the journey without providing a detailed map for me to follow. Instead, he pointed out the general direction I should go, with his enthusiasm he convinced me that it was a way worth following, and then he allowed me to plow my own route through the underbrush. My journey without map took far longer than a guided tour would have, but it was far more worthwhile. I know the philosophical landscape surrounding the bits which appear here far better than I would have otherwise. I owe that to Jim.

My dad made me a traveller. Long before I understood, he instilled in me a respect for learning and a desire to know. Looking back I can now see many places where the journey could easily have ended without failure, but quitting almost never appeared as a genuine option to me along the way. That this dissertation stands completed is therefore a testament to my dad.

Joanne has been with me every step of the way. It is not just the case that she has allowed me to spend far too many Saturdays away from her and the kids, put up with my bad moods when I lost my way, and proofread countless pages of manuscript; all of these are things one expects (perhaps unjustly) a wife to do. She has listened to my thoughts long before I committed them to paper, and read them after I had done so. And in doing this she helped me find the way. If my thoughts did not make sense to her, if I could not make her understand,
then I knew that I was still lost. Only when I could convince her that what I had made clear was important, could I rest assured that I was not just playing a philosophers' game. To her I owe the most.
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INTRODUCTION

Recent decades have witnessed dramatic alterations in the relationship between science and society. The forces impelling these changes have come from two distinct directions. Since the costs of conducting scientific research have escalated dramatically, scientists have actively sought public funding for their projects. At the same time, public debates over such issues as environmental degradation and the development of atomic power, both military and civilian, have brought demands from outside the scientific community for greater citizens' control over scientific and technological advances. The debate over recombinant DNA research is in many respects a watershed, involving as it does 'pure' research at the cutting edge of biology, and a clear call by non-scientific, political bodies for some measure of control over this research.

The slightest suggestion of social control over scientific inquiry is enough to send a shudder through the community of scientists. Immediately the specters of scientists-martyrs are raised to demonstrate the danger of allowing practical, political concerns to intrude upon the search for truth. Examples of grave practical failures brought about by acting on politically acceptable but scientifically unacceptable theories are also pointed out. The message is clear: If you wish to act successfully in the world, you must heed the advice of your scientists; but you must never interfere with their work, as this will almost automatically lead to theoretical error and practical failure.

On the other hand, there are compelling reasons for believing that
society at large should have a say in the work of science. An increasing amount of the growing cost of research has been underwritten by governments since the First World War; and a scientist would have to be naive indeed to think that this subvention comes with no strings attached. More important is the fact that the consequences of discovery, be they good or evil, are universally shared. This is as true of improved communications and increased life expectancy as it is of the threat of nuclear warfare. Granted that all share the risks and benefits of science's success, it is at least plausible that all should have a voice in deciding what benefits are worth striving for and at what level of risk.

The scientist is thus forced into a dilemma. In order to pursue his research he must ensure that government funds will continue to flow, but at the same time he must try to avoid the kind of public control that usually accompanies public funding. He must demonstrate his legitimate claim on the public purse while indicating that close public scrutiny and criticism of his work is illegitimate.

One way to establish the legitimacy of the scientist's call for support and for autonomy is to claim that the knowledge he seeks is a good in itself, that it has intrinsic value. Without denying the appeal of this claim, we should note that it is not strong enough for the scientist's political purposes. From a practical point of view this defense does not seem able to command the large resources desired. Classical studies, visual arts, literature and philosophy are but a sample of other fields which pursue goals that are intrinsically valuable, and none of these enjoy financial support of the magnitude required for research. Furthermore, as science becomes more specialized and its
subdisciplines more esoteric, the portion of society able to enjoy
the knowledge gained decreases drastically. The pursuit of pure
science becomes the preserve of small elites, and their call for
support appears to be nothing more than the pleading of one more
special interest group. This appeal to the value of knowledge itself
is therefore at its weakest in providing for that part of science
that is most in need of legitimation—pure research.

The argument under consideration is not only unable to insure
an adequate level of financial support, but it is also unable to
protect the autonomy of science. One can readily admit that know-
ledge is a good in itself and yet deny that its pursuit must always
be unfettered. This is the case because there are other human ends,
such as justice or happiness, that can also plausibly be held to be
valuable, and the pursuit of which can come into conflict with the
pursuit of knowledge. Therefore, the scientist must either establish
that knowledge is not merely a good but the pre-eminent good, or ad-
mit that in the conflict among valuable ends knowledge may come out
second best.

A more direct way of legitimating support of research lies in
showing the importance of science in the achievement of those ends
that society finds valuable: that is, public support of science can
be defended by pointing out its practical value. While this route
has been spectacularly successful in recent years, it is not without
its drawbacks. In the first place, the emphasis on the practical
again threatens to tip the balance in favor of applied research and
against pure. Secondly, this emphasis seems to open the door to di-
rect intervention of public, non-scientific bodies. If science's
value is instrumental, if it is little more than a means to attain society's ends, then social control over science is inevitable. At the very least it should be expected that it will be the community at large, and not the scientific community, that sets research priorities.

These drawbacks can be overcome and the legitimation deployed if a particular description of the scientific enterprise is accepted. While admitting that science does lead to the production of myriad useful commodities, its defenders argue that this aspect is in no way essential to it. The goal of science is proclaimed to be knowledge and it is but a fortuitous coincidence that this knowledge gives power. Utility is merely a by-product of disinterested inquiry. Although useful information is a by-product of research, the argument continues, it must be recognized that this disinterested research is the most efficient process for procuring it. No more efficient way of producing useful information is known; and in particular, no way is known to successfully transform science so that its primary goal is something other than pure knowledge. Finally, it is argued that scientific practitioners best understand the complex workings of their research process, therefore any control of it should be left to their hands, if attempted at all.

A crucial element in this picture of science is the supposition that the scientific method cannot be made over into a more efficient instrument for fixing immediate practical problems. The reason given for this is that scientific discovery is intrinsically unpredictable. There is no way of determining in advance whence the solution to a given problem is likely to come. The defenders of science's autonomy can cite
numerous examples in which practical results in one area come from discoveries in a distant one. Thus Hermann von Helmholtz rhetorically asked, "Who, when Galvani touched the muscles of a frog with different metals, and noticed their contraction, could have dreamt that eighty years afterwards, in virtue of the self-same process, whose earliest manifestation attracted his attention in his anatomical researches, all Europe would be traversed with wires, flashing intelligence from Madrid to St. Petersburg with the speed of lightning?" Furthermore, the development of particular fields of science is also radically unpredictable. Each discipline has its own intellectual problems with which it deals and, as in the case of practical problems, it is in principle impossible to predict which of the alternative approaches will bear fruit. The innovator in any field, the great scientist, is one who has not only studied its problems closely but who also has insight or intuition into probable solutions.

Acceptance of this legitimation and description makes science all but impervious to external critique or control. Against the criticism that there are important social problems that science does not address it can be countered that what now looks like irrelevant basic research, may well turn out to be of immense practical import for these problems. And against the criticism that science has actually created new and intractable problems it can be countered that the criticism fails to distinguish carefully between science and technology. Science, pure research, strives only after knowledge. This knowledge may be used for evil ends, to be sure, but to try to prevent evil by preventing the acquisition of knowledge is bound to be futile since the process of acquisition is inherently unpredictable.
ii. The Recombinant DNA Controversy

The difficulties in the relationship between science and society, and the strategies deployed by the champions of science's autonomy are all evident in the controversy over experiments involving recombinant DNA techniques. A brief look at that debate will therefore be useful in giving some life to the form outlined above.  

The issue which ignited the large public controversy was one which the researchers involved took to be an internal scientific question. Robert Pollock, a young microbiologist, asked whether the experiments contemplated by one of his colleagues might not pose a serious risk of spreading infectious disease; and he went on to suggest the experiments be delayed until the risk could be evaluated and steps taken to counteract it, if it were found to be significant. A number of his fellow investigators agreed with Pollock that there was at least a potential risk, and they publicly called for a voluntary moratorium on such experiments. To the scientists, this private debate and the steps taken to temporarily block some research were not extraordinary events. After all, they argued, scientists regularly take reasonable safety precautions in the conduct of inquiry; moreover, they often discuss these precautions among themselves in order to improve them. As one of the earliest participants in the discussion remarked, "no one has proposed that freedom of inquiry should extend to scientific experiments that endanger public safety."  

Had the debate remained at this level, it would probably have passed unnoticed by the general public; but two things happened to inflame it into a major public controversy. First, people both within and without
the scientific community began to question the tacit assumption that scientists alone should decide on acceptable levels of risk to public safety. Why, they asked, should government cede its legitimate interest in protecting health and safety to scientists who are eager to conduct experiments? This question suggests that there might be a discrepancy between the interests of research scientists and those of society, and that if government fails to take an independent look into the issue of risk, the scientists will be thrown into a typical conflict of interests. Out of these concerns arose attempts, at a local level in Cambridge, Mass. and nationally in the United States Senate, to place legislative controls on research activity.

The issue was further broadened when others began to raise questions about the social consequences of the knowledge which might be gained by performance of the proscribed experiments. It was suggested that the hazards of biological contamination were not the only risks that needed to be considered. It was argued that perhaps recombinant DNA research ought to be banned entirely because it brought closer to reality the possibility of genetic engineering. Fear of the social consequences of an application of research results was thus raised as a primary reason for limiting the basic research.

It is interesting to note that this second issue was raised by some scientists as well as by laymen. For example, the former chairman of the biology department at the California Institute of Technology, Robert L. Sinsheimer, wrote:

Institutions such as Caltech and others devote much energy and effort and talent to the advancement of science. We raise funds, we provide laboratories, we train students, and so on. In so doing we apply essentially only one criterion--
that it be good science as science—that the work be imaginative, skillfully done, in the forefront of the field. Is that, as we approach the end of the twentieth century, enough? As social institutions, do Caltech and others have an obligation to be concerned about the likely consequences of the research they foster?4

While some scientists supported, or at least condoned, the attempt by outside bodies to control or regulate recombinant DNA research, the majority recoiled from the idea. And it is this reaction that is most interesting for our purposes.

A widespread reaction among scientists was shock and chagrin at the suggestion that as a group they might have a vested interest that conflicted with the general good of society. On their view, the very fact that the original moratorium was self-imposed and self-regulated at some cost to individual investigative teams should have reassured the public that the members of the scientific community would protect it from any immediate dangers arising from their work. Closely related to this first reaction was the scientists' belief that their community not only had protected the public safety well, but that it alone could offer this protection. The question of risk, they argued, is a technical one that can only be handled adequately by them. As they repeatedly pointed out, the first safety question was whether the use of the bacterium E. Coli in experiments seriously risked human infections; and this question is surely to be answered, if at all, only by sound scientific study.

The issue of immediate hazard is interesting and vexatious, but it is not what makes the debate over DNA unique. For the scientists are correct in noting that similar concerns need be dealt with in much experimentation, particularly that dealing with human subjects. What is
unique, and what provoked the greatest ire among biologists, was the suggestion that promising investigations should be stopped because they could yield dangerous knowledge. This is an idea that strikes directly at the practice of free inquiry.

A fairly representative response to the perceived threat to freedom is that of the Nobel Laureate David Baltimore. Baltimore contends that controls would be ineffective and that they would be pernicious both to science and to society at large.

The unpredictability of scientific discovery figures prominently in Baltimore's argument that selective controls could not succeed. Assuming that we could decide which areas of knowledge would probably prove harmful to society, he argues that measures designed to stifle discovery in these areas would prove futile because "successes are likely to come from unpredictable directions." Considering Sinsheimer's caution that research into the aging process carries serious danger of social disruption, Baltimore writes:

Major breakthroughs cannot be programmed. They come from people and areas of research that are not predictable. So if you wanted to cut off an area of fundamental research, how would you be able to devise the controls? I contend that it would be impossible. You could close the National Institute of Aging Research, but I doubt that any major advance in that field could be prevented. Only the shutdown of all scientific research can guarantee such an outcome.  

The conclusion of this argument is that only the pace of scientific discovery can be controlled, and this minimal control is managed by the rough tool of financial constraints.

Baltimore goes on to argue that the attempt at control would ultimately prove detrimental to society as a whole. The central core of the reasons he gives for this conclusion has to do with the creative force of
discovery. While it is probably true that science is disruptive of the existent social order, this need not trouble us unduly since a) society will be able to establish a new equilibrium; and b) the upheaval will most likely produce new opportunities, thereby increasing freedom. Along the same lines, he contends that only totalitarian leaders, fearful that discoveries could undermine their power, would try to dictate to science. The unifying theme in this set of arguments is that the scientific quest for knowledge is a socially progressive, liberating enterprise; and to stifle this quest would inevitably lead to a repressive society.

Finally, Baltimore invokes the putative distinction between pure research and technology, claiming that his arguments against control apply only to the former.

The arguments pertain to basic scientific research, not to technological applications of science. As we go from fundamental to applied, my arguments fall away. There is every reason why technology should and must serve specific needs. Conversely, there are many technological possibilities that ought to be restrained.  

The crux of Baltimore's position is that unfettered inquiry is intrinsically progressive. Production of new knowledge opens up new possibilities for action and this increase in the number of choices for action amounts to an increase in freedom. This, however, is as far as science can take us. Which of the possibilities are actualized, how the knowledge is utilized, are questions of technological application. Genetic research may make it possible to eliminate certain hereditary diseases or to clone an army of made-to-order warriors, but this is the limit of research's power. On Baltimore's view, the investigator plays no part in making the decision of which possibility to actualize, so he
bears none of the responsibility for the consequences of that decision. A society which wishes to prevent the creation of an army of clones should exert its power at the level of technological decision making. It should not attempt to limit the range of possibilities, but to insure that the proper choices from these are made.

It is hard to read the scientists' contributions to the recombinant DNA debate without feeling that they are either extremely naive or disingenuous. Almost without exception these writings take for granted an idealized view of science and of science's place in society.

The suspicion of naivete is strongest concerning the charge of conflict of interests. That scientists should be surprised at this charge illustrates the degree to which they have heretofore avoided public scrutiny. For in reality allowing the scientists interested in furthering their careers by conducting a set of experiments to decide how safe those experiments are, or should be, is little different from allowing utility companies to decide how safe nuclear reactors are, or should be, or from allowing any manufacturers to decide actual or desirable safety levels for their products. Manufacturing and utility companies do, of course, have an immediate financial interest in proving the safety of their products or equipment, an interest which scientists apparently lack. But to argue that this difference is a fundamental one is to take far too narrow a view of interests. Even if scientists have no financial incentive to prove their proposed work safe, they do have a strong interest in going ahead with experiments. Not only might they have a purely intellectual interest in answering questions, but since scientific prestige and support depend so critically on making original contributions through discovery, the individual scientist also has a
strong interest in pursuing experiments as quickly as possible.

David Baltimore's rather standard distinction between science and technology and his belief that science inevitably increases freedom also suffer from an idealized picture of science's position in modern society. He ignores the fact that in the present world the link between science and its technological application is particularly intimate; close enough for the scientist to be able to foresee with some accuracy how his discoveries will be used. Although it may be comfortable for the scientist to absolve himself of responsibility for the untoward consequences of his 'pure' research, this absolution loses its potency the more foreseeable those consequences are. When the consequences are both obvious and clearly unacceptable the scientist can invoke Baltimore's defense only at the expense of his human responsibility. The cost of this defense to the prestige of science is made clear by looking to the literary tradition which produces the figures of Dr. Faustus, Dr. Frankenstein and their associates.  

Baltimore's sanguine opinion that scientific inquiry is always in the service of greater freedom similarly seems to have its roots in his ideal view of science. An opposing picture of the liberating potential of discovery is presented by the historian of science, Loren Graham:

Scientific theories and technological innovations are thrust upon societies in ways which may either buttress or undermine existing value preferences. Established groups in society are usually more successful than nonestablished ones in turning science and technology to their advantage; therefore, there is an inevitable tendency for science and technology to buttress the values of ruling classes or political groups.  

A close reading of Baltimore's argument reveals the extent to which he relies on unarticulated philosophical theses. His contention that the socially disruptive effect of science is unproblematic, for example, can
be made cogent only by acceptance of a non-cognitivist value theory. His argument is that whenever a society based on certain values is disrupted a new social equilibrium, based on different values, will be established. This view is plausible only if it can be assumed that it is impossible to argue convincingly that the old replaced values are superior to their replacements. That is, the supersession of social values can be guaranteed unproblematic only by assuming either that the change is necessarily progressive or that there is no way to argue the relative worth of conflicting values. It is hard to see how anyone could claim at the end of the twentieth century that all social change is progressive, so it appears that Baltimore is left with the alternative that in the realm of values well reasoned argumentation is out of place.

The central thesis of Baltimore's contribution likewise needs to be supplemented by tenets borrowed from philosophy; in this case the philosophy of science. His central claim, that efficient control of science is impossible due to the inherent unpredictability of discovery, depends crucially on a particular model of scientific development. This claim is tenable only if science grows through a series of radically novel discoveries and the postulation of new theories. Granting that science is nothing more than an ongoing saga of truly revolutionary change makes it impossible to foretell its future, and thus assures the futility of controls. If, on the other hand, scientific development can be portrayed as proceeding in a more orderly fashion, then the unpredictability thesis in its strong form fails, taking with it the claim that science is un governable. The picture of science as a slow gathering of new information and the gradual widening of the scope of extant theories is one which allows for effective control.
Philosophy of Science as Methodology.

A surprising facet of the protracted public controversy over recombinant DNA was the general silence of the professional philosophers of science. That debate was carried out almost exclusively between the lay public and the practicing scientists. One reason for this silence is that arguments offered by scientists in their own defense quite adequately represent the position to which many philosophers of science would subscribe. But a deeper reason is that such controversies between society and its science are seen as lying outside of the domain of competence of contemporary philosophy of science. Ever since logical positivism garnered a dominance in the field, philosophy of science has been pursued as a methodology of science. The task of the methodologist is generally taken to consist entirely of studying the methods used by scientists. He may undertake this by means either of detailed scrutiny of the actual work done by individual scientists (usually historical personages) and research teams with the aim of accurately describing practice, or of producing idealized logics detailing how science should be practiced.

By virtue of this shift to methodology the philosopher becomes a partisan of science. He accepts without argument the goals which scientists themselves proclaim; and he also generally accepts their claim to have achieved these goals. The methodologist forgoes any right to pass judgment on the goals chosen by scientists; and, in particular, he refuses to become involved in those cases where these scientific goals conflict with the goals of other segments of society. (Hence the silence on the DNA issue). The most he can competently contribute to such disputes
is information whether certain organizations of, or constraints on, the scientific community are liable to be detrimental to the progress of research.

This view of the philosopher's role has not gone unchallenged. The critical theorists of the Frankfurt School were among the first philosophers to object to the delimitation of the realm of philosophy, and to point out the inherent dangers of that approach. Their writings, most importantly those of Max Horkheimer, consistently called for a broader conception of philosophy of science; one which could be formulated as a critique of science.

A principal focus of the critical theorist's attack was the positivistic bifurcation of facts and values, and subsequent assignment of values to the sphere of the irrational. Within the positivists' program science was conceived as a completely value-free enterprise designed to acquire factual knowledge. Methodology was therefore concerned to discover within scientific practice that core value-free method which was so powerful in settling questions. For the logical positivists as well as for their intellectual successors, this scientific method is set up as the epitome of rational cognitive procedure. (The classical positivists' dictum, of course, was that any proposition that could not be handled by this method was meaningless; and, although no modern methodologists would accept this stringent dictum, most continue to directly link epistemic rationality with science's methods.) Now since it is impossible to settle value disputes in a scientific way, it follows that such discussion falls outside the realm of the rational. Evaluative questions cannot be answered by a rational discourse.

The dichotomy of facts and values presents an immediate difficulty
for a proponent of science. It leads to the conclusion that he cannot answer the first half of the dilemma of science in society: he can offer no rational reply to the question of why science ought to command the resources desired. Unless he is willing to allow that the goal of science is the production of useful commodities (and thus surrender the claim that science is a purely epistemic enterprise) the consistent methodologist must admit that this promotion of scientific method is irrational. He must like Karl Popper, confess that his rationalism is limited and grounded on a faith:

My rationalism is not dogmatic. I fully admit that I cannot rationally prove it. I frankly confess that I choose rationalism because I hate violence, and I do not deceive myself into believing that this hatred has any rational grounds. Or to put it another way, my rationalism is not self-contained but rests on an irrational faith in the attitude of reasonableness. I do not see that we can go beyond this.¹⁰

The grounding of scientific rationalism on faith is for science either a source of great strength or its Achilles' heel—depending on the extant constellation of social powers. For Max Horkheimer and the first generation of critical theorists it was the vulnerability of science that was most evident, while for Jürgen Habermas and the second generation it is the imperviousness of scientific rationality to criticism that is obvious.

Horkheimer's *Eclipse of Reason,*¹¹ written near the end of World War II and from his exile in the United States, is an eloquent cri de coeur concerning the dangers inherent in the separation of value consideration from scientific rationality. After tracing this historical development of the bifurcation he turns to a consideration of its most recent reformation by the positivists.

One consequence of positivism's bifurcation of facts and values and
its descriptive methodology is that it is forced to treat science ahistorically. It simply refuses to acknowledge that scientific method has evolved to its present form. No one, least of all the positivist, would argue that modern method is identical with Aristotelian method. However this should force the positivist to recognize that modern methodology must worry about legitimation. He must now face the question: Why is current scientific method to be valued and emulated?

Traditionally philosophers had answered such queries by saying that science is valuable because it attains truth. This claim is supported in turn by a discussion of values and by a discourse on the meaning and criteria of truth. Thus in order to legitimate current method, one must deal with both value theory and epistemology as these disciplines were traditionally understood. Horkheimer makes it quite clear that this is how science ought to be defended, in terms of its claims to attain truth. But positivism has already declared these two areas of philosophy to be meaningless. The ahistoricity of positivistic methodology leaves it open, on the one hand, to the contention that science is merely a product of various social forces. On the other hand, it leaves science defenseless against those who can muster sufficient social power to impose a new methodological orthodoxy on it by whatever means. It is this second possibility which particularly excited Horkheimer. Thus he wrote:

It is true that even in Germany, the notion of Nordic mathematics, physics and similar nonsense played a greater role in political propaganda than in the universities; but this was due to the momentum of science itself and to the requirements of German armament rather than to any attitude of positivist philosophy, which after all reflects the character of science at a given historical stage. If organized science had yielded completely to the Nordic requirements, and had accordingly crystallized a consistent methodology, positivism would eventually have had to accept it.12

Horkheimer's argument has the effect of turning a favorite rhetorical
device of positivistic philosophers against them. It is not a serious
critique of science—one which brings into question goals as well as
methods—that gives rise to perversions such as the Lysenko episode,
but the blind refusal to delve into matters of values.

Jürgen Habermas's central concern is that the excessive emphasis
on rational scientific methods threatens to impose unscrutinized values
on all of society, but under the guise of value-neutrality. It is no
longer the case that science is threatened by social forces espousing
different values. Instead, the current danger is that science is being
used purely as a means to advance vested interests of particular seg-
ments of society.  

iv The Way Ahead.

The motivating concern of this dissertation is the issue of social
control over scientific research. The primary question is whether such
control is ever legitimate. Should we decide that it is legitimate,
then a second question arises: how is this control to be effected?

It has been suggested already that there are two distinctive ways
of arriving at a negative answer to the first question. One route claims
that science has a simple goal—truth—and assumes that this goal is so
valuable that its attainment should not be jeopardized for any reason.
The second says that science produces information; information which
is sure to be vital in the pursuit of many diverse goals. Philosophers
of science, when they consider the issue of control at all, prefer the
first route: scientists, when they need to defend their position, take
the second.

Insofar as the question of control is a political one, the scien-
tists' preferred argument is the stronger one. Yet the dissertation begins by considering a diverse grouping of philosophers of science. This starting point clearly needs some defense. Why begin with the philosopher's argument? And why consider these particular philosophers?

The answer to the first question is, in part, practical. Scientists claim that outside interference would hamstring their creative work, but they fail to support this claim. All one finds at crucial points in their writing is reference to scientific horror stories. This should not be too surprising. After all, scientists are paid to do science, not to write treatises on method or historical monographs. Philosophers of science are the ones who have made the explication of method their special task. So any demonstration that external control must disrupt scientific progress is more likely to be made by philosophers. Moreover, since the philosophy of science has self-consciously become more historical, any demonstration that control has disrupted scientific progress in the past also seems more likely to appear in the philosophical literature.

There is a more serious reason for the focus on philosophers. The apparently diverse arguments preferred by scientists and by philosophers share a common structure, a structure largely determined by their common representation of science. Science is characterized by method. It is not its goals which distinguishes science from undertakings, but its way of pursuing them. Information and putative truths which do not exhibit proper methodological credentials, which have not been arrived at in the right way, are automatically discounted as unscientific. This agreement that science is a methodical goal-directed activity allows the question of its goal to be held in abeyance. Attention can be con-
centrated on the shared elements of the arguments: namely, the beliefs that there is a method to doing science and that scientists understand this method while laymen do not.

Social control is most often seen by its opponents as interference with the scientific method by those who do not understand it. And this interference is to be resisted because it will make science less efficient. But this reason for resisting control needs support. The crucial need is to show that there is a method which scientists do follow in their work, and which excludes any external control. It is the philosophers of science who try to describe scientific method, so it is to their work that we must turn first in seeing if this need can be met.

The selection of philosophers to be considered is fairly straightforward. Thomas Kuhn, Imre Lakatos and Karl Popper are undoubtedly the leading proponents of newer philosophy of science. Their diverse methodologies share an antipathy toward many premises of positivism. Most importantly, all three believe that good methodology must be an accurate representation of actual practice in research. If there is a way to circumvent the difficulties in the older positivistic tradition highlighted by the critical theorists without fundamentally altering the aims of methodology, then it should be found in their work. Of equal importance with the elements they share are those on which they differ. The lively and protracted disputes among these contending schools of methodology clearly pinpoint the weaknesses of each; and the points at which the debates remain inconclusive often mark the issues that cannot be settled because of the limitations of the value-free approach to methodology.
Feyerabend, the fourth philosopher considered in part one, quite simply cannot be ignored. Alone among methodologists he realizes that many of the conundrums and the disputes between alternative methodologies remain insoluble so long as they are treated in isolation from problems of practice. It is not enough to compare suggested methods with actual scientific practice, according to Feyerabend. Rather they must be judged against practice in general—against various forms of life practice. Feyerabend thus serves to return us to the dilemma set out in the leading sections of this introduction.

His opponents often portray Feyerabend as a skeptic whose sole purpose is to find fault with anyone else's work. This seriously underestimate his importance. To be sure, he does try to show that there is no scientific method by disposing of each new edifice produced by methodologists. That attempt is important in itself, given the centrality of the claim that a method exists. Even though his piecemeal arguments against method must remain inconclusive, his place in the current discussion is assured. He alone treats the legitimation of science in contemporary society as a serious—indeed, a crucial—issue in the philosophy of science. By asking "What's so great about science?" he forces the discussion beyond consideration of the methods used and into an investigation of the purposes that these methods are supposed to serve.

Feyerabend's writings form a convenient bridge back to critical theory; his conception of the proper scope for the philosophy of science comes close to that deployed in critical theory. Like Horkheimer and Habermas he rejects the limitation to study of scientific method.

In the second part of this dissertation we take up the central
tenets of the critical theoretic approach to the philosophy of science. There is, however, a serious impediment to the appropriation of these writers. A very commonly expressed criticism of the critical theorists is that they operate with a seriously deficient view of science and the philosophy of science. On this account critical theory offers a cogent attack against logical positivism. But positivism has long since run its course and become an historical curiosity, and so critical theory has lost its topical interest and should follow its opponent into the museum of philosophy.\(^{14}\)

Like most completely devastating philosophical criticisms this attack is an admixture of useful insights and misunderstandings. Most of the methodological writings of the critical theorists do hold up positivism as the antagonist. One reason for this is of course historical. At the time of their composition positivism was the dominant philosophy of science. More important than this, the critical theorists believe that many of the central and objectionable tenets of positivism continue to have a firm hold in the modified methodologies of philosophers such as Popper. There is little doubt that the critical theorists' blanket accusations of positivism have tended to blur many important distinctions between contending schools of philosophy of science, as well as to obscure their common points of opposition to thorough critique.

The useful insight is that, if critical theory is to maintain the compelling elements of its critique, then it must take account of the changed scene in the philosophy of science. Before we can put the arguments of the critical theorists to work we must see if they can come to terms with the alternative methodologies of writers like Popper, Lakatos and Kuhn. They must make good the claim that these modern methodologies
also lead into the same sorts of dilemmas as did their antecedents.

At first sight this need to come to terms with newer methodologies presents us with an enormous task. There is, after all, a wide diversity in the current views of methodologists, so it would appear that in the comparison with critical theory we must treat each methodology separately. The task is made more onerous by the seemingly unending stream of new methodologies and emendations to existing ones.

The situation is not as difficult as it appears, however; for the very diversity of methodologies lightens the task. Much of the critical theorists' dialectical work has already been carried out by the proponents of the alternatives themselves. Each modern methodologist has been as much concerned to criticize the alternatives offered by his fellows as to develop his own position. The systematic review of this ongoing struggle in methodology presented in the opening chapters promises to reveal both the continuities among the alternatives—those assumptions and premises which remain aloof from the debate—and the vulnerable spots in each of them. The unsettled state of methodology represents an opportunity for a renewed critical theory; it provides a rich soil in which to cultivate a more vigorous hybrid of the classical critique of science.

The focus in the second part of the dissertation falls on the problems left unresolves by the analytic methodologists and the relation of these to the shared assumptions of value freedom and the purely epistemic interest of science. Chapters V and VI strive to explain Habermas's writings on scientific method as they relate to the controversies in analytic methodology. This forces an extension of scope to include the philosophy of the social sciences, as well as Habermas's own conception
of critical science.

The final chapter brings together the ideas of Feyerabend and Habermas on how science is to be controlled. Despite the fact that the two men share much in their critiques of methodology, and agree that science can legitimately be controlled, they diverge radically in their assessments of how this is to be done.

The solution Feyerabend gives is an essentially negative one. He argues that science cannot escape the scrutiny of the general public; that it must be judged by this public. Moreover, he argues that this judgment is rightly carried out in whatever terms the public chooses. There is no way of attaining the objective value judgments; whatever judgments are advanced are grounded in a particular tradition and remain valid only for adherents of that tradition. The practical upshot of this is that Feyerabend recognizes the legitimacy of calls for social controls over science, but this control is to be exercised by particular citizens' initiatives based in varying traditions.

The principal difficulty with Feyerabend's proposal lies in the realm of the practical. On his view, the struggle to control science is a contest between opposed special interest groups: between the proponents of science, on the one hand, and of various traditions on the other. But the contestants are of disparate strength. As Feyerabend has noted throughout his work, science has already largely broken antagonistic traditions. The contest he envisages therefore seems to have a predetermined outcome.

A vigorous counterpoint to Feyerabend's negative solution is found in the works of Habermas. Habermas agrees that the dilemma of science in society cannot be ignored, but he is far less sanguine about the
success that isolated citizens' initiatives can hope to have against the massed forces of science, government and business. He diverges from Feyerabend in trying to uncover a method whereby values can be grounded, and thus tries to provide sound footing for the social control of science. Habermas and Feyerabend also differ radically in their assessments of the cogency of the fact-value distinction.

Before we can begin to come to grips with these issues it is necessary to deploy some terminology to be used in understanding the often confused debates over contending analytic methodologies. It is important to construct schemata for clarifying the differences among the alternatives.

Foremost in importance is the distinction between normative and descriptive methodologies. For the descriptive methodologist the preeminent task of a successful methodology is to give as accurate as possible a depiction of the actual procedures of working scientists. He begins his study without an articulated view of scientific method, or at least with a view that is amenable to alteration on the basis of sociological and historical findings. The normative methodologist, by contrast, starts with an idealized picture of how science should proceed. He generally posits a goal for science and then sets out the methods by which this can be attained.

Given their differing perspectives on the task of methodology, it is not surprising to discover that the two schools also disagree on the means of evaluating methodology itself, and on its proper relationship to scientific practice. Descriptive methodology is to be judged entirely in terms of its accuracy: a particular variant stands or falls according to how well it captures what scientists do. Normative
methodology can stand removed from practice. It is judged by whether the methods it lays down would allow the achievement of science's goal. The comparison of normative methodologies is in terms of their efficiencies in this task.

The normative methodologist sees himself as an arbiter of scientists' practice. He offers strong prescriptions of the form: "If you wish to be scientific, follow these methods". Actual practice which does not follow these strictures can be dismissed as unscientific, or pseudo-scientific. Although normative methodologies could, in principle, be used to stigmatize vast areas of current research as pseudo-science, it is usually only particular disciplines or subdisciplines that fall under censure. The descriptive methodologist is much more lax in his prescriptions. Any norms he presents are merely those found in the real world of research; norms operative in scientific communities. They therefore take this form: "These are the methods which the contemporary community of nuclear physicists follow. If you wish to become a member of this community, imitate this practice". This methodologist explicitly shuns the task of reforming the methods in use in thriving research communities by, for example, suggesting that they adopt a logically exact language or imitate methods followed by other successful communities.

Each of these approaches to methodology has its own characteristic weakness. A normative methodologist must remain forever wary of falling into the trap of spinning a priori methods which then go completely ignored by successful scientists—a trap Descartes is often accused of having fallen into. He runs the risk of irrelevance. The descriptive methodologist must be concerned about missing the essential and unique
elements of science in his rush toward accurate depiction of real practice. This danger is clear at the very starting point of study. He must decide what fields of practice are sciences, and therefore worthy of his notice as methodologist of science. Conventional wisdom may be useful in classifying some disciplines as sciences, but at the edges of research such wisdom begins to break down. Here the methodologist must decide whether entire areas of inquiry (e.g., research into the paranormal) ought to be included in the scope of his study. To the extent that he is willing to allow scientific disciplines and specialities to evolve their own unique methods, he is unable to invoke the measure of "scientific method" to make his decision. At some point his study must cross over into a mere description of whatever social group happens to interest him; that is, it shifts from being a methodology of science to being mere sociology.

We should note that something is lost in this shift from methodology to sociology. The methodologist can legitimate his enterprise in a way that the sociologist cannot. He can argue (although this argument is never made explicit, because the call for legitimation remains unheard) that his study is important because it allows us to better understand, and perhaps improve, an important institution in our society. The importance of the methodology of science therefore is parasitic on the importance of science itself. Once the unique status of science is dissolved, this legitimation disappears. At the level of pure descriptive sociology a study of the methods of scientists is on all fours with studies of the method of chess players or of professional wrestlers.

None among the leading philosophers of science maintains either a purely normative or a purely descriptive methodology. The differences
among them are differences of emphasis.

Popper, in his earliest writings, assumes that these two aspects coincide in his critical rationalism; that is, he takes it for granted that his methodology not only gives an accurate portrayal of how real science proceeds, but also lays down useful norms of how it should proceed. It can thus be used to understand and correct practice. He avoids the pitfall of irrelevance by choosing a conspicuously successful science as a model and then working out a model which roughly characterizes this practice. The final step is the proclamation that this is the method which ought to be followed by anything wishing to legitimately call itself a science. There remains in this procedure a great deal of flexibility in the fit between pure method and messy practice. Where this flexibility generates difficulties—where practice seems to diverge too far from the norms—Popper generally settles the issue in favor of the normative methods. The researcher whose actions do not follow the prescriptions is being unscientific, if only temporarily.

The happy coincidence between norms and descriptions did not become contentious until the appearance of Kuhn’s writings. He set out to tip the balance in favor of descriptive accuracy by showing that the then standard philosophies of science, including Popper’s, did not adequately picture science. In particular, he wanted to demonstrate that they could not be used to explicate most of the key developments in the history of science without distorting the available historical evidence.

Kuhn avoids the problem of deciding what practices constitute science in the first place by accepting the consensus on past historical events. The discoveries of Galileo, Lavoisier, Priestley, etc., are all
breakthroughs which a methodology must study. In the second place, Kuhn and other descriptive methodologists join with the normative school in distinguishing science from other enterprises in terms of its ends. Both schools hold that science is an epistemic undertaking; its goal is the production of pure knowledge. This understanding of the goal of science is not one of the issues over which methodologists remain divided. The agreement is, however, deceptive, for their divergent explications of the theory of knowledge do leave a large gap between the schools—as will become clear in what follows.
NOTES


2 One issue not raised in this controversy is the need for financial support of science. For a typical argument in favor of support see Research Universities and the National Interest, (New York: Ford Foundation et alia, 1978).


4 "The Presumptions of Science," Daedalus 107 (Spring, 1978), p. 27.

5 David Baltimore, "Limiting Science: A Biologist's Perspective", Ibid., p. 43.

6 Ibid., 42-43.

7 Even this weak difference does not hold in the area of recombinant DNA research. Some genetic scientists have established their own private laboratories in order to capitalize on the commercial possibilities of their discoveries.


13 It bears emphasis that Horkheimer and Habermas point out different aspects of the same problem. It is the failure to legitimate science, by means of value theory and epistemology, which gives rise to both phenomena. Furthermore, both writers agree that the task of legitimating science must take account of the relationships between science and the rest of society.

CHAPTER ONE

1. Max Weber on Value Neutrality

The thesis of the value-freedom of the natural sciences is so deeply entrenched in standard methodologies that it is seldom explained, and almost never argued for. It is only in discussions of the methodology of the social sciences that moral autonomy is brought into question. Even in this area, however, there is seldom found a cogent argument in defense of this thesis. Proponents of the standard view are usually content to settle the issue by asserting that the physical sciences remain free of moral elements and then arguing that the social sciences—if they wish to become progressive and truly deserve the honorific title of 'science'—ought to imitate this successful tactic. This lack of an explicit articulation of the thesis, particularly as it operates in the natural sciences, has undoubtedly generated much confused, fruitless debate. Avoidance of this pitfall makes it necessary to spend some effort in getting clear about precisely what is at issue.

A most influential statement of the thesis is found in the methodological writings of Max Weber. Weber's writings are important because he accomplishes three interconnected tasks. First, he gives criteria for distinguishing factual, scientific, discourse from evaluative discourse. Second, he points out the difficulties which arise when this distinction is ignored. He thereby gives an argument for expurgating values from science. Finally, he indicates a number of places where the
realms of fact and of value may come into contact without either being imperilled.

In formulating the thesis, Weber was explicitly concerned only with the social sciences: his intention was to outline an acceptable method for the social scientist to follow. This exclusive focus of attention presents no insuperable problems however. In the first place, it is obvious that insofar as this particular tenet is concerned, Weber wished to claim a unity of science. The expurgation of value judgments from their propositions is to be a common trait of both the physicist and the scientific sociologist. Furthermore, this provisional assumption of a methodological unity of science presents no difficulties to the position we will explore here. The argument to follow does not attempt to establish as a point of logic that the social and natural sciences must have diverse methodologies.

One feature of Weber's methodology, a feature which strikes the reader immediately, is that it is a normative one. His arguments are advanced with the ardor of a crusader against the abuses of his day. Large segments of his writing are attacks directed at the practices of his contemporaries in the academic social sciences. Some of the particular arguments therefore necessarily refer to the idiosyncratic conditions of the German academic world at the beginning of this century. For example, he rails against the advocacy of political positions from the professor's lectern because "certain value-questions which are of decisive political significance are permanently banned from university discussion." 1

The crusade against formerly topical abuses should not conceal the fact that Weber's underlying interest was in the fundamental problem of
determining when and how the individual who undertakes science as a profession can legitimately become involved in the political issues of the day. Despite the individualistic bent of his formulation, it is clear that Weber's guiding concern is the same as ours. Behind the question of how the scientist should comport himself in the face of practical public concerns lies the question of how science can be used in dealing with problems of public policy.

Weber's solution of this issue relies on making a clear-cut distinction between a person's roles as scientist and as citizen; with the dividing line being drawn according to whether any specific question is one of fact or one of value. The scientist qua scientist is competent to definitively settle factual questions, but in the realm of value decisions the role of the scientific expert disappears entirely. According to Weber, all remain equal as citizens in making these decisions. No issue of public policy, however, is purely factual. At the very least, the setting of goals to be pursued inextricably involves value choices.

That there is an unbridgeable gap between the realms of fact and of value is a recurrent and crucial theme in Weber's methodological writings. Thus he writes that methodology "claims for itself only the right to state that certain problems are logically different from other problems and that their confusion in a discussion results in the mutual misunderstanding of the discussants. It claims furthermore that the treatment of one of these types of problems with the means afforded by empirical science or by logic is meaningful, but that the same procedure is impossible in the case of the other." The question this clearly raises is that of how these problem areas are to be distinguished.

It is here that the bedrock upon which Weber's entire argument ulti-
mately rests is revealed. What distinguishes the realm of fact is that there is a method which insures that all conscientious investigators will eventually reach the same conclusion. As he puts it, "a systematically correct scientific proof in the social sciences, if it is to achieve its purpose, must be acknowledged as correct even by a Chinese—or—more precisely stated—it must constantly strive to attain this goal, which perhaps may not be completely attainable due to faulty data." Any legitimate scientific question must be assumed to have a definite answer, an answer on which a consensus can be attained. In the realm of values, by contrast, there is no original consensus; and more seriously, there is no method whereby one can be achieved. "[T]o judge the validity of such values is a matter of faith."4

Having laid down this strict dichotomy, Weber then proceeds to a discussion of the legitimate and illegitimate ways in which these diverse realms of discourse may interpenetrate. Two kinds of mistake are obviously possible: on the one hand the methods of science can be used in an attempt to resolve the putatively irresolvable disputes over evaluations; while on the other, the individual scientist might let his personal values influence his scientific output.

Weber counters the first form of mistake by taking particular examples of it and showing wherein they err. For instance, he argues against attempts to derive moral norms from the sociological observation of the trend in modern life of progressive rationalization. He contends that the scientifically proper use of the term 'progress' in such contexts is a limited one. Only to the extent that technically more efficient means of achieving a set end are consciously adopted is the term applicable. Science can talk of means being progressively improved; it cannot speak
meaningfully of the goals themselves progressively improving. It therefore remains meaningful to ask if the progressive rationalization of life is good. But no consensus should be expected.

While discrediting all attempts to produce a science of ethics, a science which pretends to establish goals, Weber admits that the inter-subjective discussion of values does have its place. At the individual level this can lead to an increased clarity about the value axioms chosen. And at the social level, in the realm of public policy, the scientific discussion influences policy by showing which means are available for the achievement of the goals, by determining the comparative costs of the alternative means, and by pointing out the unintended consequences of the successful quest for it. The scientific investigation will possibly show that the goal is in fact unattainable, or that its attainment is extremely costly in terms of other desired ends. In either of these cases the scientist will clearly have a very strong influence on decision making, but Weber emphasizes that the scientist cannot go beyond this factual analysis. "To apply the results of this analysis in the making of a decision, however, is not a task which science can undertake; it is rather the task of the acting willing person: he weighs and chooses from among the values involved according to his own conscience and his personal view of the world."5

The hallmark of any scientific question is that in principle it has a definite answer. Now this answer may prove elusive at any given time, and this elusiveness may be traced to any of several reasons. Weber notes that the scientific consensus may fail to materialize because of "faulty" data. The most straightforward (and for Weber the least interesting) case is where there is simply not enough data to answer the question.
Such a situation is obviously to be remedied by more research. But the data could be faulty in another way: it could be that a researcher submits evaluative statements as factual ones. That is, instead of merely reporting the empirical facts he may allow his personal evaluations to embellish them, or even in the extreme to misrepresent them. Similarly, a consensus may fail to form because individuals allow their value judgments to influence their acceptance or rejection of facts. Whenever these latter faults appear, science itself is threatened because communication between those holding different values is made more difficult or even impossible. "[P]ersonal value-judgments have tended to influence scientific arguments without being explicitly admitted. They have brought about continual confusion and have caused various interpretations to be placed on scientific arguments."  

What is at stake in keeping evaluations out of scientific arguments is the very possibility of science. The identifying mark of the scientific argument—the mark which distinguishes it from evaluative arguments—is the possibility of reaching an intersubjectively agreed answer. "[S]cientific truth is precisely what is valid for all who seek the truth." To allow in evaluations, either overtly or covertly, imperils the chances of intersubjective agreement, and hence of attainment of scientific truth.

There are a number of places where Weber does allow to evaluations a legitimate influence on science. In the first place, the individual may choose to subject a specific problem to scientific scrutiny because of its importance to him. Such an evaluation of importance need not be made on purely epistemic grounds; indeed it may be made on the basis of practical concerns. This evaluation of certain problems as more worthy
of study than another presents no danger to science, because science proper does not begin until this selection is completed. Secondly, Weber allows that the explanatory conceptual scheme chosen may even be chosen in accord with the investigator’s personal values. This also presents no danger as long as the concepts satisfy the independent scientific norm of being the "most unambiguously intelligible". In the terminology of later philosophy of science, both the selection of issues and the construction of explanatory systems fall within the context of discovery; and scientific argument takes place almost exclusively in the context of justification.

Finally, Weber argues that the product achieved by following the canons of scientific methodology is worthwhile in itself (in addition to being valuable because of its prospective applications), but again he insists that this judgment is made outside the arena of scientific discourse. Along these lines he contends that the prospective scientist must decide beforehand to undertake science "as a vocation."

In summary, Weber’s argument depends on a clear distinction between evaluative statements and factual ones. Evaluations are radically subjective, depending on personal choice rather than public argument, and being underwritten ultimately by faith. Factual statements fall entirely in the public realm, and their validity is underwritten by intersubjective agreement. Individuals may hold any values, but if they wish to become members of the scientific community they must accept certain norms, norms which assure the possibility of consensus.

Before passing on to other writers we should note that Weber is far more sophisticated and forthright in his argumentation that are many of his popularizers. For example, he does not merely assume that the factual
knowledge science generates is obviously and undoubtedly valuable. Instead he acknowledges that "the belief in the value of scientific truth is the product of certain cultures and is not a product of man's original nature." Furthermore, he recognizes that the generation of factual information is not the only conceivable goal of science; thus he points out that the scientist at one time saw his task to be that of showing the way to God. In these insights he presages much of the argument to be presented here.

ii. Popperian Rationalism

Max Weber's taxonomy of the legitimate and illegitimate places of value judgment does not take us very far in understanding the underlying reason why such judgments must be excised from science. He does not undertake the kind of detailed scrutiny of the methods of science that could show the way in which the allowance of personal value judgments would disrupt the process. For such detailed analyses we must turn to mainstream philosophers of science.

The philosophical writings of Karl R. Popper dovetail nicely with Weber's, being an extended look at the methodological questions raised but not settled by Weber. According to his autobiographical account Popper's philosophy of science had its origins in his attempt to discover a criterion for distinguishing true sciences from pseudosciences—particularly social pseudosciences. His guiding problem was that of pinpointing wherein lay the difference between a theory such as Einstein's theory of relativity and other so-called scientific theories such as Freudian psychoanalysis and Marxism. The difference he cites is one of method.

The core tenets of Popperian methodology are by now well known.
At their center is his assertion that science does not produce certain knowledge; thus at the end of his most extended study he writes: "The old scientific ideal of epistêmê—of absolutely certain, demonstrable knowledge—has proved to be an idol." The search for a demarcation criterion must lead, therefore, in some direction other than that of a search for a criterion of truth.

The demarcation criterion Popper claims to have discovered by analyzing the acceptable sciences is that of falsification.

I shall not require of a scientific system that it shall be capable of being singled out, once and for all, in a positive sense; but I shall require that its logical form shall be such that it can be singled out, by means of empirical tests, in a negative sense: it must be possible for an empirical scientific system to be refuted by experience.

The criterion of falsifiability is superior to that of verifiability in that there is a logical asymmetry between falsification and verification. While a universal law can never be verified by any number of singular observation statements, it can be falsified by a singular observation statement. Since the next experiment may yield negative results, one can never be sure that a law is true; but on the basis of one acceptable piece of evidence he can be sure that it is false.

The key to understanding the experimental method is thus to see that the scientist is not trying to prove his theories, rather he is trying to demonstrate that they are wrong. If the scientist seeks counsel in Popper's writings, it will be that he should make as strenuous an attempt as he can to show where his theories and explanations go astray.

With only this sketchiest of outlines before us a very serious problem seems to confront Popper. The commonsense view of both the layman and the practicing scientist is that science seeks the truth. What supposedly makes it a valuable enterprise is its ability to capture this
prize. If the prize is now declared unattainable, what will motivate the quest?

A number of replies are open to Popper here. He could reject the commonsense idea that truth is science’s goal. One version of this would lead to a radical skepticism or 'irrationalism': since science does not yield truth, its methods are nothing special, and not to be preferred to any others one may wish to pursue. Another would have it that the goal of science is simply utility. Scientific method is to be preferred to others because it provides the means of producing whatever it is that improves human well-being. Alternatively he could choose to define 'truth' in such a way as to make it attainable; he could define it to mean usefulness, as he suggests the pragmatists do. Or he could define it to mean that on which all scientists agree, as Weber seems to do.

Popper takes none of these ways out. He insists, in the first place, that truth is to be understood to mean correspondence with reality. He then re-asserts that the goal of science is true explanation. Finally, he holds that science is valuable precisely because it constitutes this search for truth.

The air of paradox surrounding this position— that science is worthwhile because it strives after truth, a goal it can never know it has achieved— is dissolved in the Popperian theory of scientific progress.

[O]ne great advantage of the theory of objective or absolute truth is that it allows us to say ... that we search for truth, but may not know when we have found it; that we have no criterion of truth, but are nevertheless guided by the idea of truth as a regulative principle ... and that, though there are no general criteria by which we can recognize truth— except perhaps tautological truth— there are something like criteria of progress towards the truth.
Science is to be prized because it is a method of getting ever nearer to the true description of reality.

The explication of the notion of scientific progress makes it clear that Popper holds scientific methodology to be essentially comparative. We have no criterion for determining how close to the truth any given theory is, but we do have clear and definitive criteria for saying that one is nearer than another. And it is these criteria which allow us to say that science is progressive. We progress from one theory to another because the latter is closer to the truth.

According to Popper, scientific theory building always begins with problems. Most often the problem facing the inquirer is a refutation of a previously accepted theory. This problem sets the groundrules for what can be admitted even as a potentially satisfactory explanation. The new theory must, of course, solve the immediate problem at hand, but it must also do much more. Since the prior theory itself had been proposed as a solution of earlier problems, and had been tentatively accepted because of its successful handling of these, the new theory will be considered only if it too can solve these problems. If the replacement theory can explain all that its predecessor could and solve the problem that refuted the other theory, but can do no more, then Popper suggests that the new theory is just an ad hoc emendation of the old. By contrast, a new theory which suggests novel observations and predicts unexpected results has greater content than the theory it seeks to replace. To avoid the ad hoc adjustment of old theories, adjustments which do not tell us anything new about the world, Popper stipulates that an honest potential replacement must predict some new results. It must, that is, suggest some novel observations by which we can test it.
Insisting on the two foregoing criteria insures that the continuous displacement of one theory by another is always progressive, in the sense of leading to theories of greater content. Added to his demarcation criterion they insure that we have ever more comprehensive theories which have not been falsified; theories with increasing verisimilitude.

Popper's entire conceptual apparatus of 'truth content', 'falsity content', and 'verisimilitude' is intended to capture the essence of the commonsense idea of progress in science. We need not scrutinize the details of this apparatus beyond, perhaps, the concept of relative verisimilitude. Of that concept Popper gives the following characterization:

a theory $T_1$ has less verisimilitude than a theory $T_2$ if and only if (a) their truth contents [the class of all true statements which follow from each theory] and falsity contents ... are comparable, and either (b) the truth content, but not the falsity content, of $T_1$ is smaller than that of $T_2$, or else (c) the truth content of $T_1$ is not greater than that of $T_2$, but its falsity content is greater. In brief, we say that $T_2$ is nearer to the truth, or more similar to the truth than $T_1$, if and only if more true statements follow from it, but not more false statements, or at least equally many true statements but fewer false statements. 16

It should be clear that this definition of relative verisimilitude cannot give a foundation for anything like a criterion of verisimilitude. (And Popper makes it clear that this was not his intention in formulating it.) 17

For in order to use it as the basis for a criterion we would need to determine the truth content of each theory; i.e., we would need a criterion of truth—something which Popper has, of course, already forsworn.

The lack of a definitive criterion of absolute verisimilitude does not leave the concept sterile. Even without such a criterion there will still be cases in which the intuitive idea will prove quite serviceable; and it seems to be just these cases which Popper wishes to capture and
explain. The notion will prove workable whenever the truth of the background knowledge is unproblematic.

One of the fundamental and unchanging premises of the entire corpus of Popper's work is his thoroughgoing adherence to falsifiability. There is absolutely no part of science which can remain immune from criticism. A reliance on background knowledge that is treated as unproblematically true therefore seems to go diametrically against the grain of his thought. This, however, is only an appearance of heresy.

We noted earlier that a single negative result can lead to the refutation of a universal law. But this claim is surely too glib. As Popper notes, this reliance on singular observation statements merely shifts the problem back one step; for one can always ask whether he should accept this singular statement. At this point we must face, along with Popper, a trilemma. First, we could admit that the basic statements with which we had hoped to refute hypotheses can themselves be rejected on the basis of other statements. But if we adopt this we face an infinite regress. Second, we could accept the basic statements dogmatically, rejecting out of hand any questioning of them. This route is open only at the cost of scrapping the thoroughgoing falsificationism that is the hallmark of Popper's empiricism. Third, we could seek to ground these statements directly in sensory data. But this can be carried through only if we have some way of insuring the proper fit between our language and the world: that is, we can go this way only by accepting what has come to be called "the myth of the given."

None of these alternatives is acceptable to Popper. Instead, he solves the problem of the empirical base by holding that the acceptance of any particular basic statement is not forced, but rests ultimately
upon a decision. Theoretically, there is nothing to stop us from withholding acceptance from any basic statement; nor is there anything to prevent us from withdrawing acceptance once it has been tentatively given. The decision to accept cuts off the infinite regress, while the proviso that this decision can be withdrawn protects the theorist from falling into a dogmatic slumber.

We are now in a position to isolate those cases in which Popper's notion of verisimilitude will help to pick out progressive scientific changes. Whenever two theories are in substantial agreement—that is, whenever the proponents of both have decided to accept most of the same basic statements—then it may be possible to declare that one theory is nearer to the truth. If the scientists agree on the observational results, if they agree that particular experiments give good enough reasons for holding some statements true and others false; then the idea of verisimilitude can come into play without any difficulty.

There is an interesting corollary to Popper's account of the comparison of rival theories that bears emphasis here because it is important to his social science methodology. He admits in principle that a falsification only shows that some part of the complex of a theory plus its background knowledge is mistaken without indicating where the error lies. The researcher faced with a putative falsification could choose to reject either the experimental result that creates the problem, the explanation under test, or some element of the hitherto accepted background knowledge; "and it is sheer guesswork which of [these] ingredients should be held responsible for any falsification." There is no methodological tenet that can guide this decision. The one move that Popper will not countenance, however, is the complete rejection of all background
knowledge.

The reason for this restriction is not hard to find. We should not try to start anew from scratch because this could "easily lead to a breakdown of a critical debate." The upshot of this is that "though every one of our assumptions may be challenged, it is quite impractical to challenge all of them at the same time. Thus all criticism must be piecemeal." 19

It is difficult to imagine what a complete rejection of all background knowledge would be like; so there is apparently little to be lost in accepting this Popperian stricture. Indeed, it seems almost a truism. However, behind the beguiling facade there lurks a menacing unclarity. Even if it is impractical (or impossible) to reject at once all parts of background knowledge, the real question remains of how much can be jettisoned at a time. At what point does it become impractical to reject any more of the traditionally accepted? More radically, at what point does it become reasonable to risk a breakdown of debate in order to gain greater verisimilitude?

We shall have an opportunity to return to these and related queries in the following chapter.

iii. Popper on Value Neutrality

To date we have said nothing about Popper's views on the value neutrality of science, the ostensible topic of this foray into his methodology. His thesis that ultimately a decision is involved in the acceptance of basic statements presents a golden opportunity to reintroduce the issue. One could argue that this decision to regard some bit of information as unproblematic is guided by typically ethical considerations. The point
can be made more general still by claiming that in the event of a falsification it is ethical attitudes which decide whether it is the falsifying observation, the theory on trial, or some element of traditional background information that gets rejected.

The preliminary part of Popper's answer to this suggestion consists of the claim that we have not yet sufficiently emphasized the social nature of science. Science is objective just because its method of testing theories is intersubjective. Popper here departs from Weber's views, and does not try to argue that the individual scientist must somehow purge himself of all extra-scientific value commitments. "If scientific objectivity were founded ... upon the individual scientist's impartiality or objectivity, then we should have to say good-bye to it." Objectivity is rather an aspect of science as a social enterprise, it results from the "friendly-hostile co-operation of many scientists." What this means in practical terms is that the individual may very well make his personal decisions about which basic statements to accept or about which part of a falsification-theory-background-knowledge complex to reject on the basis of some ethical predilection. This violates no ethos of science. But he can be sure that his fellow scientists are not going to feel bound by his choices. Rather they maintain their freedom to criticize any one of his decisions. The case for objectivity is made even stronger by Popper's assumption that scientists share almost no value commitments in common. The individual who bases his decisions on his own ethical values can therefore be sure that one of his fellows who holds conflicting values will subject the decisions to severest scrutiny. The result of such scrutiny will be either the suggestion of different decisions (in which case the scientists need find some way to mediate
their difference or give up communicating) or the mutual acceptance of the decisions. If the latter results it is safe to assume that some grounds other than the value-oriented ones can be found for basing the decision. The individual scientist's recognition that his decisions will be critically challenged tends to have the secondary effect of making him more self-critical of them. Realizing that ethically guided choices will be most savagely scrutinized, he will try to avoid them.

The appeal to the social critical aspect of science does not tell the whole story, however. It remains a possibility on this view that scientists may still disagree in their decisions. The scientist who has chosen, say, to stay with his theory even in the face of a putatively falsifying experiment may later decide to stick with his original choice even in the face of a critical argument by a colleague. Such a possibility would seemingly become more likely if the original decisions of the protagonists were guided by dearly held ethical values.

Popper recognizes this possibility and goes so far as to admit that such cases do happen. However he holds that they pose little threat to science as a social institution. He is able to resolve the apparent difficulty by the assertion of a common aim which all scientists share, regardless of other differences: the goal of effective communication.

[Scientists try to avoid talking at cross-purposes .... They try very seriously to speak one and the same language, even if they use different mother tongues. In the natural sciences this is achieved by recognizing experience as the impartial arbiter of their controversies. ... In order to avoid speaking at cross-purposes, scientists try to express their theories in such a form that they can be tested, i.e., refuted (or else corroborated) by such experience.]

This quite clearly indicates that as a methodological tool the scientist decides always to give primacy in arguments to the testimony of experience.
But this response only temporarily delays the real question at issue; for we can now ask why it is that the scientist places such high premium on the continuance of discussion?

The only plausible reply to this query is that all scientists, qua scientists, share a common goal and the continued critical discussion is a means of achieving it. The goal postulated by Popper is, of course, interesting truth, or verisimilitude. Only so long as there is a sufficient store of unproblematic background knowledge, of more or less universally accepted basic statements, does it remain possible to compare degrees of verisimilitude. Had science no way of deciding which part of a complex and inconsistent set of observation, theory and background ought to be rejected, it would no longer make sense to speak of scientific progress. For Popper, such an admission of the senselessness of the ideal of scientific progress leads straight into nihilism and irrationalism.

iv. The Methodology of the Social Sciences

The social sciences present no novel problems as far as Popper's methodology is concerned, although the consideration of them does cause some shifting of emphasis.

In the first place, it seems as if the goal of the social sciences is the same as the natural sciences'. The social scientist seeks to provide true explanations rich in content. And to do this he profitably uses the method of formulating bold conjectures and rigorously testing them. The one cautionary note Popper adds is that the social scientist must clearly distinguish a scientific prediction from an "unconditional historical prophecy". The difference lies in the fact that a really
scientific prediction is always conditional in form, asserting that if certain initial conditions are fulfilled, then a specified result will follow. An historical prophecy, by contrast, attempts to discover some law of historical development which, regardless of the decisions of individual men, determines the direction of historical change.

A great part of the argument against the tenability of historical prophecy relies on Popper's interesting and original arguments against any kind of deterministic theory. An adequate discussion of these arguments would take us far afield from our present interest. There is however one element in the critique of historical prophecy that is germane. This is the contention that scientific prediction most characteristically deals with small-scale changes in relatively isolated systems. The upshot of this is that social science's legitimate task is that of formulating similar laws governing small changes under given initial conditions. The social scientist ought not concern himself with large-scale social change.

We should also note that Popper sees no need to introduce any qualitatively different argument to the effect that the social sciences are capable of being value free. While it is true that the social researcher is brought up within a particular society, and is imbued with his society's descriptions of itself, this is irrelevant to the ability to produce a social science. As in the natural sciences, the genesis of a description or explanation is irrelevant. All that matters is the manner in which these are tested. The social scientist's decision to accept any particular basic statement, theory or piece of background knowledge will be subjected to precisely the same sort of critical intersubjective testing as is that of the natural scientist. Both branches
of science ensure their value neutrality by virtue of their insistence on intersubjective scrutiny.23

v. Mannheim's Sociology of Science

A fruitful counterpoint to the Weberian value-neutrality thesis and to Popper's modifications of it is found in Karl Mannheim's sociology of knowledge. What is for Mannheim most problematic in Weber's schema is the focus on the individual scientist as the source of spurious evaluations in science. This is problematic because it tends to seriously underestimate the problem of objectivity in social science. Weber's defense is against what Mannheim terms the "particular conception of ideology"; and such a defense is painfully simple because at this level "it is possible to refute lies and eradicate sources of error by referring to accepted criteria of objective validity common to both parties"24 to a dispute.

A far more serious challenge to the very possibility of social science is posed by the "total conception of ideology." This conception "calls into question the opponent's total weltanshauung (including his conceptual apparatus), and attempts to understand these concepts as an outgrowth of the collective life of which one partakes." The total conception forces us to consider how it might be possible to compare "fundamentally divergent thought-systems and ... widely differing modes of experience and interpretation."25

Weber's discussion is thus taken to be misleading because he assumes that disagreements in science can be exhaustively categorized as undecidable because data is lacking or because one of the parties has
made a mistake. His focus on the individual as the locus of mistakes contributes to this. As long as disputes are categorized as being between an individual and the group to which he belongs, there is some good reason to believe that the former is at fault, that he has violated an accepted rule.

Mannheim shifts the focus of discussion and thereby shows a far more difficult problem. On his view, the social sciences are distinguished by the fact that the descriptions and explanations of events are largely determined by the social class of the observer. One's entire view of the social world depends on the class to which one belongs, and so the student of society comes to his study with a set of preconceptions of the social world. Students from differing class backgrounds will have different preconceptions of the social world. The conflict now becomes one, not between an individual's idiosyncratic notions and those of his community, but between the articulated views of different communities. And it is no longer clear that these communities share a common set of rules for the mediation of disputes.

We need to emphasize here that Mannheim does not advance the total conception of ideology as an insuperable barrier to social science. His is not the conclusion that social science is impossible. He shares with Weber (and Popper) the commitment to the normative ideal of a science that is acknowledged correct even by a Chinese. Where he differs is on the questions of how, and how easily, this is to be achieved.

In setting his problem Mannheim hit upon a deep and fundamental issue in the philosophy of science. As we will presently see, the relativism made possible by the total conception of ideology continues to plague contemporary methodologies of science. Unfortunately, Mannheim's
solution falls far short of the mark.

The solution he offers consists essentially of a backing away from the problem. Having argued that any theory of society corresponds to the social class of the theorist and having implied that the multitude of theories are in conflict with one another, Mannheim avoids the issue of relativism by claiming that each of the theories represents one perspective from which the social reality can be viewed. Just as is the case with three-dimensional visual perception, no single viewpoint allows a complete perception of the object nor are the perceptions from different vantage points contradictory, so in political thought, no one theory captures the entire social system nor are the different theories contradictory. Mannheim opposes relativism with his analogical thesis of relationalism:

The controversy concerning visually perceived objects (which, in the nature of the case, can be viewed only in perspective) is not settled by setting up a non-perspectivist view (which is impossible). It is settled rather by understanding, in the light of one's own positionally determined vision, why the object appeared differently to one in a different position. Likewise, in our field also, objectivity is brought about by the translation of one perspective into the terms of another.27

The relationship between Mannheim's views and Popper's is a tangled one; but the effort spent in untangling it will pay some handsome dividends.

Both writers chide Weber for his contention that the problem of value neutrality arises in the social sciences because of the human frailty of individual researchers and that it is therefore to be resolved by greater care on the individual's part. The real solution to the problem is seen by Mannheim as well as by Popper to lie in the social structure of science—Popper's savage criticism of Mannheim's sociology
of science notwithstanding. The two part company, however, on their
descriptions of how the social structure resolves the problem.

Popper's picture is one of conflict and struggle, mediated by
commonly accepted rules. Regardless of any theory's origins it will
be subjected to severe criticism. The catholicity of the scientific
community, the fact that its members come from all social classes, guar-
antees that no single class-bound perspective can predominate. However,
while this catholicity insures that scientific method will not yield
partisan answers, it cannot simultaneously insure that it will yield
determinate answers. The possibility that the scientific community will
disintegrate along partisan political lines remains open. This possi-
bility is foreclosed in the Popperian system by the adherence of all
scientists to a common scientific ethos: explanations are to be stated
as clearly as possible and in such a way that they can be unambiguously
tested against experience. Whoever violates these rules or refuses to
accept the arbitration of experience is to be banned from the community.

Mannheim's view, by contrast, is one of synthesis and reconciliation.
He portrays the theories of society offered by different social classes
not as contradictory, but merely as different. The diversity of theories
corresponds to a diversity of equally valid perspectives from which
society is viewed. He writes:

It has become uncontrovertibly clear today that all knowledge
which is either political or which involves a worldview, is
inevitably partisan. The fragmentary character of all knowledge
is clearly recognizable. But this implies the possibility of
an integration of many mutually complementary points of view into
a comprehensive whole.

Just because today we are in a position to see with increasing
clarity that mutually opposing views and theories are not infinite
in number and are not products of arbitrary will but are mutually
complementary and derive from specific social situations, politics
as a science is for the first time possible.
On first reading Mannheim's thesis seems surprisingly naive. It is not, after all, obvious that all of the class-bound, partisan political philosophies are complementary perspectives which can be blended to produce a complete and consistent picture. Some of the theories he describes do seem to be genuinely contradictory. In recognizing this Popper's description does appear to be more realistic.

Mannheim's sanguine opinion may be dictated by a number of important factors. With Popper and Weber he shares an abhorrence of relativism: the situation in which there is a plethora of social theories is to be avoided if at all possible. He also shares their belief that relativism is to be avoided through the creation of a special social community. (I take it here that Mannheim's "socially unattached intelligentsia" and Popper's scientific community are identical for all practical purposes.) What then becomes a real problem for Mannheim is how this community is to maintain the social cohesion requisite for its functioning, but without becoming one more social class.

If the unattached intelligentsia is taken to be a cohesive social class, then the problem is that of the reflexivity of the sociology of knowledge. The problem is that its comprehensive theory is open to the charge of being but one more perspective. Its perspective will correspond to its own interests as much as that of any other group will correspond to that group's interests. And again relativism threatens. 30

Were Mannheim to accept Popper's view that the various theories offered are irreconcilably contradictory, while maintaining that the unattached intelligentsia is not a new class with its own interests, then there would be no reason he could cite for the group to stay together long enough to reach a consensus. In the face of conflict it
would splinter into partisan cliques. This point can be made clearer if we return for a moment to one of Popper's uses of his method.

It is crystal-clear in the original formulations of his methodology that Popper intended it to be exclusively a theory of scientific method. As a methodology critical rationalism is designed to capture the essential elements of that enterprise whose peculiar goal is the attainment of interesting truth: truth about nature as well as truth about society. In later writings, though, Popper treats his system not only as a methodology of rational theory choice, but as a much more powerful methodology of rational action. Thus he writes:

Assume that we have deliberately made it our task to live in this unknown world of ours, to adjust ourselves to it as well as we can; to take advantage of the opportunities we can find in it; and to explain it if possible, ... and as far as possible, with the help of laws and explanatory theories. If we have made this our task, then there is no more rational procedure than the method of trial and error - of conjecture and refutation.31

The task for which his method is the most rational is none other than living in the world.

This tendency to consider his a general theory of rationality is most evident in Popper's writings on social policy and social change. He tackles issues of social organization by treating social structures and scientific institutions as similar contrivances for solving problems. In each case we begin with a problem (practical and theoretical respectively) and a traditionally accepted background (of institutions and knowledge). And in either case we are forced to reject some part of a complex system, with the choice of which part left open. The one solution Popper will not countenance in either case is the revolutionary rejection of the entire background. Such a radical solution would lead to "the breakdown of critical debate" and the disappearance of civilization.32
The extension of the method of falsification to the political decision-making arena seems to be a rather simple-minded violation of Weber's injunction against confusing social science and policy making. Popper apparently overlooks the fact that science presupposes an agreement over goals, while in political action it is quite often precisely the goals that are in question.

Popper, it seems, is forced to recommend his critical rationalism as a method for solving political problems, for want of any acceptable alternative. In a passage that could have been copied from Weber he writes that "no decision about aims can be established by purely rational or scientific means." But he continues to aver that proponents of conflicting aims ought to discuss them critically because the only alternative is violence.

However for the most part, Popper overlooks the consideration that if the goals pursued by different groups are diametrically opposed and dearly held, it may well be that violence becomes a viable alternative. Unless there is some community of values, there is no reason for the alternative of argument to be pursued.

The dilemma here is one which, on Mannheim's view, every member of the intelligentsia who takes up social science must face. He may either attach himself to one of the antagonistic social classes and pursue partisan goals with all of the means available to a party; or else undertake the intellectual "mission" of the intelligentsia to attempt a synthesis of the contending positions. If it appears that a synthesis is impossible, then there would be no reason to take the latter tack.

This unsettled issue between Mannheim and Popper continues to pervade
discussions of social science methodology; and it is an issue with which we must deal in the following chapters if we are to reach any satisfactory understanding of the social control of science. The immediately succeeding chapters examine Popper's claim that the shared pursuit of knowledge is enough to maintain the cohesion of the scientific community, while later chapters return to the question of whether this community should be considered to be but one more social class with its own vested interests.
Notes to Chapter I.


2 Ibid., pp. 32-33.

3 "Objectivity' in Social Science and Social Policy" in The Methodology of the Social Sciences, p. 58.

4 Ibid., p. 55.

5 Ibid., p. 53.

6 Perhaps the most common example of this occurs when the scientist makes a claim on the basis of insufficient evidence.

7 "Objectivity," pp. 54-55.

8 Ibid., p. 84.

9 Ibid., p. 110.


12 In dealing with Popper's philosophy it seems best to start with a relatively brief and simple account which can then be clarified and modified by, first, studying its internal development, and then by scrutinizing the changes in it wrought by outside criticism. The later sections of the present chapter will bring to light some themes present but not emphasized in his writings, while the following chapter will look into some of his critics' works.

Ibid., p. 41.


See Ibid., p. 58-60.

"Truth, Rationality and Growth", p. 239.

Ibid., p. 238.


Ibid., p. 218.

See "Two Faces of Common Sense", pp. 54-58, for a discussion of this goal.

The overly zealous reader of footnotes will no doubt have noticed that the descriptions of this process quoted in sec. iii, above, were taken from Popper's discussion of the social sciences.


Ibid.

As is widely known, Mannheim restricts his sociology of knowledge to the social sciences. He explicitly claims that mathematics and the physical sciences fall outside of its purview.

Ibid., pp. 270-271.
28 See Open Society, Ch. 23.

29 Ideology and Utopia, p. 132.

30 Another possibility is to admit that social scientists form a unique social class and that their perspective is a privileged one. This possibility has been rejected by innumerable commentators as self-defeating.


33 "Utopia and Violence", in Conjectures and Refutations, p. 359.
CHAPTER II

The preceding chapter has provided a rough outline of the relevant aspects of Popper's methodology. The question we must now raise is whether this is an adequate methodology of science. This issue of justification is a perplexing one, as we shall presently see; but Popper's comments on the genesis of his philosophy offer an apparent foothold from which a tentative assessment can begin. He claims to have discovered his demarcation criterion by scrutinizing the difference between clearly successful theories and unsuccessful, pseudoscientific ones. This portrays his methodology as a descriptive undertaking. The most obvious means of critically judging it is therefore to see if it does accurately describe scientific practice by looking either to the history of science or to its current practice. A number of eminent historians have made such comparisons of theory and practice, and it is to their work that we now turn.

1. The Kuhnian Paradigm

The first alternative to Popperian methodology is that which develops out of the research of Thomas S. Kuhn. Kuhn's writings serve as an ideal source for comparison because he is explicitly interested in providing a methodology that is adequate to the task of describing science. His aim is to provide an insight into how the social institution of science functions.

Kuhn's historical research into scientific practice, while showing
many similarities to the Popperian view, comes to a radically different
global picture of science. The principal difference is that for Kuhn
science is usually a much more prosaic affair. Where Popper portrays
science as a "revolution in permanence", with scientists constantly
trying to subvert whatever theory currently reigns—and frequently suc-
ceeding—Kuhn sees a more stable society, with scientists willing to work
within the theoretical system of the day in order to develop it while
overlooking some of its clear flaws. For Kuhn, the stability of scien-
tific development is only infrequently disturbed by changes of revolu-
tional proportion. Popperian methodology errs in overestimating the
importance of major breakthroughs. Such sudden advances do play a part
in the progress of science; but to focus on them as the model for all
scientific activity is to neglect or devalue the normal work of the great
majority of researchers. More critically, this narrow focus on revolutions
blurs the recognition that everyday, nonrevolutionary inquiry must be
undertaken if revolutionary episodes are to occur and their advances to
be consolidated.

In approaching Kuhn’s description it will prove helpful to break
his findings roughly into two parts, one concerned with the everyday
workings of the scientific institution—including the training of new
scientists—and the second concerned with science in revolution.

The central concept in Kuhn’s methodology, and a lynchpin in his
dispute with Popper, is that of ‘normal science’. Any careful scrutiny
of the research enterprise, says Kuhn, reveals that most of its work is
not aimed at discovering the weaknesses of currently held theories. What
one finds instead is that the greatest portion of activity is devoted to
developing these theories: to extending the scope of their application,
and to the refining of them by way of more precise interpretation of key theoretical terms and more accurate determination of experimental findings.

One of the striking features of science, in contrast with philosophy, for example, is that it is more or less free of controversy over the meaning and proper application of terms. The rejection of any foundation of unassailable observation reports couched in a neutral language makes this feature problematic. Kuhn and Popper reject any notion of the givenness of experience. The world is not open to experience as categorically labelled; rather, all experience in order to be reported must be interpreted in the terms of a language. Any theory must therefore contain a categorical structure in the terms of which all relevant experience is to be interpreted. And this language—the vehicle carrying observation reports—is of human construction. Observation reports being humanly interpreted and theory-dependent, it seems likely that controversy over meaning should arise frequently. However, this expectation is not fulfilled in normal science. Meaning disputes are unexpectedly absent there. 2

Popper's theory of science does not have an adequate means of dealing with this problem. The core of his 'solution' is the assumption that scientists will arrive at a consensus on how experience is to be properly interpreted. In essence, he does not recognize a real problem here.

Kuhn looks for a sociological solution to this problem of communication and finds one in the system for educating scientists. Part of the scientific education involves learning the language of a particular theoretical system. And so it is the common education which smooths over the apparent problem of meaning by providing scientists with a common language in which free and easy communication is possible.

The centerpiece of the education is the exemplar. The scientist is
typically taught using standard examples of successful solutions to routine problems. Science textbooks are composed in large part of examples worked out in greater or lesser detail, demonstrating how solutions are arrived at. These are followed by exercises which challenge the student to solve similar problems. Much training in experimental technique similarly involves the student in performing exemplary procedures. Here the important matter is not the outcome of the experiment, but how the student arrives at it. The aim is not to uncover novel truths, but to develop correct technique. Learning to understand the exemplars used in teaching is in large part learning to use the standard scientific language in the standard way. A particular way of looking at the world is thus covertly inculcated during the process of training. And so we should expect that most scientists will agree on the interpretation of nature, in view of their common induction into science.

The manner of instruction is important for reasons other than that it assures easy communication. In the first place, it serves as a filter on the problems that the scientist addresses. Kuhn notes that scientists do not choose topics of study randomly, rather it is a particular kind of problem that piques their curiosity. "[O]ne of the things a scientific community acquires with a paradigm is a criterion for choosing problems that, while the paradigm is taken for granted, can be guaranteed to have solutions." Scientific investigation seeks to solve puzzles where "to classify as a puzzle, a problem must be characterized by more than an assured solution. There must also be rules that limit both the nature of acceptable solutions and the steps by which they are obtained." A common education based on exemplary solutions to puzzles
serves to insure that scientists will recognize as interesting and solvable a common set of problems. Furthermore, it ensures that all scientists will accept common rules for proper solutions to puzzles. In this way education sets out a criterion for good science: it is that work (either theoretical or experimental) which finds commonly acceptable solutions to recognized puzzles according to the standard rules of puzzle solving.

The education of the scientist, of course, is not the sole focus of Kuhn's interest. His description of it serves as a backdrop for his study of scientific method in general. Education explains how the individual comes to participate in normal science, but it does not go far in explaining the nature of normal science.

Kuhn makes it explicit that the great bulk of research fits into the category of normal science.

Mopping-up operations are what engage most scientists throughout their careers. They constitute what I am here calling normal science. Closely examined, whether historically or in the contemporary laboratory, that enterprise seems an attempt to force nature into the preformed and relatively inflexible box that the paradigm supplies. No part of the aim of normal science is to call forth new sorts of phenomena; indeed those that will not fit the box are often not seen at all. Nor do scientists normally aim to invent new theories, and are often intolerant of those invented by others. Instead, normal scientific research is directed to the articulation of those phenomena and theories that the paradigm already supplies.4

The structure of the scientific community is oriented toward the production of normal science. Individuals are taught to experience the world one way—they learn the conceptual framework of one theory—and they are taught to do science in a particular way.
ii. The Popperian Reaction

The brief characterization of the operation of normal science contains at least three elements antithetical to the Popperian. The first is the claim that normal scientists may simply overlook those phenomena that do not conform to the expectations generated by the accepted theory. The second is that those engaged in normal science need not actively search for novel, previously unexpected phenomena. And the last is the suggestion that scientists seek to make the data fit the theory, rather than making the theory fit the data. From the Popperian perspective all three of these features have the flavor of dogmatic commitment instead of free inquiry.

We saw in the last chapter that for Popper the fact that an experimental prediction is falsified creates an immediate problem for the scientist. The failure of a prediction indicates that there is a contradiction somewhere in the conjunction of background knowledge, theory under test, and experimental result—a contradiction which must be eliminated forthwith. To suggest, as Kuhn does, that this contradiction can simply be overlooked represents an unconscionably cavalier attitude toward the importance of observational testing. According to the Popperian ethos the scientist who is faced with such a situation ought to attempt the formulation of a new theory which eliminates the contradiction and predicts novel observations as well. The failure to do this indicates either a lamentable lack of creative thinking or a dogmatic attitude toward an accepted theory. The Kuhnian ethos seems excessively lax in allowing the researcher to treat the putative falsifier either as an interesting anomaly to be dealt with at some unspecified time or as a serious threat to his theory demanding immediate attention.
The suggestion that in normal science one need not be concerned to discover novel facts leaves the door open to the unrestrained *ad hoc* modification of otherwise falsified theories. Popper recognized that a theory could always be protected from falsification in the face of adverse findings by adding extra hypotheses or reinterpreting theoretical terms. He also recognized that such a strategy is not necessarily obscurantist. To allow the strategy while guarding against its abuse, he argued that the only allowable auxiliary hypotheses are those which increase the content of the original theory; i.e., those that predict novel, and falsifiable, phenomena. Kuhn's claim that normal science is unconcerned with novelty raises the suspicion that the scientist is being given license to entertain any *ad hoc* hypothesis or linguistic argument in trying to develop or immunize his chosen theory.

Popper's reaction to the picture presented by Kuhn is to grant that it may be an accurate portrayal of much scientific practice, but then to denigrate this part of the practice. Thus he writes:

*I shall therefore state again that what Kuhn has described does exist, and that it must be taken into account by historians of science. That it is a phenomenon which I dislike (because I regard it as a danger to science) while he apparently does not dislike it (because he regards it as 'normal') is another question; admittedly, a very important one.*

In my view the 'normal' scientist, as Kuhn describes him, is a person one ought to feel sorry for. ... The 'normal' scientist, in my view, has been taught badly. ... He has been taught in a dogmatic spirit; he is a victim of indoctrination.

Clearly, then, his strategy for dealing with Kuhnian objections is to stress the *normative* character of his own methodology. Some scientists do proceed as Kuhn describes, but they are the second-class citizens in the research community (the 'applied scientists') whose findings are of
relatively little worth. The truly important advances are made by those who try to break out of the framework of accepted theories. Progress toward truth comes only through revolutions in conceptual and theoretical frameworks. At the bottom of Popper's distrust of Kuhnian methodology is the suspicion that normal science is antithetical to revolutionary changes which are the lifeblood of scientific progress.  

iii. The Kuhnian Response

Such a response to Kuhn seriously misinterprets his methodology. On his view the practice of normal science is absolutely necessary to the progress of knowledge, both in its everyday incremental increase and in its unusual revolutionary breakthroughs. As we noted earlier, in everyday puzzle solving normal science provides the inquirer with the models of success he needs as patterns for his own work. It thus allows him to answer many questions of detail without becoming bogged down in questions about proper technique. These puzzles are important, but to treat them as the epitome of scientific inquiry is to overlook the import of revolutionary change. As Kuhn puts it, "the central episodes in scientific advance—those which make the game worth playing and the play worth studying—are revolutions." What then becomes critical for his methodology is to see how normal science's practice is related to the occurrence of revolutionary episodes.

Scientific revolutions, according to Kuhn, "always involve the replacement of one method of doing normal science with a different method. It is normal science that is the object of change. Progress is achieved by overthrowing the accepted paradigm and offering another in its stead.

There is something to Popper's suspicion that normal science is
counterrevolutionary. We should expect that the scientific community as a whole does not accommodate itself easily to revolutionary change. According to Kuhn, this is precisely what we do find. Individual scientists tend to resist the introduction of a novel theory partly because a change to a new way of doing science can be made only with difficulty, and partly because the new theory is not likely originally to be as successful as the old. Revolutions are therefore often completed only when the next generation of scientists has received its training inside of the new disciplinary matrix.

The tasks of methodology then become those of determining under what conditions revolutionary changes can succeed against this background of conservative inertia, and of setting those conditions under which such change should take place.

Kuhn argues that rejection of one theory should not follow immediately upon the discovery of a problem and, indeed, that in the mature sciences rejection is not automatic. His argument against simple falsification is founded on purely strategic grounds.

All theories, he argues, are confronted with numerous anomalous results and contain unclarities, possible contradictions and so forth. Were all scientists to spend their time trying to develop alternative theories devoid of these problems, the progressive character of science would in all probability disappear. Kuhn suggests that many fields of inquiry not classified as scientific, and not exhibiting progress, suffer from just such a superfluity of theoretical systems. This also seems to be the case in many of the social sciences. The temporary unconcern with problems in the sciences allows the inquirers to develop the theory conceptually and to experimentally test it in detail. In
this way they are able to determine which of the problem areas are truly severe and which merely anomalous. This, in its turn, shows how really tough tests of the theory can be developed. A theory becomes ripe for replacement if there are severe problems which repeated attempts made from within its perspective cannot solve. From this arises a criterion for the judgment of suggested replacement theories. An alternative theory will be seriously considered only if it proposes a solution to those problems untouched by its predecessor.

Kuhn's liberalized rules for the revolutionary replacement of theories thus allow scientists a time for deciding which unsolved problems are most critical, and so to set out conditions any new theory must fulfill. This serves to focus scientific investigation in a way that Popperian falsification in its original form did not.

Kuhn's methodology also portrays the scientific community as much more diversified than does Popper's. For the latter, all scientists should be revolutionaries, constantly criticizing accepted views and trying to replace them with something better. Kuhn admits that some scientists are true revolutionaries, but many more continue to work within the system. Some try to remedy particular ills (while ignoring other pressing ills), often with great success; while other scientists try to get even clearer about the theory's insurmountable problems, thereby pointing the way toward a new vision of the world. The revolutionary clearly needs the second group to prepare the way for his breakthrough—to set the large problem he solves. But he is no less dependent on many researchers of the first sort. They are the ones who will consolidate the gains brought about by the revolution. They will set about modifying and clarifying the new theory to handle objections it
will undoubtedly face. Unless one is willing to demand that the great revolutionary be able to perform all of these tasks (as well as others, such as designing experiments which severely test his theory), then on Kuhn's view it is necessary to recognize not only the existence, but also the crucial importance of these nonrevolutionary members of the scientific community.

One illuminating way of looking at Kuhn's sociology of science is suggested by Popper's own writings on social science and social policy. A stable fixture of Popper's attack on historicism and utopian social planning is the thesis that in the social world human action often has unforeseen consequences. Prophecy and utopian revolutions are to be avoided because of this. He asserts that "the main task of the theoretical social sciences ... is to trace the unintended social repercussions of intentional human actions." And these sciences have the practical role "of helping us to understand even the more remote consequences of possible actions, and thus of helping us to choose our actions more wisely."  

Kuhn's sociological and social psychological studies of science might now be viewed as part of this theoretical social science. What he is doing is demonstrating that a strict adherence by all scientists to the Popperian model of scientific rationality would have the unintended consequence of freezing out progress. The practical offshoot of this is to suggest a different sort of social structure for science, one which countenances and even encourages many members of the community to engage in activity that Popper must classify as unscientific.

In defending his original position against the charge that it countenances unscientific stratagems Kuhn tries to spell out the difference
between his views and those of others in terms of the distinction between rules and values. Rules, such as the rules of logic, have the character that if two parties disagree over the proper conclusion of an argument, then the disagreement must be caused either by the premises or by one party's misapplication of the rule. In general, the correct application of rules is stipulated in advance, so in the event of the latter type of disagreement it is clear which party has made the mistake. The failure to admit this mistake and then to correct it is ground for the charge that one is being unscientific. Rules such as these are what is sought by Popper and other mainstream philosophers in their quest for an algorithm for rational scientific theory choice.

Kuhn's alternative to the prevailing view is that the reasons invoked to justify the embracing of a theory are better characterized as values. To be sure, aspects like accuracy in prediction, simplicity, wide scope are all important elements in theoretical systems. And all are elements which scientists learn to regard highly during their education. Treating these as values is crucial for two reasons. In the first place, it allows for the possibility of a value conflict. Simplicity and predictive accuracy may well yield different assessments of competitor theories: theory_1 may be simpler but less accurate than theory_2. A rule calling for the choice of the more accurate theory (e.g., Popper's falsification) would always force this choice. Kuhn's alternative gives the individual scientists a greater flexibility in the decision. Some individuals will prefer the simpler theory and embrace it, while their colleagues choose that which gives greater accuracy. This flexibility, due to the lack of a preference ordering among values, is necessary to the progress of science, as it allows the deve-
lopment of theories in a manner that a strict set of rules would not. Kuhn claims that history demonstrates this leeway for development of theories to be of crucial importance.

The second aspect of values is that there is usually a wider range for decision about just how they apply in particular situations. This also allows for greater variability among individual scientists in the choice of theories.

The importance of this talk about values can again be explicated by turning to social policy issues. Just as the shared values in a political society allow individuals a greater leeway of action than do strict laws, so in the epistemic community scientists can disagree on particular assessments without shredding the fabric of consensus upon which the Popperian scientific community is based. To replace these values with stricter rules of rationality would force many more scientific disputes to be settled with the charges of unscientific procedure and expulsions from the community.

iv. The Kuhnian Revolution

The differences between Kuhn and Popper outlined to date, being of a mainly strategic nature, are perhaps reconcilable. Imre Lakatos, in fact, attempts to mediate such a reconciliation. (See the following chapter.) That there is a far wider and apparently unbridgeable rift becomes clear once we turn to Kuhn’s notion of scientific progress.

Popper, of course, insists that progress is to be spelled out in terms of getting nearer to the truth; and the intuitive idea of proximity to truth is given a measure of rigor in his definition of verifiability. It should be clear how the values listed above help science
progress in this direction. A theory with wider scope, for example, will have greater content. And if this increased content remains unfalsified, it will naturally have greater verisimilitude. Popper thus justifies these scientific values by stipulating a goal for science (truth) and endeavouring to show how they contribute to the attainment of—or approach toward—this goal. Progress is clearly defined as an ever nearer approach to truth.

The Popperian account of progress seems to capture well what happens in normal science. There most advances involve incremental increases in knowledge; the theory is extended into a virgin domain, or more accurate experimental results are obtained through refined techniques. Radical revolutionary change, however, cannot be accounted for on this model.

At this point one of the principal terminological confusions in the Popper–Kuhn debate must be eliminated. Both philosophers expend much energy talking about scientific revolutions as central, indeed crucial, elements in the progressive development of science. This surface agreement merely serves to disguise a very fundamental controversy. Popper and Kuhn each mean different things when they write of revolutions.

Popper, despite his rhetoric about the scientific "revolution in permanence", and so forth, has a relatively impoverished concept of revolutions. Unfortunately, this does not become completely clear until very late in his writings. In his response to Kuhn, Popper writes:

even a minor discovery (it may be made by an animal) is revolutionary. I mean that many engineers and technologists are minor and major revolutionaries. I mean, more precisely, that established beliefs (or routines) are overturned every day. Sometimes these are major discoveries: more often they are very minor discoveries. The heating engineer who faces the problem of how to install a central heating system required to
work under unusual conditions may just apply his established rule of thumb, and thus fail to solve the problem: in the face of this failure he may depart from his routine and ... arrive at a critical solution of his problem. 10

Between the heating engineer and the Einstein there is just a matter of degrees: both are Popperian revolutionaries.

Kuhn is far more radical (and clearer) in explaining his concept of revolutionary change. For him a revolution is a paradigm shift from one method of doing normal science to another. A comparison of the pre- and post-revolutionary science reveals a number of drastic changes. The exemplars that are used to demonstrate proper technique or method are altered, and with them the educational process. The choice of problems (guided as it is by the methods available) will also most probably be changed. Some problems that once important in the pre-revolutionary era will no longer be considered so, either because the new method solves them or because they can no longer be considered as puzzles with assured solutions. Finally the observation language used to describe the results of experiments will change--sometimes subtly, sometimes more drastically.

Kuhnian revolutions are thus severely shocking events. To the individual scientist forced to choose between an old theory and a revolutionary new one the issue may be overwhelming. His choice is between familiar ways of looking at the world and of operating in it, ways learned during his formation as a scientist, and radical new ones. Given the fact that this new theory in its formative stages will be unable to do as much as its rival can, the individual's shift to the new is made more difficult. But on Kuhn's portrayal, this individual can either stick to the old or shift to the new and neither decision can be ruled irrational. More importantly, the decision to stick to the old
will not stop scientific progress.

One of the cornerstones of Kuhnian methodology is the thesis that the disciplinary matrix provides criteria for judging proffered puzzle solutions. Since revolutions are shifts in disciplinary matrices, they can involve changes in these criteria, alterations in the specification of satisfactory scientific research. It should be clear, then, that the comparison of theories by opponents and by proponents of a revolutionary change may well reach opposite conclusions. This problem is exacerbated by the difficulty in communications the opposed groups experience. Kuhn writes that many philosophers of science assume that theories can be compared by recourse to a basic vocabulary consisting entirely of words which are attached to nature in ways that are unproblematic and, to the extent necessary, independent of theory. That is the vocabulary in which Popper's basic statements are framed. He requires it in order to compare the verisimilitude of alternate theories. ... I have argued at length that no such vocabulary is available. In the transition from one theory to the next words change their meanings or conditions of applicability in subtle ways.11

Because of these meaning shifts the proponents of the alternative theories may on extraordinary occasions be unable to agree on the results of experiments. Kuhn's contention here is neither that communication between the two groups is impossible nor that it is merely difficult. Rather, he wishes to say that the change from one theory to another, from one conceptual framework to another, is like a typical Gestalt switch by virtue of which the world is experienced in a different way. As in the Gestalt switch one can learn to make it. What one cannot do is make the shift part way: one must see one picture or the other, but not parts of both at the same time. Kuhn also suggests that it is not so easy to shift back and forth from theory to theory.
While Popper takes much of Kuhn's normal science to be revolutionary, he also works to undermine the latter's notion of revolutions. Against Kuhn's thesis that across a revolutionary change a critical and persuasive comparison of theories may prove impossible, Popper offers the bromide that "a critical discussion and comparison of various frameworks is always possible. It is just a dogma—a dangerous dogma—that different frameworks are like mutually untranslatable languages." 12

There is good reason why Popper becomes so vehement in his attack on Kuhn's idea that some revolutions may be so wide-ranging as to preclude effective communication between the adversaries. Popper's empiricism ultimately rests on the foundation of basic statements that are agreed to by consensus. An infinite regress is prevented by an essentially practical decision by the participants to accept this set of statements. Now if the theory-laden nature of the observation reports is allowed to disrupt communication to the extent argued by Kuhn, this consensus is imperiled, and the regress again yawns open. Furthermore, the Popperian idea of scientific progress is also inextricably tied to a consensus among scientists. Insofar as the concept of verisimilitude is more than a mere logical notion, it requires the protection of the unanimity in the scientific community. If verisimilitude is to offer practical advice in the choice between conflicting theories, then it will be necessary to be able to compare the truth- and falsity-contents of the respective theories. These measures can be made only if all agree on what statements are true and what false.

This may provide a rationale for Popper's attack on the Kuhnian theory of revolutions, but it surely does not give an argument against Kuhn's thesis. Even if the goal of science is pure truth, it may be
the case that an effective ban on communication between a theory and its competitors is a useful tool in achieving the goal.

Interestingly, the principal argument sketches Popper draws against the possibility of thoroughgoing revolutions are practical ones. Thus in The Logic of Scientific Discovery he writes: "If some day it should no longer be possible for scientific observers to reach agreement about basic statements this ... would amount to a new 'Babel of Tongues': scientific discovery would be reduced to absurdity. In this new Babel, the soaring edifice of science would soon lie in ruins." 13

In a later essay, in defense of the point that much background knowledge must be unquestioned in the comparison of theories he says:

While discussing a problem we always accept ... all kinds of things as unproblematic: they constitute for the time being, and for the discussion of this particular problem, what I call background knowledge. ... almost all of the vast amount of background knowledge which we constantly use in any informal discussion will, for practical reasons, remain unquestioned; and the misguided attempt to question all—that is to say, to start from scratch—can easily lead to the breakdown of a critical debate. 14

We noted in the last chapter that Popper's attempt to extend his method of critical rationalism into the arena of public policy making was made problematic by the fact that the participants in that arena might not find the breakdown of debate unacceptable. In particular, if the antagonists pursue irreconcilably antithetical goals and if they value those goals highly, then they may well wish to challenge one another to a contest of power rather than one of ideas. We also noted Mannheim's concern over the problematic unity of the social scientific community. It remains possible that that community may disintegrate in the face of conflicts over alternative explanatory systems. Now it appears that Kuhn is making an analogous point about theoretical con-
licts in the more mature physical sciences. It remains possible that the proponents of competing explanations will be willing to risk a breakdown of communications if that will help advance their theories.

There are, to be sure, key differences between Mannheim's position and Kuhn's. Most obviously, Mannheim suggests that the principal threat to communications is posed by the nonepistemic commitments of the disputants, while Kuhn's claim is that the community cannot be fractured over purely epistemic concerns. (Mannheim's solution to the problem, in fact, relies on the presupposition that the theories of the conflicting parties are not irreconcilable.) But there are even more important similarities. Chief among these is the element of relativism injected into science.

The element of relativism becomes apparent in Kuhn's account of the notion of scientific progress. It seems that his account must lie completely in the area of descriptive sociology. Indeed, some of his pronouncements on progress deprive the notion of the strong normative force it enjoys in Popper's writings. Thus Kuhn writes:

Why should progress ... be the apparently universal concomitant of scientific revolutions? Once again there is much to be learned by asking what else the result of a revolution could be. Revolutions close with a total victory for one of the two opposing camps. Will that group ever say that the result of its victory has been something less than progress? That would be rather like admitting that they had been wrong and their opponents right. To them, at least, the outcome of revolution must be progress, and they are in an excellent position to make certain that future members of their community will see the past history in the same way.

From this it appears that Kuhn admits that the history of science as it is written by scientists is largely a victors' history. The proponents of the currently reigning theory and methodology not only train pros-
pective new scientists in their techniques, but they also construct revisionist histories displaying a linear development to current beliefs.

The dilemma facing Kuhn here is plain. On the one side he wishes to forthrightly deny an appeal to any criterion, such as the criterion of verisimilitude, which has dubious epistemological status. "We may... have to relinquish the notion, explicit or implicit, that changes of paradigm carry scientists and those who learn from them closer and closer to the truth." This denial undercuts the justification of science employed by Popper in arguing the merits of his normative methodology of science. On the other side the suggested alternative—viz., that we leave it to the scientists to determine what developments are progressive—has the seeming effect of completely surrendering the normative side of methodology to a purely descriptive sociology of science. Lost in this retreat from normative methodology is any serious attempt at rational critique of science as well as all rationale for considering the scientific community to be an especially important object of sociological study.

Kuhn naturally recognizes the danger lurking here, and tries to defuse it. In answer to the question of whether his methodological remarks are to be read as descriptive or normative he writes:

The answer, of course, is that they should be read in both ways at once. If I have a theory of how and why science works, it must necessarily have implications for the way in which scientists should behave if their enterprise is to flourish. The structure of my argument is simple... scientists behave in the following ways: those modes of behavior have (here theory enters) the following essential functions; in the absence of an alternative mode that would serve similar functions, scientists should behave essentially as they do if their concern is to improve scientific knowledge. The simplicity of this answer should not beguile us into over-
looking the very weak sense of normative Kuhn invokes or the strongly relativistic overtones of the answer. Kuhn's methodological prescriptions are the rules of a game. Their status is precisely the same as that of, say, the rules of basketball: if one wishes to play basketball, then he must abide by the same rules as the other participants in the game. Admittedly, the rules say absolutely nothing about why one would (or should) want to play the game. (Or, to return to one of our guiding questions, about why others should pay the players' game fees.) The norms are also relative because they are chosen with an eye toward the improvement of scientific knowledge, and the participants in the game called science are the ones to decide what constitutes scientific knowledge. The judgment of whether a particular development within the field is progressive is left to the participants in that field, while the methodologist is left to conduct a sociological study of how the judgment is reached. The methodologist's final position is thus to say "this is what scientists count as progress and these are the methods they use to achieve it; if you wish to achieve it as well, imitate their rules."

v. Summary

The contrasts between Popper and Kuhn are stark. Some of them may well yield to the development of a third option, but others seem to defy such treatment. The fact that such strong disagreement arises between them is made all the more striking by the large common ground they share.

Chief among their common commitments is their mutual desire to oppose to the tradition in the philosophy of science that preceded them
a new methodology that would more accurately depict real scientific research, particularly as it is to be distinguished from other endeavors, and that would still be normative. Their agreement on this aim, however, is disrupted by the different emphasis in each man's project.

Popper's interest becomes clearly focused on the normative aspect of methodology. His early search for a demarcation criterion is explicitly and obviously motivated by a desire to be able to unmask some projects which parade as science. The actual workings of science hold his attention only because he wishes to distill the essence of proper method from them.

The Popperian project is strongly normative in that it offers a reason for preferring the sciences over the pseudo-sciences. Science is held up as an ideal to be studied and emulated because it is successful in its pursuit of a goal which is good: the truth. Of course, Popper admits that we have no criterion of truth, so we can never be certain that science has captured its prize. But we can rest assured that we can accurately judge our progress toward this goal.

This goal and the criteria for judging progress are combined to yield judgments on actual science. Those moves which decrease the verisimilitude of theories are retrogressive and hence unscientific.

Thus Popper's methodology is primarily normative, and strongly normative—postulating the goal of truth. He also takes it to offer practical advice to the scientist. While he often claims that it is also descriptively accurate, this aspect is underplayed in his later writings.

Kuhn nearly inverts this order. His primary interest is in an accurate description of science as it has been and is practiced.
of this descriptive study arises a normative methodology, but it is weakly normative.

Kuhn does not postulate any explicit goal of scientific inquiry. If science does in fact have a goal, then it is to be discovered by the methods of sociological study of what scientists themselves say and do. The normative methodology that is created is thus not to be vindicated by showing that following the method does lead to a worthwhile end. Instead, the vindication is much weaker. The methodology is to be followed because that is what scientists do when they are being scientists. Likewise the practical pronouncements the Kuhnian is liable to make are weak and hypothetical in form: If you wish to be a member of the scientific community (or of a particular one of its sub-communities) then act in this way.

The obvious next step in the evolution of methodologies would be to devise one which more evenly balances the normative and descriptive: a methodology which is both strongly normative and descriptively adequate: This is the task Imre Lakatos sets himself.
Notes to Chapter II:

1 For a partial catalogue of the similarities see Thomas Kuhn, "Logic of Discovery or Psychology of Research?" in I. Lakatos and A. Musgrave, eds., Criticism and the Growth of Knowledge, pp. 1-2.

2 We should note that these remarks refer to the mature natural sciences. Kuhn notes that meaning disputes are to be found in the contemporary social sciences. He seems to treat this as a sign of their immaturity.


5 See The Logic of Scientific Discovery, sec. 20.

6 "Normal Science and Its Dangers" in Criticism, pp. 52-53.

7 The second prong of Popper's counterattack consists of a partial denial of Kuhn's description of science. After admitting that normal science does exist Popper goes on to say that "Kuhn is mistaken when he suggests that what he calls 'normal' science is normal." (Ibid.; cf. "Reply to Critics" in P. A. Schilpp, ed., The Philosophy of Karl Popper, v. 2, pp. 1144-48.) This second prong, however, remains unexploited for he does not undertake the detailed scrutiny of episodes in science necessary to substantiate his charge.

8 "Reflections on My Critics" in Criticism, p. 241.


10 "Reply to Critics," v. 2, p. 1147.

11 "Reflections on My Critics," p. 266.


13 p. 104.

15 *The Structure of Scientific Revolutions*, p. 166.

16 Ibid., p. 170.

CHAPTER III

The writings of Imre Lakatos are interesting insofar as they incorporate the sociologically sophisticated and historically more accurate account of science characteristic of the Kuhnian methodology into a system that maintains the strongly normative aspects of the Popperian. The problem from which he begins is that of producing a methodology that adequately depicts the real practice of science, but one that is normatively grounded such that it could rule some parts of this practice irrational and pseudoscientific.

Lakatos clearly sees himself as a defender of the Popperian position. Nonetheless, his opening salvo is fired from the Kuhnian camp. The key insight of Kuhn's methodology is that the falsification of a theory is not automatically followed by its rejection or replacement. On this issue Lakatos takes his first stand against Popper. In actual practice theories are not rejected upon the appearance of a putative falsifier; moreover, this does not indicate that the practice is unscientific. Scientists do hang on to theories in the face of refuting evidence, but this is more than a mere sociological fact to be explained away as a typically human failure to achieve an ideal—in this case, the ideal of Popperian rationality. Lakatos accepts that this gap between falsification and rejection, as well as some of the strategies deployed in the face of a putative refutation, are essential to the continued progress of first-rate science.

While he rejects Popper's assertion that the gambits Kuhn outlines are unscientific, Lakatos raises the more serious charge that Kuhn's
own account of these gambits and their use threatens to make scientific change irrational. His critique of Kuhn is composed of two central parts. At the descriptive level he says that Kuhn's account is simply wrong. Science is not characterized by periods of calm in which one paradigm predominates, and brief revolutionary periods during which paradigms compete. Instead, Lakatos sees science as a continual struggle between competing paradigms. The second, and more crucial, criticism is that Kuhn seems to locate the entire issue of revolutionary change at this descriptive level. On Lakatos's reckoning, the consistent reliance on psychological and sociological explanations deprives Kuhn of any logic of scientific progress. Whatever the members of the scientific community accept as progressive, the Kuhnian must accept as well. Thus Lakatos charges, "In Kuhn's view there can be no logic, but only psychology of discovery." ¹

The aspect of the appeal to descriptive, sociological accounts which is most bothersome to Lakatos is that it relativizes the rules for acceptance of explanations in science. The decision to accept or reject a proffered explanation is made according to the rules found within a particular paradigm, but then there is no rational account for the change from one paradigm to its successor. This is the source of Lakatos's strident charge that Kuhn makes revolutionary scientific progress irrational:

[A] new 'paradigm' emerges, incommensurable with its predecessor. There are no rational standards for their comparison. Each paradigm contains its own standards. The crisis sweeps away not only the old theories and rules but also the standards which made us respect them. The new paradigm brings a totally new rationality. There are no super-paradigmatic standards. The change is a bandwagon effect. Thus in Kuhn's view scientific revolution is irrational, a matter for mob psychology. ²
The obvious tonic for this deficiency would be 'super-paradigmatic standards'; standards which can be justified in terms of science's unchanging epistemic goals.

A related weakness Lakatos points out is that Kuhn's eschewal of strong epistemic norms cannot be carried through consistently. He argues that before a sociology of science can begin, the sociologist must be able to locate his subject: sociology of science requires some demarcation criterion in order to isolate the scientific community. This implies that the sociologist has at least a vague notion of what a science should be like. Now, asks Lakatos, if this demarcation criterion can be used to judge which segments of society form scientific communities, why can it not also be used to decide when a particular revolution has carried a group outside of science?1

Lakatos hopes to supply a theory of scientific rationality capable of telling us when the rejection of a theory should occur, when it becomes irrational to hold on to a theory despite its refutations. Similarly, he hopes to provide the guidelines that determine when the various defensive ploys and immunizing strategies discovered by Kuhn can be reasonably utilized.

i. The Methodology of Scientific Research Programmes

The core of Lakatos's methodology is his reconstruction of the Kuhnian depiction of science. The goal of this reformulation is to replace as many as possible of Kuhn's sociological explanations of scientific change with new explanations showing these changes as instances of obeying epistemically normative rules.
Lakatos's first alteration is to replace the vague notion of a paradigm with that of a research programme. Revolutions in science are not contested by proponents of alternative theories, but by proponents of alternative research programmes. A research programme is in essence a continuous series of individual theories. As the first theory runs into difficulties (as it inevitably will) it will be replaced by a second one designed primarily to preserve as much of the first as is consistent with a solution to those difficulties judged to be most dangerous.

The research programme serves many of the functions served by the Kuhnian notion of normal science and its paradigm. Chief among these is that it channels inquiry in fruitful directions. A refutation indicates that there exists a problem in some part of the latest theory, but does not point out its location. Logically speaking, the scientist could then seek to reform any part of the theory or of its auxiliary hypotheses; alternatively, he could attribute the fault to the ceteris paribus clause's being mistaken. All research programmes have, both positive and negative heuristics which serve to narrow the range of allowable modifications, and thus to direct the scientist's interest.

The negative heuristic tells scientists not to question certain basic premises and assumptions of the theory. These form the 'hard core' of the research programme, the elements common to all of the theories in the series which give the programme its continuity. On Lakatos's view, as long as the research programme remains healthy and thriving this hard core remains unquestioned. It is the last part of the programme to be brought into disrepute.

The so-called 'protective belt of auxiliary hypotheses' is the portion of the theory which is subject to change when adverse findings
arise. While the negative heuristic deflects criticism onto this protective belt, the positive heuristic indicates how this belt may be most profitably modified. "[T]he positive heuristic consists of a partially articulated set of suggestions or hints on how to change, develop the 'refutable variants' of the research-programme, how to modify, sophisticate, the 'refutable' protective belt ... [It] saves the scientist from being confused by the ocean of anomalies."

This positive heuristic both tells the researcher which anomalies to consider as crucial problems needing his immediate attention, and gives him some practical advice on how these may be dealt with.

Lakatos contends that science can be characterized as an ongoing competition between rival research programmes. Given this, he needs to develop a set of guidelines for deciding when a particular research programme ought to be abandoned in favor of its competitors. When does it become irrational to refuse to switch programmes and stick with the old one instead? Unless Lakatos can answer this question in a plausible manner, his project does not offer much of an improvement over Kuhn's. All it would do would be to give some added detail, not overcome the problems of relativism and 'irrationalism'.

To answer this fundamental question Lakatos turns to Popper's normative project for inspiration. Popper had argued that since the goal of science is to increase knowledge, the only scientifically acceptable modifications to theories are those which increase the theories' testable content. Lakatos borrows this idea wholesale and from it develops his concepts of progressive and degenerating problemshifts:

Let us say that ... a series of theories is theoretically progressive (or "constitutes a theoretically progressive problemshift") if each new theory has some excess empirical
content over its predecessor, that is, if it predicts some novel, hitherto unexpected fact. Let us say that a theoretically progressive series of theories is also empirically progressive (or "constitutes an empirically progressive problemshift") if some of this excess empirical content is also corroborated, that is, if each new theory leads us to the actual discovery of some new fact. Finally, let us call a problemshift progressive if it is both theoretically and empirically progressive, and degenerating if it is not.

The gross outlines of the Lakatosian theory of scientific rationality are easy to sketch using these definitions. Given the choice between a progressive research programme and a degenerating one, the rational scientist ought always opt for the progressive one. Similarly, the scientist should consider his research programme a success if following its positive and negative heuristics leads to progressive problemshifts, and unsuccessful if this leads to degenerating ones.

Lakatos's methodology clearly strives to fulfill the ideal set out by Popper. He offers it, first, as an accurate description of science; indeed, as a more accurate account than either critical rationalism or the Kuhnian theory of paradigm shifts. Secondly, he embraces the Popperian notion of normative methodology. He accepts knowledge as the goal of science and then argues that his is an efficient method for the generation of knowledge. Finally, he gives the scientific community substantial prescriptive directions: abandon degenerating research programmes in favor of progressive ones.

ii. The Prescriptive Side of Methodology

The early papers on the methodology of research programmes portray its prescriptive side as a theory of rational decision-making in scientific practice. Like its Popperian ancestor it appears to tell the
scientist how to go about deciding when he is faced with competing explanations. The theory of decision-making it propounds, however, will not stand up to criticism. And in the face of such criticism Lakatos abandons the idea that methodology can be prescriptive for scientific practice. The most articulate and interesting development of the criticism is found in Paul Feyerabend's Against Method. It is to this argument we now must turn.

A key difference between the methodology of research programmes and critical rationalism is that the former makes the evaluation of explanations far more difficult, partially by focusing this evaluation on a period in the explanation's development. Lakatos recognizes that the development of a research programme over time is not uniform, nor is there any evolutionary law decreeing that programmes always go from progressive to degenerating and never vice versa. The same programme may go through periods of stagnation and degeneration before suddenly and unexpectedly undergoing a progressive problemshift. Moreover, there are strong determinants other than epistemic ones which influence whether a programme is progressive or not. A research programme may fail to be empirically progressive, for example, if its theoretical development outstrips the development of the experimental apparatus needed to test its predictions. More to the point, it may well begin degenerating if for political or sociological reasons it fails to attract new scientists or adequate levels of funding (as may well happen if proponents of a rival programme control the educational and research institutions).

At this point Feyerabend argues that Lakatos cannot generate either strong prescriptions nor even heuristic advice for the scientist
forced to decide between rivals. For if any degenerating programme may become progressive once again, the individual whose goal is truth may choose to stick with this programme in the rational hope that his work (perhaps combined with the work of others) may get it back on the progressive track. But if, as Lakatos allows, "One may rationally stick to a degenerating programme until it is overtaken by a rival and even after," what advice does the methodology give to the researcher? Lakatos makes it rational both to switch to a progressive programme and to stick doggedly to a degenerating one.

One counter-argument Lakatos offers to Feyerabend relies on the distinction we saw earlier, in Kuhn's work, between advice for individuals and for social groups. He allows the individual to continue working on a degenerating programme, but not the community.

This [allowance that individuals can rationally choose any programme] does not mean as much license as might appear for those who stick to a degenerating programme. For they can do this mostly in private. Editors of scientific journals should refuse to publish their papers, which will, in general, contain either solemn reassertions of their position or absorptions of counterevidence (or even rival programmes) by ad hoc, linguistic adjustments. Research foundations, too, should refuse money.

Thus the methodology yields no direct advice to the individual scientist, but it does advise the research community (or at least those in positions of power over that community). It does not tell the individual when to abandon a programme, but it does tell the powerful when they should kill one off, or else drum it out of the corpus of respectable science.

Before passing on to criticism we must note how strong this advice is. Among the external, sociological, conditions which we noted earlier that may serve to keep a research programme in a non-progressive
though are precisely those which this current piece of advice encourages. The lack of funding and of opportunity to publish ongoing research will deprive the proponents of the programme of the wherewithal for research and of critical comment on their work. And as a secondary effect it will deprive the programme's research community of new members, since young scientists will generally go where money and fame go. The adoption of Lakatos's guidelines will have the intended effect of hastening a (possibly temporarily) degenerating programme onto a downward fatal spiral.

The question we must now raise is whether this distinction between individual rationality and communal rationality is a legitimate one. Kuhn used a similar distinction to point out a serious flaw in Popperian methodology. What he argued was that, if every individual scientist acted in accord with the Popperian canon of always trying to refute hitherto successful theories, science would become unable to progress. The acceptance of naive falsificationism as the primary rule of individual scientific rationality would insure that science could not achieve its goal of the growth of knowledge. Part of Kuhn's antidote was to suggest that a theory of rationality in science should direct its attention to the social structure of science with a view to determining how this structure could allow individual inquirers greater license. License allows individuals to act in different ways in the same situation, and it is precisely this diversity which helps science reach its goal. Scientific institutions designed to foster diversity thus service the growth of knowledge. It is this importance of diversity which drives Kuhn to prescribe a science guided by values rather than governed by rules.
Lakatos uses the distinction between the individual and the social in an altogether different way. He seemingly follows Kuhn in accepting that as a matter of individual rationality particular scientists may react differently when they find themselves in a degenerating re-search programme, and in arguing that this is vital if science is to achieve its goals. Were all scientists to abandon a programme at the first sign of degeneration (or at any other arbitrary point) what may be only minor problems would automatically take on the proportions of fatal objections. And many programmes would die prematurely. But instead of investigating social institutions which would allow individual scientists to nurture them, as Kuhn does, Lakatos invents rules of social rationality to kill them. The individual may choose rationally to work on such a programme, but the social institution is directed to hinder his work by seeing that he remains impoverished and incommunicado.

The peculiarity of Lakatos's stipulation is that it remains un-grounded. If there is no way for the individual to assess the knowledge-generating promise of a faltering programme, and therefore no rational grounds for determining whether it should be abandoned; then there is no obvious way for the leaders of the scientific community to accurately assess promise, and no rational grounds for them to fix its fate. At least Lakatos has provided no suggestions for a social method for assessing promise. The prescription he suggests must therefore take on the character of a convention, or as Feyerabend phrases it, it represents the adoption of a mere "conservative attitude."\(^8\)

The absence of an epistemic grounding for the prescription to eliminate degenerating programmes is a major setback for Lakatos's metho-
ological project. Feyerabend takes the failure to be additional ammunition in his struggle against method. If there is no rational reason to prefer progressive over non-progressive programmes in the quest for knowledge, the intellectually honest thing to do is to accept an extremely liberal attitude and allow all sorts of research programmes to proliferate.

Kuhn, on the other hand, is free to argue that this failure represents nothing less than a vindication of his own methodology from the charge of irrationality. Lakatos's objection to the Kuhnian depiction of a paradigm shift was that it offered no rules according to which the scientific community's change could be described. The decision to adopt a new paradigm (or alternatively, to retain the old) was left as a matter of psychology. But now it appears that Lakatos's methodology fares no better. The decision to pursue one research programme rather than its rivals is not made on the basis of a rational assessment of epistemic potential, but on the basis of social power relationships.

Faced with these problems Lakatos backs away from any direct concern with methodological prescriptions. In place of offering such prescriptive direction to the scientist, Lakatos reorients his project so that it gives advice to the historian of science. Thus at the beginning of his paper entitled "History of Science and Its Rational Reconstruction" he describes methodologies as "rules for the appraisal of ready, articulated theories," and notes that "this is an all-important shift in the problem of normative philosophy of science. The term 'normative' no longer means rules for arriving at solutions, but merely directions for the appraisal of solutions already there."

The shift to the history of science does not represent a complete
retreat in the search for a methodology that can affect scientific practice. Lakatos's forays into history are intended to buttress his claim that the methodology of research programmes is superior to its rivals. Episodes of successful science taken from history are to serve as the benchmark against which methodologies are to be measured.

The means of justification for which Lakatos does opt is based on the attempt to make as much as possible of the history of science into a rational enterprise. This is to be done in stages. The first step is to isolate those episodes which best serve as examples of scientific achievement. To do this Lakatos is willing to rely on the judgment of the elite in the scientific community. Like Popper, Lakatos takes it that methodology must begin with some notion of good science; but like Kuhn, he believes that this notion must come out of the scientific community. The first step only provides raw material for the methodologist; it sets the phenomena to be explained.

The methodologist then seeks to give a rational reconstruction of the events. That is, he tries to construct a model of rational scientific activity which takes as much as possible of the exemplary practice and makes it out as rational. The theory of rationality devised by the methodologist is used as a standard against which the historical agent is compared. An invidious comparison may, however, cut either way. If the theory is such that much of the history is interpreted as irrational (as Lakatos argues is the case with Popper's theory) then this is sufficient reason to reject the theory and return to the drawing boards. Once the theory has proven itself, once it has shown that it portrays most of science's history as rational, the situation changes. Then when he is confronted with episodes which cannot be expli-
cated in the theory's terms, the methodologist can rule that the participants acted irrationally.

The final step in the Lakatosian scheme is the comparison of the alternative methodologies. This is done simply by checking how much of history each methodology can portray as rational. Whichever one can make the greatest amount rational is the winner.

There are clearly two crucial and controversial points in this justificatory scheme. The first is the selection of the important historical episodes, and the second is the rational explication of these episodes. We shall return to the former concern presently.

The more immediate controversy Lakatos must face is one common in all of the historical and social sciences: the explanation of human action. Rational action is generally taken to be both goal-directed and rule-governed. The rational agent is understood as taking the best means to attain his goal or goals, for example. This gives rise to a problem which Lakatos's peculiar approach to the historical record exacerbates.

The problem is that, although it may be possible to give a rational explanation of an agent's actions (i.e., we can attribute to him a set of goals and some ordered rules of action), it is not always clear that the agent acted for the reasons cited. In the most vexatious cases it may well be the case that alternative theories of rationality can be used to provide conflicting accounts of the same action.

In historical studies, where a theory of rationality serves as a hermeneutical principle for isolating those bits of data that are important from those that are mere extraneous baggage, this problem is amplified. To take but a small example, Popper and Feyerabend both
cite Einstein's attitude toward empirical falsification as evidence for their different views of rationality. Popper claims that Einstein believed a negative experimental result would force the rejection of his theory of relativity, while Feyerabend claims that Einstein stated in print that the negative result would not have swayed him a bit. Each writer is prepared to accept some of Einstein's pronouncements as canonical and to reject others as uncharacteristic slips of the tongue or pen or wit. And each makes these decisions according to the rationality principle he has chosen.  

The question of interpretation need not be an unanswerable one. Lakatos, however, compounds the problem by refusing to seriously consider what evidence is available in particular cases. Instead of setting out to describe the beliefs and motivations of the acting scientist (as far as they are ascertainable from the extant evidence) Lakatos gives a reconstruction of what his beliefs and motives would have been, and should have been, had he been completely rational. To put the point into Popper's terminology, Lakatos seeks a third world reconstruction of the history of science. The historian is licensed to regard the "historical fact as a fact in the second world which is only a caricature of its counterpart in the third world." Thus his rational reconstruction "is not just a selection of methodologically interpreted facts: it may be, on occasion, their radically improved version."  

Given that Lakatos's historical research is geared to produce a reconstruction of events designed to show them rational according to the canons of his methodology, it is hard to fathom how this history can non-circularly vindicate that same methodology. While his principle that "In the light of better rational reconstructions of science
one can always reconstruct some of actual 'great science as rational' might function as an adjudicator of reconstructions, he strips it of any power by his cavalier attitude toward actual science.

Thomas Kuhn raises the same criticism against Lakatos:

What Lakatos conceives as history is not history at all but philosophy fabricating examples. Done in that way history could not in principle have the slightest effect on the prior philosophical position which exclusively shaped it. That is not to say that historical reconstruction is not intrinsically a selective and interpretative enterprise, nor that a prior philosophical position has no role as a tool for selection and interpretation. But it is to insist that, in the only sort of history which can hold philosophical interest, a prior philosophical position is not the only selective principle and also that it is not, as a selective principle, inviolate.

The driving force behind Lakatos's revisionist history of science and the heart of the rationality principle he deploys is his belief that science is a completely epistemic enterprise. This becomes clear in the method he adopts for demarcating the set of historical episodes for study.

Lakatos cannot distinguish his programme from Kuhn's by claiming that it gives the scientist better advice, but he can still maintain that Kuhn's is amiss because it fails to provide any non-arbitrary demarcation criterion. Kuhn cannot explain why he holds some communities to be scientific communities, hence worthy of study, and others to be unscientific. As a consequence of this, once he has labelled a community as scientific he must accept any paradigm shift it makes at face value.

Let us imagine for instance that in spite of the objectively progressing astronomical research programmes, the astronomers are suddenly all gripped by a feeling of Kuhnian 'crisis'; and then they all are converted, by an irresistible Gestalt-switch, to astrology. I would regard this catastrophe as a horrifying problem, to be accounted for by some empirical externalist explanation. But not a Kuhnian. All he sees
is ... an ordinary revolution. Nothing is left as problem and unexplained.14

On Lakatos's own theory this lacuna is to be filled by maintenance of the normative view of science. He maintains, in essence, that certain episodes of human history are to be isolated as instances of science by virtue of their epistemic goal. Some among these are further distinguished by their success.

The actual approach to the history of science which Lakatos follows differs surprisingly little from Kuhn's. The device he uses for isolating the successful episodes of science is that of polling the scientific elite. He attempts to establish, however, that this does not commit him to thoroughgoing empirical sociology of science. The judgments of the elite are open to self-revision, or even to an outside, normative, challenge; and even the very "definition of the scientific elite" is not simply an empirical matter.5 The need to demonstrate a difference between Kuhn's sociological orientation and his own forces Lakatos to develop his second departure from Popper.

iii. The Normative Side of Methodology

Many of the criticisms of Kuhn that Lakatos develops are identical to those raised by Popper. Both contend that he has not taken the normative side of methodology seriously enough. Both argue that Kuhn erred by not recognizing science as a means to truth, or at least a means of getting nearer the truth. The result of this is that Kuhn has no properly normative demarcation criterion for science. Given this concord in their commitments to the view that science is a goal-directed activity aimed at truth, it is a shock to find Lakatos accusing Popper of
failing to take the epistemological value of science seriously enough. But this is precisely the charge he lays.

Lakatos describes the rules unveiled by Popper as the rules of the scientific game. The set of rules allows us to distinguish science from pseudoscience. He does not deny that this formulation of the demarcation criterion is influential and important. Indeed, he characterizes his own efforts at setting out the methodology of research programmes as but an attempt to specify the rules of the scientific game more carefully and accurately.

Popperian methodology fails to do two things. It fails to demonstrate that it is superior to alternative methodologies; and it fails to establish that the scientific game is worth playing. Lakatos focuses his attention on the second failure, but as will become clear, his remedy for it is also intended as the basis of a demonstration of his method's superiority.

Lakatos's charge is that while Popper believes science is pursued because it succeeds in moving us closer to truth, he cannot show that it actually progresses in this way.

Popper's classical *Logik der Forschung* is consistent with the game of science being pursued simply for its own sake. Of course it is abundantly clear that Popper's instinctive answer was that the aim of science was indeed the pursuit of Truth; but, inasmuch as in 1934 the correspondence theory was in eclipse, he thought he could do nothing but adopt a cautious position, which, in its foundation if not its spirit, was entirely skeptical.16

The crux of this criticism is that within the Popperian system there is nothing which establishes that science achieves its goal. Popper offers little more than the valiant hope that scientific method allows us to get closer to the truth. Only an argument that science does
succeed "can separate constructive fallibilism from skepticism and from its evil consequences, like relativism, irrationalism, mysticism."\textsuperscript{17}

There is a genuine dilemma for the fallibilist of Popper's ilk. He assumes that science is a purely epistemic enterprise; that the goal of science is truth. The most efficient way to legitimate his own methodology (or to show its superiority to rivals) would seem to be that of showing how following its norms does produce truth. However this route is cut off by the fallibilist's eschewal of any criterion of truth. If we cannot know when we have reached the truth, how can we choose from among competing methodologies that set of rules which is best in attaining truth?

Popper's answer to this question, of course, is that we should not seek criteria of truth, but criteria of progress toward truth: "we have no criterion of truth, but are nevertheless guided by the idea of truth as a regulative principle (as Kant or Peirce might have said), and ..., though there are no general criteria by which we can recognize truth--except perhaps tautological truth--there are something like criteria of progress towards the truth."\textsuperscript{18} It is thus the increasing verisimilitude of theories which serves as his criterion of scientific progress.

Lakatos correctly notes the centrality of a strategy of convention- alism to the assessment of verisimilitude. It is only because there is a set of basic observation statements which are treated as unproblematic background knowledge that scientists are able to test theories to decide which have greater verisimilitude. Taking his cue from this, Lakatos suggests that the conventional acceptance of well corroborated theories ought to be countenanced also. "Why not extend Popperian
hardheaded conventionalism from the acceptance (without belief) of some spatio-temporally singular statements to granting similar acceptance to some universal statements ... and even beyond that, to some conjectural weak 'inductive principle'?

The mechanics of Lakatos's suggestion for the production of acceptable theories is straightforward. He follows the basic contours of the Popperian methodology for the construction and severe testing of scientific theories. He diverges from this methodology by suggesting a technique for preserving as much as possible of a falsified theory. Instead of rejecting all falsified theories, "we take the extent 'body of science' and replace each refuted theory in it by a weaker unrefuted version. Thus we increase the putative verisimilitude of each theory, and turn the inconsistent body of scientific theories ... into a consistent body of [accepted] theories."

This strategy of conventionalism appears in two distinguishable forms in Lakatos's writings. The paper "Changes in the Problem of Inductive Logic" advances conventionalism as a solution to the problem of the relation between theory and practice. There he charges that "Popper cannot explain the rationality of our practical actions, cannot have any practical philosophy and especially, any philosophy of technology which is based on science." The later "Popper on Demarcation and Induction" treats the more fundamental issue of the epistemological status of science. Both papers argue for the conventional acceptance of well tested explanations. However, the identity of the solutions to what are for Lakatos disparate problems is liable to be obfuscating rather than clarifying. What is at issue in the first case is the acceptance as a reliable guide to action of a well tested theory; while in the second it
is the acceptance as true of that theory.

In the first of these papers Lakatos is careful to state that his interest is in practice. Thus, after describing the technique of deriving the set of accepted theories (as quoted above) he goes on to say "we may call [these theories], since they are recommended for use in technology, the 'body of technological theories'." He further notes that the procedure for generating technological theories is not without cost. It violates the principle that the only acceptable scientific ploy in the face of a refutation is to modify the theory in a content-increasing way. But this cost is acceptable because "here we do not aim at scientific growth but at reliability."22

Lakatos is relatively clear here in asserting that science and technology differ, and that the difference is primarily one of goals. His is the common position that technology aims at successful practice. He wishes only to reaffirm the conventional wisdom that science can serve as an accurate guide for technology.

As long as Lakatos remains content with technological theories he has little argument with Popper. The latter's response is simply that his own concern has consistently been in science as an epistemological enterprise. To use the unfounded portion of scientific theories as a guide is perfectly reasonable if we realize this will on occasion get us into trouble, and if we maintain a proper scientific method for dealing with such troubles when they arise. The debate between Popper and Lakatos on this issue is a plain nonstarter.

The more serious deficiency of Popperian methodology is that in epistemological terms it is indistinguishable from skepticism. As a remedy to this Lakatos recommends, in essence, a consensus theory of
truth. Unfortunately, this theory appears only in an inchoate form in his writings, but there are serious difficulties with this suggestion. The crucial one is simply that it does not do the job it is designed to do: it does not halt the slide toward skepticism.

Even Lakatos realizes the weakness of this solution. He writes that a solution to the problem of the criterion would be "interesting only if it is embedded in, or leads to, a major research programme; if it creates new problems--and solutions--in turn. But this would be the case only if such an inductive principle could be sufficiently richly formulated so that one may, say, criticize our scientific game from its point of view."23
Notes to Chapter III


2 Ibid., pp. 90-91.


4 "Falsification", p. 50.

5 Ibid., p. 33. Italics deleted.


7 Ibid. Italics added.

8 Cf. Against Method, ch. 16. The foregoing argument is heavily indebted to Feyerabend.

9 Philosophical Papers, v. 1, p. 103 and n. 2.

10 See Against Method, pp. 57-58, n. 9 for references.


12 Ibid., p. 132.


15 Ibid., p. 125, fn 1.

16 "Popper on Demarcation and Induction," Philosophical Papers, v. 1, p. 155.
17 Ibid., p. 159.


21 Ibid., p. 183.

22 Ibid., p. 189.

23 "Popper on Demarcation and Induction," vol. 1, p. 164. Italics deleted.
CHAPTER IV

i. The Aim of Methodology

The previous chapters have forced to the surface the fundamental issue in contemporary philosophy of science: How is a methodology of science to be chosen? Kuhn, Lakatos and Popper each present a theory of scientific method: and these three theories can only be considered as competitors. Complicating the choice is the fact that each of the writers has a different conception of what an adequate methodology should accomplish.

The principal function of methodology in the Kuhnian conception is the descriptive one: the adequacy of a methodology is to be assessed according to how well, or poorly, it captures the essential activities of practicing scientists. It gains its prescriptive powers indirectly. The guiding virtue of the methodology is its accuracy; so he who wishes to conduct himself as a scientist would do well to pay close attention to the details of practice it describes.

This historically and sociologically informed approach to the study of science certainly possesses a wealth of attractive elements. In the first place, it avoids the sterile formalism endemic in most previous philosophy of science. Of particular import in this is that Kuhn and his followers begin to break away from views which postulate a unity of methods across all disciplines and sub-disciplines of research. The close scrutiny of particular research groups is undertaken for its own sake rather than as a means of determining whether their fields of study are scien-
tific or not.

Kuhn's work is important insofar as he takes the first steps away from the assumption that the sole goal of science is to provide closer approximations of the true picture of the world. But these steps are tentative ones.

His articulated position is that the fundamental mistake of previous descriptions of science is their ascription to it of a teleological character. That is, he argues against the view that science is goal-directed. However, the real target of Kuhn's attack seems to be the particular characterization of the goal promulgated by writers such as Popper. Thus he writes, "We may, to be more precise, have to relinquish the notion, explicit or implicit, that changes of paradigm carry scientists and those who learn from them closer and closer to the truth."

Kuhn's rejection of the claim that scientific revolutions occur because the newly accepted theory is closer to the truth is driven by his desire to accurately describe the history of science. The assessment that one theory has greater verisimilitude is not the reason that it is accepted. But the rejection threatens to make revolutionary scientific change irrational.

The Kuhnian response to this change is twofold. First he asserts, along with Popper, that scientific practice is our only paradigm of rational activity, and so the irrationality charge is nonsensical. In this vein he claims that his writings constitute an attempt to show that existing theories of rationality are not quite right and that we must readjust or change them to explain why science works as it does. To suppose, instead, that we possess criteria of rationality which
are independent of our understanding of the essentials of scientific process is to open the door to cloud-cuckoo land.²

Science is to be the foundation of a critique of rationality theory, and not the reverse.

There is a second prong of Kuhn's defensive maneuver. In it he argues that the historical development of science is essentially evolutionary in the sense that any theories could be serially ordered by use of criteria "including maximum accuracy of prediction, degree of specialization, (and) number (but not scope) of concrete problem solutions."³ This defense is open to variant interpretations; one of which preserves the major thrust of his descriptive program while another involves a major retrenchment and retreat toward a normative one.

On one interpretation, Kuhn may simply be adumbrating some of the findings of a careful scrutiny of science's history. That is, increases in accuracy and specialization, etc., are characteristics which are discovered in the course of describing scientific practice. The importance of these features is completely captured in noting that they are universally found across the revolutionary change from one theory to another.

Embracing this option leaves Kuhn with no way of dodging the first horn of the scientist's dilemma: he has no obvious argument that science is deserving of public support. Why should the public contribute toward an enterprise that simply becomes more accurate and esoteric while solving more puzzles? At the same time the descriptive alternative falls prey to Horkheimer's objection (which is echoed by Lakatos). Were scientists to change their works in ways which fail to fit the criteria, the descriptive methodologist would have to sacrifice the criteria. That the descriptive methodologist would be forced to make
this change shows that Kuhn cannot avoid the strong charge of relativism by this route.

According to the other interpretation the core of this defense is that there are identifiable goals which remain central to science throughout its evolution; goals that the scientists on either side of revolutionary episodes would embrace. Furthermore, these goals seem to be fundamentally related to science's character as an epistemic enterprise. On this reading, Kuhn's disagreement with the normative methodologists focuses on matters of detail rather than substance. All agree that the aim of science is to solve epistemic problems. Kuhn differs only in being less sanguine that a set of determinate criteria for directly judging the truth or the verisimilitude of theories is to be found.

The movement toward a normative methodology has some promise of solving the immediate problem. The normative methodology postulates a straightforward goal, and this allows for a simple judgment of the worth of the competing methodologies. Methods are to be rated according to how well they achieve the goal: that method is best which is most efficient in the epistemic endeavour. On the strong normative programs of Popper and Lakatos, the method which best produces truth is superior. This simplicity in principle unfortunately does not carry over into practice. The main impediment to its use is the absence of a convincing criterion of truth. Without a criterion it is impossible to determine which method is best.

Faced with the intractable problem of a criterion of truth Popper's writings become profoundly ambivalent. Originally he was very concerned to defend the status of science in modern society and to extend its
method into wider areas of life. Therefore in his earlier period Popper was inclined to utilize his criterion of verisimilitude as a vague criterion of nearness to truth (despite his disclaimers to the contrary). The structure of his argument was that science is valuable because it yields truth; and that his method of bold conjectures and refutations is to be preferred for science precisely because it has proven to be the best means of producing theories of increasing verisimilitude. This argument can have its intended practical force—it can influence practice inside and outside of the scientific community—only if the last claim can be redeemed. It is persuasive only if we can assess verisimilitude unequivocally.

The resurgence of sociologically and historically sophisticated methodologies, in effect, called on Popper to redeem his claim. Their proponents argued that actual practice did not follow Popperian guidelines. Confronted with this Popper could have countered with his own descriptive work, showing his opponents mistaken, or with arguments demonstrating how scientists employing these alternative methods fell short of their goal. The latter tack would have, of course, brought him directly into contact with the necessity of an absolute criterion and measure of verisimilitude. Instead, Popper surrendered the field of descriptive methodology to his antagonists: he shunned worry about the practice of science and retreated into his Third World. Having done this he can now allow that Lakatos and Kuhn may be correct in describing the day-to-day activities of scientists, and even that some of the protective strategies they depict may be important to scientific progress. But he allows this only as matters of psychology. Having granted this much of his opponents' paradigms, he claims that his own interest is in
the logic of science. His methodology thus gets transformed into a utopian blueprint for a science that is unlimited by typical human foibles, such as the lack of imagination.

If the shift to Third World epistemology is taken in good earnest, its cost to Popper's overall philosophy is high. He must forsake much of his bold program. He had portrayed his methodology not only as a propaedeutic to scientific research, as a set of prescriptions for practice, but also as the outline of a practical method by which a better society could be achieved without violence. In its new truncated form Popperian falsification cannot even serve as a reliable guide to the practice of science. Having severed the tie between his epistemology and the real operation of the prototypical human epistemic machine, Popper is at a loss to explain how his normative methodology can possibly be of assistance in so practically oriented a field as politics.

We should reiterate here that the root of the current difficulty lies in the inability of a thoroughgoing falsificationism to generate criteria of truth or applicable criteria of verisimilitude. It is because he cannot prove that science pursued in accord with Popperian methods yields more truth in the long run that Popper is forced to propound his thesis as a Third World logic.

The incompatibility between falsificationism and criteria of truth and verisimilitude makes it clear that no theory like Popper's can solve the dilemma of science in society. Popper deflects the desire of outsiders to control science by arguing that only if it is free from such dominance can science reach the truth. But then he is unable to validate this claim: he cannot show that a value-free research attains truth while other methods do not. This conclusion is
only reinforced by Lakatos's attempt to reconcile all of these factors.

As we saw in the previous chapter, the emendations to Popperian falsification suggested by Lakatos can be taken as matters of research heuristics, as part of a pragmatic justification of scientific method, or as part of an epistemic justification. It is only the last of these which interests us now.

Lakatos's suggestion is to extend the conventionalism already present in Popper's method. Where Popper grounds this empiricism on a conventional acceptance of basic observation statements, Lakatos wishes to extend the conventional acceptance to some laws. Thus he says that laws which have been severely tested and have survived, which have been corroborated, ought to be accepted as true.

This procedure, if acceptable, would cleanly avoid the first horn of the dilemma. It would demonstrate that science does achieve its goal of truth. However, it achieves this success only by trivializing the whole issue.

ii. The Dilemma of Methodology

It should be clear by this point that an adequate solution to the dilemma of science in society requires some radical departures from the standard positions outlined above. There are two fundamental assumptions shared by Popper, Kuhn and Lakatos: falsification and the belief that science has a purely epistemic goal. Because of the falsifiability thesis it is impossible to prove that science has achieved the truth; and since the goal of scientific method has been taken to be discovery of truth, it becomes impossible to prove that any given scientific method succeeds.
The most promising path to take at this juncture appears to be that of rejecting the assumption that science is a purely epistemic enterprise. Despite the centrality of this issue, it is not argued for by any of its proponents. Popper comes closest to raising it as an issue: he forthrightly states what he takes to be the goal of science and allows that this is not a matter for dogmatic legislation, but one open to rational discussion. One might reasonably expect him to develop the rationale for taking truth to be the goal of science; however this expectation goes unrewarded.

Among other prominent philosophers of science party to the methodology dispute only Paul Feyerabend treats the question of the aim of science as a fundamental one. The focus of his most recent work is precisely this issue and the closely related one of how the aims of science can be reconciled with other worthwhile social projects. To date we have considered only a portion of his deep and wide-ranging philosophy of science, so it will be helpful to attempt an overview of his entire program. 5

Most of Feyerabend's earlier writings can be considered as falling within the ambit of methodology as conventionally understood, although he maintained a fairly radical position. With Kuhn and Lakatos he argued the historical and contemporary practice of science. He diverged from them however, in refusing to put forward a positive method. While Kuhn and Lakatos used their historical findings first to confute other methodologies (including each other's) and then to defend their own, Feyerabend undertook only the first task.

Much of Feyerabend's work in this period was devoted to demonstrating that the episodes in the history of science universally considered exemplary could not be explicated by any of the methodologies
in the field. At the very beginning of Against Method he sets this out clearly.

The idea of a method that contains firm, unchanging and absolutely binding principles for conducting the business of science meets considerable 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 results of insufficient knowledge or of inattention which might have been avoided. On the contrary, we see that they are necessary for knowledge.6

It is important to note that within the context of these arguments Feyerabend assumes, with the mainstream methodologists, that the sole aim of science is an epistemic one. That is, he accepts as its goal the advancement of knowledge, and then argues that strict adherence to the methods proposed could not yield progress toward this goal.

The negative aspect of his work has had the unfortunate consequence of giving credence to the view that Feyerabend is concerned only to play at a skeptic's game, that he wants merely "to confuse rationalists by inventing compelling reasons for unreasonable doctrines."7. This imputation of purely skeptical motives is often used to justify one of two reactions. Some commentators have decided to dismiss his argument because of his motives, while others have argued that his position falls prey to standard objections against any skepticism.

A number of the writers who treat Feyerabend as a naive skeptic have sought to dismiss the entire program of Against Method as but another species of self-defeating argument.8 Feyerabend wishes to disparage all methods, so the argument goes. But in order to do this he must himself use some method, and the one he uses in this book is the
completely pedestrian one of historical research coupled with critical argumentation. He must recognize, therefore, the authority of this kind of method. At this point he is caught, for one can build a complete method of scientific practice starting with just these humble tools. The conclusion then follows that Feyerabend has perhaps given persuasive reasons for rejecting particular methodologies, but not for rejecting the general quest for a method.

Feyerabend's rejoinder at this point is simply that if one wishes to convince rationalists, he must use their methods. That is, one provisionally accepts and uses the rules of the methodology game in order to show the game's proponents that the rules lead to absurd results. The hope is that the proponents will recognize the absurdity and reject the whole game (instead of just tinkering with the rules).

There is a problem with this rhetorical technique. As long as Feyerabend's position remains negative there is a temptation to reject out of hand the strong conclusion and to continue tinkering with the extant canons of method instead. Feyerabend's argument leaves the methodologist with a choice between giving up methodology in general and giving up a particular method. Therefore it is not surprising that methodologists have, for the most part, conceded Feyerabend's arguments against the Popperian or Lakatosian depictions of science while refusing to allow that they vitiate the search for method. This is well represented in Kuhn's response to Feyerabend's "On the Critique of Scientific Reason":

Of course the author of Against Method is against the method presented in this collection. His essay supplies many reasons for his opposition, some brilliant, many cogent, and all unrelievedly negative. The last characteristic underscores what one already knew. Feyerabend does not want to
improve methodology or the theory of science, but to do away with it. That aim he is unlikely to achieve, but it gives him the skeptic's easy advantage in debate. Any gap in a particular methodological argument is for him a sign of the impossibility of the entire enterprise. Response in this place would be pointless.  

There is a second step in Feyerabend's sustained argument in which his position becomes more radical. As we have seen in the foregoing chapter, both the descriptive and normative methodologists select a certain set of historical achievements as exemplary of great science and try to explicate these. Darwin, Einstein, Galileo, et alia are universally accepted as the leading scientists and every effort is made to explain their successes. (Kuhn apparently breaks with this procedure in citing the importance of normal science. But even this break is of minor importance. He continues to think in terms of exemplary science. It is now the everyday practice of the modern physical sciences that must be explained.) Feyerabend's second step is to ask on what grounds these episodes (or normal practices) are chosen as bearing study. Here he wonders: "What's so great about science?"  

This questioning is tantamount to the demand that science as a whole be legitimated, and it goes to the heart of a basic assumption of most methodologies. For instance, it throws into doubt Kuhn's principle that "scientific behavior, taken as a whole, is the best example we have of rationality."  

Given that this question strikes at so fundamental a tenet, it is not surprising that it too is treated as a symptom of Feyerabend's skeptical intentions and therefore dismissed. John Watkins characterizes Feyerabend's question as follows:  

Our task ... was to discuss criteria of scientific progress; it was not to discuss whether scientific progress is good or bad for
mankind. This gave Feyerabend an easy opening: we dogmatically took it for granted that science is good and failed to examine the (meta-) question: "What's so great about science?". But suppose we had dealt with this question and that our answer had boiled down to this, that science seems to have done better than Azande magic, theology, etc., in its search for truth. Would that have met his complaint? Not at all. He would have complained that we dogmatically took it "for granted that Truth is something quite excellent" and failed to examine the (meta-meta-) question: "What's so great about truth?".12

The preliminary portion of Watkins's retort apparently answers Feyerabend's query. This appearance is misleading, for the comparison of Zande magic with modern science runs awry. Feyerabend argues that in the comparison of different traditions there is a natural tendency for the advocates of each to use its standards to judge the other. The proponent of science, for example, judges magical practice according to the methods of scientific practice and finds magic wanting. Such comparisons are inevitably biased, therefore inconclusive.

The comparison Watkins offers exemplifies the tendency. One conclusion of our scrutiny of the Lakatosian methodology of research programmes was that the claim that following its rules got us closer to the truth remained unsubstantiated. It consisted of a definitional stipulation. Thus Watkins's bold claim that science has done better than magic in the search for truth is convincing only for one who has already accepted this particular methodology or a closely related one. Beyond this, Watkins assumes without argument that the goal of Zande magic is the attainment of truth and therefore that the relative amounts of truth revealed is the proper measure of comparison.

The weakness of the putative answer is not very important to Watkins because his main reaction to Feyerabend is one of dismissal. Instead of seriously considering the basic complaint, that "the excellence
of science is assumed, it is not argued for." Watkins suggests that it can be ignored safely. If one attempts to answer it, Feyerabend will just raise another question. Rather than getting drawn into an unending series of questions the community of methodologists should draw the line by making some assumptions about what issues cannot be broached. Among the untouchable issues is that of the importance of science.

The reaction to his later writings highlights the problematic status of Feyerabend's second step. It is both necessary and fraught with danger. Only by taking the step does he avoid the charge that his is another self-defeating form of skepticism, but taking it carries him outside the generally accepted bounds of the methodology of science, so methodologists can pass over his arguments in silence.

When faced with this choice Feyerabend unequivocally opts for carrying the argument beyond the narrow boundaries of contemporary methodology. If the methodologists will refuse to seriously scrutinize the aims of science, then the issue must be brought before a wider audience. Out of this choice come his thoughts on the social control of science.

iii On the Control of Science

We should summarize the conclusions of Feyerabend's argument to this point before approaching the issue of controlling science. The main thrust of the argument is directed against the methodologists of science; that is, against those who strive to impose on scientists a single, unchanging method. Feyerabend's target here is the idea that any such method will be useful under all conditions. The argument that
no method is always helpful does not, of course, imply that none is ever helpful. So he need not, and does not, say that scientists never use a method. His contention is that scientists use an abundance of different, perhaps irreconcilable, methods.\(^\text{14}\)

A less obvious strand in the argument is that the decision which method should be used in a particular situation is best left to the agents who are acting in that situation, rather than being legislated by a disengaged group. The scientists involved in ongoing research should decide for themselves how best to proceed; they need not follow the dictates of methodologists.

The upshot of this argument is that the autonomy of scientists is to be preserved. The daily practice of science is to be unencumbered by the rules devised by non-scientists. This is the sense in which the scientific community should be methodologically opportunistic.

However beyond this, Feyerabend insists that the scientific community must be recognized as but one of the communities constituting contemporary society. The Western scientific tradition stands as one among many traditions competing for recognition. The larger question of autonomy—the question we are primarily concerned with—rises up here when we must decide among these competing claims. This is the precise point where Feyerabend's question "What's so great about science?" becomes crucial.

Feyerabend's accusation that a fair comparison between the scientific tradition and others has not been made appears on occasion as an observation of an historical accident. He suggests that the necessary comparison has not been forthcoming, but that this oversight could be rectified. For example, he writes:
In the 16th and 17th centuries there was a fair competition (more or less) between ancient Western science and philosophy and the new scientific philosophy; there was never any fair competition between this entire complex of ideas and the myths, religions, procedures of non-Western societies. These myths, these religions, these procedures have disappeared or deteriorated not because science was better, but because the apostles of science were the more determined conquerors, because they materially suppressed the bearers of alternative cultures. There was no research. There was no 'objective' comparison of methods and achievements.\textsuperscript{15}

Here the fact that some traditions have been eliminated without a fair trial is seen to be a consequence of a characteristic human weakness that "only a few people are content with being able to think and live in a way pleasing to themselves and would not dream of making their tradition an obligation for everyone."\textsuperscript{16} It seems that comparison of traditions would proceed unhindered if only everyone would accept a more liberal and self-critical attitude toward his own tradition.

The remedy suggested here hardly lends credence to Feyerabend's reputation as the enfant terrible of contemporary philosophy of science; indeed, it is little more than a restatement of Popper's position. But we should note that Feyerabend uses examples of neglected comparisons principally to make life miserable for upholders of the scientific tradition which values highly the fair competition of ideas. In this vein he excoriates scientists who reject astrology and methodologists who reject alternative systems of medicine.\textsuperscript{17} His aim here is to demonstrate to such defenders of the scientific tradition that they violate a rule they themselves proclaim.

Lurking in the background is a much stronger argument to the effect that the objective comparison of many particular traditions may be impossible. All comparisons presuppose a set of standards against which judgments are made. However Feyerabend notes that standards are
themselves parts of traditions. There is no tradition-independent standard which can adjudicate between two traditions, and if there is no standard they share, comparison is thwarted.

Earlier in this chapter we saw that non-epistemic, pragmatic goals might be used as a basis for justifying science. That suggestion is essentially that science as a whole be vindicated by showing that it delivers. Although Feyerabend has no general brief against this suggestion, the conclusion of his argument is that it must founder. Before we can begin to ask whether science is a useful instrument, we must ask what goals are to be pursued. Feyerabend claims at this point that no general answer can be given, that goals arise out of particular traditions. He makes this point with one of his characteristically colorful examples:

The results of science are obvious .... Its results will appear magnificent to some traditions, execrable to others, barely worth a yawn to still further traditions. Of course, our well conditioned materialistic contemporaries are liable to burst with excitement over moonshots, the double helix, non-equilibrium thermodynamics. But let us look at the matter from a different point of view, and it becomes a ridiculous exercise in futility. It needed billions of dollars, thousands of well trained assistants, years of hard work to enable some inarticulate and rather limited contemporaries to perform a few graceless hops in a place nobody in his right mind would think of visiting—a dried out, airless, hot stone. But mystics, using only their minds, travelled across the celestial spheres to God himself whom they viewed in all his splendor, receiving strength for continuing their lives and enlightenment for themselves and their fellow men.18

The upshot of this entire line of argument is that, although Feyerabend clearly recognizes the legitimacy of attempts to impose social controls on science, there is no general theory to tell us when science should be reined in nor any general methodology for attaining and exercising control. The conclusion of the attack on methodology is that
there can be no legitimate elite who ought to take control of society. Neither scientists nor philosophers of science can claim to know how society ought to be ordered, or even how the restricted scientific community should be run. The lack of any effective elite forces the return of control to the populace. For Feyerabend, the antidote for the excesses of science is to let traditions proliferate, and to prevent the elites of any single tradition from dominating society.
Notes to Chapter IV

1 Revolutions, p. 170.

2 "Reflections on my Critics," p. 264.

3 Ibid.

4 See, in particular, "The Aim of Science," in Objective Knowledge, pp. 191-205.

5 In Against Method Feyerabend begins his frontal assault on the place of science in society (see p. 298ff), but it is not until Science in a Free Society and the provocative essay "Democracy, Elitism, and Scientific Method" (Inquiry 23 (1980): 3-18) that the full force of his rhetoric is deployed.


7 Against Method, p. 189.


10 "On the Critique of Scientific Reason" in R.S. Cohen, et al., eds., Essays in Memory of Imre Lakatos, Boston Studies in the Philosophy of Science, vol. 39 (Dordrecht: Reidel, 1976), p. 110. Although this question is dealt with explicitly only in his latest writings, it forms part of the background against which Against Method is written. In the "Introduction", Feyerabend gives two reasons for the overthrow of any unitary method. The first is the epistemological one; the second is that the education of all scientists to use any of the methods offered "cannot be reconciled with a humanitarian attitude" (p. 20). He returns to the same theme later in discussing Popperian methodology when he raises two questions. Again, one is whether the adoption of the rules is compatible with scientific progress. The second, "far more important one", is: "Is it desirable to live in accordance with the rules of critical rationalism?" (p. 174).


14 This is stated most clearly in Science in a Free Society, pp. 163-165.

15 Science in a Free Society, p. 102.

16 Ibid., p. 80.

17 Ibid., pp. 91-96 and pp. 205-207.

18 Ibid., pp. 30-31.
CHAPTER V

Feyerabend's arguments show the extensive repercussions of methodology's failure to justify itself. Since strict adherence to any one method, regardless of the peculiarities of particular historical conditions, would sometimes prove antithetical to scientific progress, no single methodology can show itself to be universally useful. Therefore methodology cannot maintain its pretensions to being normative or to issuing legitimate prescriptions. Even if we consider science to be a purely epistemic affair, we must abandon the search for a genuinely research-guiding and perennially helpful method. Within science researchers must remain free to pursue whatever avenues they believe might be profitable; and the philosopher is no longer allowed to sit in judgment of scientific practice.

The results of the failure are not confined to the philosopher's conception of science; it must be extended to the scientist's own self-concept. The researcher must recognize that the idiosyncratic methods he uses are not binding on others either within or without the scientific community. The scientist is specifically forced to admit that science is but one institution in society, and as such it is subject to social control. Neither the supposed purity of its aims nor the uniqueness of its methods exempts it from the competition with the other institutions and the traditions which form them.

A final consequence propounded by Feyerabend is that all endeavors at providing a social theory capable of mediating disputes between traditions must also be abandoned. The adherents of different traditions
are to decide for themselves in the course of their practice whether mediation is even worth attempting. To Feyerabend, grand theories and blueprints for society represent nothing more than essays by adherents to the tradition of rationalism at imposing their tradition over all others. That is, they are part of the strategy of the rationalist intelligentsia for gaining power over society.

The final conclusion of Feyerabend's argument places him in stark opposition to the critical theory of the Frankfurt School. The dispute is worth pursuing both because the disputants share a concern about the tendency to treat science as the model for all rational thought, and because of its importance for the question of how science is to be controlled.

1. Knowledge-constitutive Interest

Jürgen Habermas clearly stands in the tradition of critical theory in setting the goals of his philosophy: he aims to produce reasoned critiques of both science and society. These critiques are in reality different aspects of one theme. For Habermas, as for his intellectual ancestors, it is impossible to gain an understanding of modern society without considering science; and, more contentiously, it is impossible to understand modern science without studying its relationship to society. Society and its science must be viewed as part of an historical process.

Whereas Horkheimer and the earlier generation of critical theorists were mainly concerned to protect science, as the epitome of rationality, from the social forces of irrationality; on Habermas's view, such a defense is now superfluous. The place of science in the contemporary world is more than secure. The current problem is rather one of prevent-
ing science from becoming the dominating force in the social world.

The principal danger Habermas perceives is that the methods which have proven themselves successful in the physical sciences will be uncritically carried over into the social sciences and into political decision-making. The danger is that the traditional methods of political discussion and accommodation will be swept away once the scientific methods which have proven themselves useful in providing means of control over physical objects prove equally useful in providing means of control over societies. Habermas's worry is that the same forces which have delivered man from domination by nature will deliver him into domination by his fellow man.

These concerns dictate the overall directions of Habermas's argument. A primary focus of his study is the methodology of the social sciences. In particular, he wishes to argue against the claim that the social sciences must mimic the methods of physical science, and the related one that political questions are reducible to questions of social engineering.

The starting point of Habermas's argument is a commentary on recent methodologies of natural science, particularly Popper's critical rationalism. His contention is that normative methodologies fall short of their mark precisely because they fail to treat the issues embedded in Feyersapend's query, "What's so great about science?" as serious ones.

Habermas rejects the standard response, that science is valuable because it reveals truth (or gets us nearer the truth), because it fails to adequately reflect the fact that it is science itself which gives us criteria of truth. He accepts Popper's Kantian view that the human intellect does not passively receive information from the external
world, but actively sorts the data according to categorical schemata. Furthermore, he agrees that the choice among schemata is not arbitrary. Popper emphasizes that this choice must be limited by the data: most schemata must be rejected because they are inconsistent with the data. Habermas, while not denying Popper's point, emphasizes that the choice is also limited by human interests. Not all consistent interpretations of the data are acceptable because many conflict with the deep-seated human interests which guide the quest for knowledge.

The bulk of Habermas's work is devoted to the study of these knowledge-constitutive interests. Concerning them he advances a trio of theses. First, he argues that there are three distinguishable and irreducible knowledge-constitutive interests: the technical, the practical and the emancipatory. Second, he claims that each interest leads to a different ideal of knowledge. Since the interests are irreducible, this means that no single ideal can yield a satisfactory unified knowledge. Finally, he contends that each of the three interests can be adequately justified.

Habermas's critique of science, and of social science in particular, is open to a singularly important misinterpretation. The major thrust of his work, as we have just said, is to uncover the interests which underlie the process of research. But the concept of 'interest' is ambiguous, and this gives rise to confusion. There is a strong tendency to think of interests as always special interests; i.e., as the peculiar concerns of isolateable groups within society. With interests understood in this way critique can take either of two forms. It could be conceived as the uncovering of the particular values which special interest groups try to impose on science. Thus conceived critique would
aim at the purification of science through the unmasking and elimination of such interests. Its goal would be value-free science as described by Max Weber in his classic papers. Alternatively, critique could be conceived as the uncovering of the special interests which the scientific community (now seen as an isolated group within society) has. The goal of this sort of critique would be to prevent the idiosyncratic values of scientists from being imposed on society at large. Feyerabend's most radical writings on science in society are critique in this sense.

We have rehearsed the difficulties facing critique understood in either of these ways. Against the first interpretation Popper's argument, that a critical theory so conceived is superfluous, is a compelling one. On his view, whatever purification of theories from the individual scientist's special interests is required is also automatically supplied by the public methods of science. We can rest assured that any explanation offered by a scientist that is distorted by his membership in some social group will be severely tested by his fellow scientists and rejected if found wanting in truth value. All that is needed to insure that science remains free of special interests is an undertaking by society to protect the institution. While the operation of scientific method remains unconstrained science naturally purges itself of special interests. And against the second interpretation the argument of reflexivity is compelling. If science is infected with the special interests of the scientific community, is this reason enough to reject it? Is it not the case that the critical theory seeking to replace scientific theory will be similarly infected by the special interests of the critical theorists?
There are large elements of critique of these sorts in the writings of critical theorists, and they appear in Habermas's works as well. Such critique is natural; given the fact that critical theory strives to maintain strong ties with practice. One of the guiding principles of the theory is that modern society stands in need of major restructuring to dissolve the domination of men by their fellows. Furthermore, one of the key dangers perceived by these theorists is that the sciences are poised to deliver more potent means of domination into the hands of those special interest groups now wielding power. A critical theory is seen as a means to oppose domination.

We must emphasize, however, that this notion of special interests controlling science is not the central concept of interests Habermas deploys in his systematic critique of the sciences.

The knowledge-constitutive interests Habermas spends so much of his time trying to uncover and elucidate are generalized ones. They are shared by all members of the human community, not just by members of specialized groups. The fact that they are universal is taken by Habermas to mean that, again unlike special interests, they can be adequately grounded.

The difference between the knowledge-constitutive interests and special interests is also elucidated in the different way the two are discovered. On Popper's view special interests are to be found roughly in the following way. We begin with a model of how science should proceed. If actual practice does not proceed in this way, we have reason to suspect that special interests may be intervening. (Of course other factors, such as the stupidity of the scientists involved, may
be the cause of the distortion.) Habermas's knowledge-constitutive interests are discovered in an entirely different manner. Again we begin with an accepted model of proper scientific method. But then we ask why it is that this model is accepted. Science is, after all, of human construction and so we can ask why humans have constructed it in the way they have.

The bare bones of Habermas's theory of human interests and their place in science were sketched in his inaugural lecture at Frankfurt:

There are three categories of processes of inquiry for which a specific connection between logical-methodological rules and knowledge-constitutive interests can be demonstrated. This demonstration is the task of a critical philosophy of science that escapes the snares of positivism. The approach of the empirical-analytic sciences incorporates a technical cognitive interest; that of the historical-hermeneutic sciences incorporates a practical one; and the approach of critically oriented sciences incorporates the emancipatory, cognitive interest.¹

ii. The Technical Interest

A most fruitful starting point for understanding Habermas's notion of knowledge-constitutive interests is his criticism of Popperian methodology.² The crucial weak point he finds in the methodology is Popper's solution of the so-called "basis-problem".

We have already seen how on Popper's analysis a scientific theory is tested against a set of basic statements and rejected if it comes into conflict with these. In order to maintain the consistency of his falsificationism Popper had to admit that these basic statements could not be justified, that they always remain provisional and vulnerable to subsequent rejection. What preserves the attempts at refutation of a theory from the logically possible 'infinite regress' is the
consensus reached by the community of researchers to accept a given set of basic statements, thereby holding them immune from criticism at least temporarily.

Habermas contends that Popper offers a completely inadequate solution of the problem. To begin with, he accuses Popper, unfairly, of persistently ignoring the fact "that we are normally in no doubt at all about the validity of a basic statement." Then, after noting "[t]he regress of an— in principle— infinite series of basic statements, of which each succeeding one would have to corroborate the assumptions implied in the previous statement, is, to be sure a logically grounded possibility," he wonders why doubt about basic statements does not arise within the scientific community. Why do scientists not doubt basic statements in the same way they doubt theories?

Contrary to Habermas' accusation, Popper also believes that basic statements are treated as unproblematic. Indeed, on his view, their unproblematic status is absolutely vital to scientific progress. He does not investigate in detail the mechanisms through which scientists achieve the needed consensus, but Popper does lay down the rationale for considering its maintenance to be of major significance. Basic statements are accepted as accurate representations of reality in part because prolonged controversy over them would leave the whole of science in ruins. And it is important to keep the social institutions of science in good repair because science is our best instrument for progressing toward truth.

Habermas rejects this rationale because of its inherent circularity. For Popper, progress toward truth is demonstrated by the increasing verisimilitude of theories. But increasing verisimilitude can be
shown only so long as a consensus over basic statements has already
been achieved. As long as we maintain a correspondence theory of truth
it will prove impossible to convince the dissident scientist that he
ought to join the consensus.

The solution of the basis-problem Habermas suggests maintains much
of the Popperian position. Along with Popper he claims that an answer
is to be found in the institutional structure of science.

Research is an institution composed of people who act together
and communicate with one another; as such it determines, through
the communication of the researchers, that which can theoretical-
ly lay claim to validity. The demand for controlled observa-
tion as the basis for decisions concerning the empirical plau-
sibility of law-like hypotheses, already presupposes a pre-
understanding of certain social norms.5

The norms Habermas has in mind differ in significant respects from
those appealed to by Popper. They remain the norms adopted by the sci-
entific community, but they cannot be justified without stepping outside
of this community. Habermas asserts that the reason decisions regarding
basic statements are in fact made with little controversy is that they
remain grounded in the pre-scientific context of social communication:

[T]he empirical validity of basic statements, and thereby
the plausibility of law-like hypotheses and empirical sci-
entific theories as a whole, is related to the criteria
for assessing the results of action which have been social-
ly adopted in the necessarily intersubjective context of
working groups. ... The so-called basis-problem simply does
not appear if we regard the research process as part of a
comprehensive process of socially institutionalized actions,
through which social groups sustain their naturally preca-
rious life.5

Controversies over the meaning of basic statements do not become criti-
cal because science, as a part of society, is guided by an interest in
successful action. The members of the scientific community avoid press-
ing the issue because controversies would destroy their community to the
detriment of society's ability to act successfully.

The key difficulty with Popperian methodology is that it cannot vindicate its bold justificatory claim to uncover reality: its boast, that science built in accordance with its strictures is in ever closer correspondence with reality, remains empty. Against this purely epistemic interpretation of science Habermas poses a pragmatic one: "empirical-scientific theories reveal reality under the guiding interest in the possible informative safeguarding and extension of feedback-regulated action."7

In the place of Popper's plain decision to accept a particular empirical methodology, Habermas offers a justification. The crux of the justification is that the use of scientific method allows the satisfaction of very basic human needs:

This interpretation, according to which the empirical-analytical sciences allow themselves to be guided by a technical cognitive interest, enjoys the advantage of taking account of Popper's critique of empiricism, without sharing a weakness of his falsification theory. ... [B]y the time that knowledge of empirical uniformities is incorporated into technical productive forces and becomes the basis of a scientific civilization, the evidence of everyday experience and of a permanent regulated feedback is overwhelming; logical misgivings are unable to assert themselves against the plebiscite of functioning technical systems.

The acceptance of this pragmatism clearly constitutes a bridging of the gap between scientific methodology and technological utilization of information (a gap recognized by Lakatos).

The postulated goal of technical control over nature also differs from the Popperian goal of truth in terms of the ease with which it can be defended. While Popper and his followers piously proclaim that their preferred goal can be rationally defended, they never come through with
the argument. Habermas, by contrast, sets out the essential elements of an anthropological justification of his postulated goal.

The practical interest in the domination of objective processes apparently stands out from all other interests of practical life. The interest in the sustenance of life through societal labor under the constraint of natural circumstances seems to have been virtually constant throughout the previous stages in the development of the human race.9

Habermas's solution to the basis-problem stands open to a number of major objections. Principal among these is a cluster of arguments to the effect that he has misunderstood and misrepresented scientific practice. There are two from this cluster which merit our attention. One holds that his pragmatism is fatally flawed; while the other holds that in his excessive preoccupation with a vaguely defined positivism he has failed to see that the newer post-positivist philosophies of science have moved radically away from the premises he criticizes. Regarding the second criticism, it is often argued that Habermas's entire argument needs revisions in light of the methodologies propounded by the likes of Kuhn and Feyerabend.10

Habermas and Popper hold the decision over which basic statements to accept to be an unproblematic one; however this tenet runs up against Kuhn's observation that sometimes in science this decision is not only problematic, but actually insoluble. His thesis of incommensurability embodies this contention that there arise scientific disagreements over meaning which are not amenable to rational solution:

We must be clear on two related points about Kuhn's position. He does not deny that a consensus on meaning is important for science. The functioning of normal science presupposes an agreement among those scientists working within the paradigm. It is only during revolutionary
periods that the consensus is broken. And even then a stable consensus among the adherents of each paradigm remains in place. For Kuhn an agreement shared by the proponents of the same paradigm—and not one shared by all scientists—is crucial in both revolutionary and non-revolutionary periods.

Because Kuhn denies the existence of perennial and universal consensus in science, he is forced to reject the Popperian model of scientific progress. However he does not wish to deny that science is progressive. Neither scientists nor philosophers can compare theories directly against reality in order to discover their degrees of verisimilitude; and scientists on opposite sides of a revolutionary divide may judge the comparative verisimilitude of theories differently. Verisimilitude therefore cannot serve as a measure of progress.

The problem of explicating his alternative notion of progress is a large one for Kuhn, as we have seen. On the one hand he conceives of the norms operative in science—including the linguistic norms governing observation reports—in sociological terms. They are the norms and patterns of speech inculcated during a scientific training. When pushed to explain why the norms of such an isolated group should be respected as pre-eminent, Kuhn is forced to claim that the question is suspect. To suggest that science’s norms need justification is "to open the door to cloud-cuckoo land." But on the other hand, he tries to blunt the charge that he makes revolutionary scientific change irrational by deploying a theory of evolutionary progress. In his own defense he states his belief that there are certain values which remain stable through revolutions. Maximum accuracy of prediction, degree of specialization, and number of concrete problem solutions form part of a set of
criteria which would allow an uninitiated observer to chart the progress of scientific development. Kuhn's own presentation of this defense, however, cites these values only at a descriptive level. He does not put forth any reason for supposing that these values must characterize future scientific change.

Habermas's thesis that there is an interest in technical control which guides theory construction in natural sciences seems to fill this serious lacuna in Kuhn's philosophy without being committed to the position that revolutions such as Kuhn describes cannot occur.

All of the criteria Kuhn suggests will characterize the evolution of science can plausibly be supported as normatively binding because of the way they directly or indirectly contribute to the technical utility of scientific knowledge. For instance, the more precise a prediction is, the more useful it will be in calculating the effects of an action. Supplemented in this way with a pragmatic explication of the ends of science Kuhn's philosophy becomes a normative methodology.

Habermas's argument for the technical interest binds scientific inquiry to the need to insure survival by dominating nature; a need which grows out of, and is evident in, everyday life. This gives birth to the suspicion that he has failed to see the obvious fact that scientists very seldom take immediate pragmatic concerns into account in formulating and testing their explanations. One element in his philosophy which feeds this suspicion is his assumption that conflict over basic statements is virtually nonexistent.

It is Kuhn's contention that meaning conflicts are an integral part of progressive scientific revolutions. To insist that new theoretical languages cannot be formed is to adopt a conservative attitude. This
Conservatism is especially worrisome if it is used to guarantee the supremacy of the traditional, everyday language. As Feyerabend and others have argued, many of the most important scientific advances were made possible only through the replacement of traditional descriptions. And such replacements inevitably produce disputes over meaning.

We must notice that Habermas's thesis does not commit him to the view that such conflicts over meaning are impossible, nor to the view that the traditional language is pre-eminent. On his scheme the interest in technical control delimits the languages that could be used in the construction of a natural science, but it does so without indicating a uniquely satisfactory one. Certain languages are eliminated as being unsuitable to the task of preserving life; they simply constitute nature in the wrong way. The problem of incommensurability, as it arises in Kuhn's work, however, is a problem in the comparison of alternative scientific languages. It comes into play only after each of those languages has passed the test posed by the knowledge-constitutive interest.

The more general argument against Habermas's position stems from the standard objections to pragmatism. Popper has argued against any form of instrumentalism that the logics of testing theories and of testing instruments differ fundamentally. The former leads to the rejection of theories while the latter merely establishes limits of proper use. Any number of writers have pointed out that pragmatic concerns seldom bother scientists in their research. And these same writers often note that the history of science gives ample reason to believe that such concerns should not intervene in science. A most efficient way of satisfying the needs of survival is by allowing science to pro-
ceed according to its own requirements, without any direct reference to these needs. Given these arguments, it is incumbent on us to clarify the sense in which Habermas's theory of science is a pragmatic one. 13

Even in his earliest works Habermas sought to disarm some of the criticisms noted above. In response to Popper's disciple, Hans Albert, he noted that "[i]t is not theories themselves which are instruments but rather that their information is technically utilizable" and that "[t]echnical utilization of knowledge is ... in no way intended in the research-process ... actually, in many cases, it is even excluded. 14 Later he admitted that his original formulation of the pragmatic thesis did leave the impression that the realm of scientific argument is rather directly tied to that of action. He went to some pains to alleviate this misimpression:

In the investigations up to this point I have brought out the interrelation between knowledge and interest, without making clear the critical threshold between communication (which remains embedded within the context of action) and discourses (which transcend the compulsions of action). To be sure, the constitution of scientific object domains can be conceived as a continuation of the objectifications which we undertake in the world of social life prior to all science. But the genuine claim to objectivity which is raised with the instauration of science is based on a virtualization of the pressure of experience and decision, and it is only this which permits a discursive testing of hypothetical claims to validity and thus the generation of rationally grounded knowledge.

His point here is to note that there is a realm of science in which theories compete with one another for acceptance, and in which concern for even pressing social needs is illegitimate. At the same time he denies that this realm of science can be completely disentangled from links with the interest in technical control: "an indirect relationship to action can be shown for theoretical knowledge, but not anything like a
direct derivation from the imperatives posed by the praxis of life."15

The indirect relationship Habermas refers to here is best understood as a methodological pragmatism. The knowledge-constitutive interest in technical control does not function at the level of the acceptance or rejection of theories; this decision must be made on the basis of the specific guidelines contained in the scientific methodology. However, these guidelines themselves must be chosen, and this choice is guided by the technical interest. The fundamental human need to insure survival acts as a check on the kinds of methodology that could possibly guide the natural sciences.

A brief summary of the argument to date is in order here. Habermas accepts the Popperian goal of a strongly normative methodology. Methodology is to serve as a guide to scientific practice. He does not accept, however, that the goal of the scientific pursuit is a purely epistemic one. This putative goal arbitrarily severs science from its historical roots in everyday actions; and when it is supplemented with the thesis of falsification it leaves science without a reasoned justification. In its place he substitutes the goal of technical control over the processes of nature. This goal recognizes science's place as part of the productive forces which man utilizes in the struggle for survival. Moreover, it is a goal which is more easily justified in anthropological terms than Popper's goal of truth. Finally, it gives us a purchase on the issue of the adequacy of methodological prescriptions: we cannot tell whether our theoretical systems correspond with reality, but we can judge—albeit in a rough way—whether they can be successfully utilized in technology.
iii. The Practical Interest

Habermas claims that there are actually three distinct knowledge-constitutive interests, no one of which can be eliminated without serious loss. The second of these, the practical interest, is the one which guides us in our normal, everyday communications with our fellows; and it is also the interest which guides those sciences which come into play when, for various reasons, the normal ways of achieving understanding fail.

[H]ermeneutic inquiry discloses reality subject to a constitutive interest in the preservation and expansion of the intersubjectivity of possible action-orienting mutual understanding. The understanding of meaning is directed in its very structure toward the attainment of possible consensus among actors in the framework of self-understanding derived from tradition. This we shall call the practical cognitive interest.  

Concerning this practical cognitive interest, Habermas seeks to establish two major theses. In the first place he argues, contrary to the position of many proponents of a naturalistic social science, that inquiry guided by this interest can yield reliable knowledge. Then he argues, contrary to the position of many proponents of a unique methodology for the social sciences, that a science of man constructed according to the methodological model of physics is indeed a possibility. While admitting that a reliable science of man can be constructed by following either the model of everyday understanding of human actions or the model of scientific explanation of physical events, Habermas quite clearly recommends the former model in most situations.

Habermas's defense of the first thesis, that the methods of reaching understanding which are implicit in everyday discussions and explicit in hermeneutics yield reliable knowledge, depends crucially on the current state of science. He argues that a full study of the methodology of science reveals that scientific progress itself is dependent on the exist-
tence of a reliable prescientific understanding of other people. Science as we know it simply could not exist without ordinary human communication among scientists. This mode of communication necessarily existed before science could be constructed; and it has not yet been affected—within the scientific community, at any rate—by the ongoing development of science.

The writings of contemporary methodologists make clear the extent to which the functioning of the scientific enterprise depends on reliable prescientific understanding among scientists. All of the philosophers of science we have studied are united in the belief that science can only be appreciated properly as a social institution with characteristic practices, norms, terminologies and so forth. Lakatos, Kuhn and Feyerabend, as well as the young Popper\(^{17}\), all accept that this adequate account of science must take into consideration how scientists really communicate with one another. And a striking feature of their works is that throughout their methodological disputes scientific communication is treated in everyday terms. All, at least implicitly, agree that this communication presents no special problem; it is to be understood in the same way that more pedestrian, non-scientific discussion is understood. This underlying agreement is even carried over into the disputes on the basis problem and incommensurability. Popper and his followers contend that scientists always somehow manage to reach common understanding of a basic descriptive language. Popper presents no detailed description of how this consensus is achieved in practice, but where he deals with the issue he gives no reason to suspect that the process differs at all from that employed in settling prosaic matters. Scientists have no unique method of achieving consensus, they are just more persistent in using the ones we all share. Kuhn and Feyerabend deny
that scientific communication is as smooth as Popper suggests, and they allow that it occasionally breaks down altogether. These allowances tend to make scientific communication resemble everyday discussions even more closely.

A number of philosophers of social science have noted the facts that communication among scientists closely parallels ordinary communication outside of science and that the methodological understanding of science is couched in the terms used in prescientific understanding of human action in general. These writers often use the facts to impugn the very notion of a social science which attempts to explain human actions in any terms other than those of prescientific understanding. One leading exponent of this view is Alfred Schutz.

Schutz, like Habermas, notes that the normal functioning of the scientific enterprise depends on ordinary mutual understanding among scientists:

[T]he postulate of describing and explaining human behavior in terms of controllable sensory observation stops short before the description and explanation of the process by which scientist B controls and verifies the observational findings of scientist A and the conclusion drawn by him. In order to do so, B has to know what A has observed, what the goal of his inquiry is, why he thought the observed fact worthy of being observed, i.e., relevant to the scientific problem at hand, etc. This knowledge is commonly called understanding. ...[O]ne thing is sure, namely, that such an intersubjective understanding between scientist B and scientist A occurs neither by scientist B's observation of scientist A's overt behavior, nor by the introspection performed by B, nor by identification of B with A.

Schutz thus clearly recognizes that there is a serious rift possible in the social scientist's understanding. On the one hand, the scientist understands his own actions and those of his fellow researchers according to one set of concepts and proto-theories; while on the other, he may seek to explain the behavior of all other humans using a different set of scientific concepts and theories.
Schutz sees such a rift as a possibility only in the social sciences. He writes:

It is up to the natural scientist and to him alone to define, in accordance with the procedural rules of his science, his observational field, and to determine the facts, data, and events in it which are relevant for his problem or scientific purpose at hand. Neither are those facts and events preselected, nor is the observational field pre-interpreted. The world of nature, as explored by the natural scientist, does not "mean" anything to molecules, atoms and electrons. But the observational field of the social scientist—social reality—has a specific meaning and relevance structure for the human beings living, acting, and thinking within it. 19

The recognition of the inconsistency in the scientist's attitude toward different groups of people is but the first step in an argument that the natural and social sciences must be methodologically distinct. For once the inconsistency is recognized, three possible courses of action remain available to the scientist. He may simply note its appearance, without taking any action to remove it. Secondly, he may strive after consistency by reconstructing science's self-understanding. That is, he may try to explain science in exactly the same way he explains all other human behavior. A prominent example of this tack is the sociology of science of the Edinburgh School. David Bloor, a major figure in this group, writes in the preface to his most important work: "If we want an account of the nature of scientific knowledge, surely, we can do no better than to adopt the scientific method itself. Science is a social phenomenon so we should turn to the sociologist of knowledge." 20 Finally, the scientist can strive after consistency by using (and refining) the same methods for understanding all social action that he currently uses implicitly in acting as a member of the scientific community. The last option is obviously Schutz's preferred one.
Schutz and other writers in the field attempt to force the
decision among the options outlined by focusing on the fact, noted
above, that social agents have explanations of their own actions. He
begins his argument by setting as a preliminary task for the scientist
a comprehension of the agent's own understanding.

The thought objects constructed by the social scientist,
in order to grasp this social reality, have to be founded
upon the thought objects constructed by the common-sense
thinking of men, living their daily life within their
social world. ... Thus, the exploration of the general
principles according to which man in daily life organizes
his experiences, and especially those of the social world,
is the first task of the methodology of the social sciences. 21

Were the full extent of Schutz's claim that the social scientist
should try to understand the agent as he understands himself, the con-
flict with the proponents of a unified methodology would be relatively
minor. Few of those proponents advocate a wholesale abandonment of the
understanding expressed in everyday discourse. Rather, they suggest
treating participants' accounts as a rich source of interesting hypotheses.
Schutz's delineation of the first task of the social scientist would thus
be assigned to the logic of discovery.

Treating understanding as an hypothesis-generating technique leaves
open the issue of testing, and it is over this issue the proponents of
unified methodology insist the social scientist must follow the same
methods as the physical scientist. In the first place, the social
researcher's interpretation must be checked against the participant's
to see if the two coincide. Assuming that they do, the question still
remains whether this interpretation is true. The participant's understanding
of the situation in which he acts and of his own action may be false;
and only rigorous scientific testing can show this.
Those who promote the methods of *verstehen* willingly admit that the actor's account of his actions may be false in particular instances, and that this falsehood might best be discovered through some form of intersubjective testing akin to that found in physics. But they refuse to accept that the entire conceptual scheme used in ordinary language to describe human action might be replaced by one of strict causation—be it couched in terms of "contingencies of reinforcement" or of biological genetic causes. Peter Winch, for one, rails against attempts to replace talk of motives, beliefs, etc., with notions of causation; complaining that all such attempts inevitably lead to inconsistencies or contradictions. Schutz claims more generally that any acceptable scientific account must satisfy a "postulate of adequacy":

> [E]ach term in such a scientific model of human action must be constructed in such a way that a human act performed within the real world by an individual actor as indicated by the typical construct would be understandable to the actor himself as well as to his fellow-men in terms of common-sense interpretation of everyday life.

This conception of social science as understanding has been savaged routinely by analytical methodologists on a number of grounds. That social actors have an understanding of their actions and language to express it does not imply that this is the only theoretical language that is legitimate. To be sure, if one wishes to live in a particular society it may be necessary to understand one's fellows' self-interpretations; but a social science need not be aimed at helping one achieve this practical end. And the fact that the everyday language accounts for actions largely in terms of motives and reasons does not show that a causal language is necessarily misplaced in the human realm. One can argue either that the motives are causes, or that descriptions in terms of causes can and should replace everyday descriptions. Moreover, the committed defender of a
unified science can go on the offensive by claiming the excessive reliance on the vague descriptions of action now current in ordinary language is an impediment to scientific progress. The natural sciences, after all, did not become progressive until they rejected common-sense explanations of natural phenomena and the traditional language expressing them.24

The attempt to establish a distinct methodology for the social sciences thus falters in the confrontation with proponents of unified methodology. There simply is no argument advanced which demonstrates that the social scientist must take into account the participants' pre-scientific interpretation, or that he cannot subject this interpretation to scientific testing of the standard type and possibly leading to its wholesale rejection.

Although Habermas also stands opposed to the project of a unified science as it is usually portrayed, the failure of Schutz's attempt to legislate a special social scientific method is not a fatal blow to his account. The original impetus for Schutz's argument was a defensive one: the argument was conceived as a counter to the claim that the methods of the natural sciences are the only valid ones for producing reliable knowledge. For Habermas this goal of combating the hegemony of natural science methodology is clearly the crucially important element in the verstehen approach. But with this in mind, it should become clear that Schutz's intended conclusion was actually too strong. Instead of arguing that the method of interpretive understanding is a legitimate one—one which yields reliable knowledge—he argues unsuccessfully that it is the only possible legitimate one.

On Habermas's view, recognition of the reliability of knowledge
gained through prescientific communication is forced by the recognition of the role this communication plays in science itself. And the importance of the practical interest in securing consensual understanding is grounded on anthropological considerations akin to those which legitimate the technical interest in control over nature.

The technical interest is legitimated by pointing out the necessity of control for survival in this seemingly hostile world. As Habermas emphasizes, however, man has traditionally secured his survival through social action; and social action requires effective means of establishing mutual understanding. Modern science is but the most recent and most thoroughly institutionalized form this project of survival through social labor has taken.

Today the institutionalized research practice of the empirical sciences secures a flow of information that was formerly accumulated prescientifically in systems of social labor. This information digests natural or contrived experiences that are constituted in the behavioral system of instrumental action. ...[S]cience has become first among the productive forces. The empirical sciences simply do not represent an arbitrary language game. Their language interprets reality from the anthropologically deep-seated vantage point of technical mastery. 23

Science is thus seen by him as a new institution for satisfying deep-seated human needs which had previously been satisfied in less organized ways.

The first line of defense of the practical interest clearly relies on a traditional interpretation of the world as reflected in practices of investigation. The tradition out of which the modern natural sciences grow recognizes two categories of things in the world; physical objects and human beings. And it also recognizes
different modes of knowledge appropriate to each category: the motion of physical objects is the subject of explanation guided by the technical interest, while the action of humans is the subject of understanding guided by the practical interest.

Habermas shares with Schutz a desire to preserve the traditional realm of understanding. However mere reliance on the success of a tradition does not strike him as sufficient warrant for its preservation. He gives two distinct reasons for this caveat. In the first place, he is concerned that a tradition may well serve as an ideological justification for the suppression of legitimate desires; so uncritical acceptance of tradition may preserve illegitimate relationships of domination. Thus it may be necessary to overturn a traditional interpretation in order to achieve liberation.

His second concern centers on the efficacy of a defense based on an appeal to tradition. As he notes, "old patterns of interpretation are also weakened and overturned 'from below' by a new practice."26 In the modern world the most likely, and worrisome, overturning of the tradition is the one in which the techniques of investigation which have proven so successful in transforming the means of production of the necessities of life will replace the traditional means of understanding man. The danger is that the methodology of science, which is constituted by the interest in domination, will be imported without modification into the study of man and will thereby produce a technology for the control of human action.

Habermas points out that the danger posed by the hegemony of natural science's methodology is already manifest in the successes
of behaviorism and in the direction some biomedical research is taking:

Today the psychotechnic manipulation of behavior can already liquidate the old fashioned detour through norms that are internalized but capable of reflection. Behavioral control could be instituted at an even deeper level tomorrow through biotechnic intervention in the endocrine regulating system,—not to mention the even greater consequences of intervening in the genetic transmission of information. If this occurred, old regions of consciousness developed in ordinary-language communication would of necessity completely dry up.27

The position Habermas wishes ultimately to defend is a complex one. He wishes to argue that the traditional recognition of both the technical and practical interests ought to be protected against any evolutionary change leading to a recognition of only the former. Such protection is called for because the cost of methodological unity is human freedom. Habermas also wishes to argue that the goal of emancipation is not an arbitrarily chosen one, rather it is the object of a rational decision.

A complete development of this argument must await an explication of the third knowledge-constitutive interest, the emancipatory interest. That explication is the task of the last section of this chapter and the whole of the next. There is another issue which arises in the discussion of Schutz and which should be dealt with more fully before moving on.

Keeping alive the project of his intellectual ancestors, Habermas is attempting to put together a theory which can have practical importance; one which will be useful in criticizing accepted practice with a view toward changing it. If we conceive of the attempt to extend the methods of the natural sciences into the study of man as a development within the dominant scientific tradition, the issue at hand can be more
clearly focused. Here the question must be squarely faced: having rejected the claim that a social science constructed on the model of physics is logically impossible, what defense can be raised against the threat of natural science's methodological hegemony? How can the widely accepted tradition be criticized effectively?

Habermas's conception of a critical theory is quite ambitious. What he seeks is a trenchant type of criticism which does not find its foundations inside of any particular tradition, but which can nonetheless be used to probe any number of different traditions. He is driven to search for an atypical form of criticism because of the severe limitations faced by either of the two possible forms of tradition-bound criticism.

The first form of tradition-bound criticism is one which finds its starting point inside of the very tradition which is its target. The critic who embraces this technique implicitly accepts the standards and rules of criticism which are a part of the tradition and tries to use them against it. The primary difficulty with this form of criticism is that it must remain partial. The rules of criticism within any tradition place limitations on the kinds of question that can be asked of it. There are issues which simply cannot be raised, and so large parts of the tradition must remain unscrutinized. If frustration with those limitations on internal criticism leads one to search for a different form, the question arises: what alternative standards of criticism are to be used? The answer seems to be that if we wish to criticize a tradition externally, i.e., without using its own standards, we must use the standards of an alternative tradition. The abandonment of internal criticism thus apparently leaves us with a competition
among rationally incommensurable traditions.

This sketch of the difficulties faced by the two forms of critique which remain bound to particular traditions can be fleshed out by returning to the recent disputes in the philosophy of science. And once again, Feyerabend’s thought is particularly instructive because in it we can see a clear development from a reliance on internal criticism to a reliance primarily on the external form of tradition-bound criticism. From the reception his early and transitional works received we can gain an idea of the difficulties the internal form of criticism faces; while in his development of the alternative criticism we can see the dangers with which it must deal.

We have seen already how Feyerabend’s early writings fell within the standardly accepted scope of methodology, albeit at the radical end of acceptability. His style of argument was to take the proposed descriptions of proper scientific method and compare them with the history of science. The conclusion of these comparisons was that none of the proposals achieved the goal of descriptive accuracy. He argued further that it was a good thing for science that scientists had not followed any one method, for this strategy would have precluded progress. Feyerabend then added to this demonstration his belief that the continued search for the scientific method would prove to be futile as well.

The reaction of the methodologists of science to this part of Feyerabend’s work has been to accept for consideration his criticisms of particular methodologies without accepting his pessimistic prognosis on scientific methodology in general. His arguments have forced others to reconsider and reformulate their methods, but have apparently led very few to forsake their faith in scientific method’s existence.
The attacks on individual methodologies by Feyerabend are good illustrations of internal criticism. In executing them he merely makes use of rules and values already accepted as parts of the methodology game. Methodologists agree that the models they construct should be tested by comparison with actual progressive science. In mounting these attacks Feyerabend is doing just what methodologists generally do. His suggestion that methodology be abandoned altogether, however, moves beyond the limits of acceptability. What is at issue here is the viability of the entire tradition, and the adherents of any tradition are loathe to let it go. Their reaction to this more radical questioning of the tradition is essentially to avoid engaging it at all.

The outright rejection of certain sorts of questions on the ground that they fall outside the proper realm of a tradition is much more obvious once Feyerabend broaches such a question. His query 'What's so great about science?', although not intrinsically suspect, is nonetheless ignored by the great majority of methodologists. Most seem to argue, with Watkins, that answering such a question is not their task.

Having reached the limits of effective internal criticism, Feyerabend turns to external criticism of the methodological and scientific traditions. His turn seems to have two motivating factors behind it, one of which is the ineffectiveness of his earlier criticism. Were he content to play at the skeptic's game, this lack of practical result would not necessarily cause him to seek out a different way of criticizing the scientific tradition and the methodologies which try to elucidate it. But Feyerabend recognizes that these traditions have a strong practical influence which should be held up to criticism.

[A] more detailed analysis of successful moves in the game of science ('successful' from the
point of view of the scientists themselves) shows indeed that there is a wide range of freedom that demands a multiplicity of ideas and permits the application of democratic procedures (ballot-discussion-vote) but that is actually closed by power politics and propaganda. This is where the fairy-tale of a special method assumes its decisive function. It conceals the freedom of decision which creative scientists and the general public have even inside the most rigid and the most advanced parts of science by a recitation of 'objective' criteria and it thus protects the big-shots (Nobel Prize winners, heads of laboratories, of organizations such as the AMA, of special schools, educators, etc.) from the masses (laymen, experts in non-scientific fields, experts in other fields of science)

... It is time to cut them [i.e., these elites] down in size, and to give them a more modest position in society.28

In this argument it becomes clear that it is the invidious social effects of reliance on specialized elites for decision-making—a reliance which is fostered and protected by appeal to a proper scientific method—that motivates Feyerabend to continue his quest for an effective form of critique.

We should note here how closely Feyerabend's motivations parallel Habermas's. Both philosophers apprehend an enormous threat in the increasing scientization of society. As long as natural science is considered the paradigm of rational decision-making there is an overwhelming tendency to downplay and eventually eliminate other traditions and ways of knowing. It is against this tendency which both men react.

Feyerabend carries his belief that there is no universally successful method for criticizing scientific theories over into the sphere of critique of traditions. Just as there is no ordered set of epistemic values which can serve as a touchstone for selecting the most acceptable theory, so there is no set of general values which can be used to choose
the best way of life from among those offered by competing traditions.

Since there are no rules of criticism which stand above all traditions, the values and rules actually used in criticism must come out of one tradition. This holds just as strongly for the critical comparison of divergent traditions as for the internal criticism of one by its adherents. For the most part, the rules which are used will be those of the particular tradition to which the critic belongs.

On Feyerabend's view then, the failure of methodologists to consider seriously his attack on the scientific tradition is neither surprising, nor does it mean that that tradition is beyond the reach of effective criticism. In trying to criticize science the citizen of a free society "will use the standards of the tradition to which he belongs; Hopi standards, if he is a Hopi; fundamentalist Protestant standards, if he is a Fundamentalist; ancient Jewish standards, if he belongs to a group trying to revive ancient Jewish traditions." 29

We are left, in the final analysis, with a competition among traditions; and on Feyerabend's account, it is a competition which should remain without a single clear winner. He seems satisfied that such a competition, if freely run, will protect against the hegemony of any particular tradition. The spread of scientization will be limited by the ability of the scientist to convince proponents of other traditions that his methods and results are valuable when judged according to the standards of their traditions. And Feyerabend is convinced that that ability is limited.

Feyerabend must immediately confront the suspicion that the argument he offers for his proposal is inconsistent. His general
methodologically informed elites) commits him to the proposition that all values are tradition-dependent. Values arise out of a particular tradition and they are ultimately justified in the terms of the same tradition. No 'Higher' justification is possible. And there are no a-traditional values to which we can appeal for a rational comparison of competing traditions. 

At the same time, however, Feyerabend wishes to argue that in-the competition among traditions some moves are illegitimate. Since he is particularly concerned that the scientific worldview is able to perpetuate itself largely by means of its monopolization of the educational system, and because he believes such a monopoly is illegitimate, Feyerabend asserts that "A free society is a society in which all traditions are given equal rights, equal access to education and other positions of power." More generally stated this becomes the claim that "democratic relativism denies the right of traditions to impose their form of life on others, and it therefore recommends the protection of traditions from interference from outside."

The apparent inconsistency in Feyerabend's argument disappears when we look at it closely. He does not assert categorically that all traditions must be treated equally and protected from interference. Rather, his claim is that protection and equal access to power are demanded by the democratic relativism of a free society. His is a hypothetical claim: If we value a free society, then it is necessary to give all traditions a chance to develop without outside interference.

The one thing Feyerabend does not do is attempt to produce any general argument, bound to no tradition or traditions, that a free society guided by principles of democratic relativism should be valued. Such an attempt would indeed involve him in the inconsistency.
The lacuna in the argument poses no problem as long as the value of a free society is not challenged. And Feyerabend may well be optimistic that no challenge will be raised by any of his immediate antagonists, since they too proclaim the value of freedom.

Feyerabend's defense of the autonomy of traditions dodges the charge of inconsistency, but it does so only by a narrowing of its intended audience. His proclamation that traditions should be free to develop with protection from outside interference is compelling only when it is directed toward the members of traditions which already accept the value of open competition with others. If this fortuitous coincidence of values does not already exist, then Feyerabend has little left to say.

Serious questions about the effectiveness of Feyerabend's proposals for the social control of science are raised by these considerations. In the first place, Feyerabend himself notes that many traditions do not recognize the value of unfettered competition, and that the adherents to particular traditions are seldom willing to eschew interference with other traditions.

Only few people are content with being able to think and live in a way pleasing only to themselves and would not dream of making their tradition an obligation for everyone. For the great majority—and that includes Christians, rationalists, liberals and a good many Marxists—there exists only one truth and it must prevail. 33

More important, for our present concerns, than these general observations that not all traditions recognize freedom as an ideal and that many which do give lip-service to the ideal do not follow its implications in practice, is Feyerabend's own repeated observation that the proponents of the scientific tradition fail to uphold the
equal freedom of other traditions.

[Science] reigns supreme because its practitioners are unable to understand, and unwilling to condone, different ideologies, because they have the power to enforce their wishes, and because they use this power just as their ancestors used their power to force Christianity on the peoples they encountered during their conquests. Thus, while an American can now choose the religion he likes, he is still not permitted to demand that his children learn magic rather than science at school. There is a separation between state and church, there is no separation between state and science.34

A point Feyerabend repeatedly returns to in his early onslaught against the notion of a scientific method is that there has never been a fair comparison (fair in the sense accepted by proponents of method) between the scientific tradition and those it sought to displace. The proselytizers of science simply did not let them take place if they could achieve dominance without them.

The rather pessimistic view that science has the ability to dominate other traditions simply disappears in Feyerabend's later writings on the control of science. There he argues that citizens' initiatives are the proper tool to use in control. While it is true that such initiatives have had some isolated successes—they "have stopped highways and nuclear reactors"

---it is not at all clear that they offer any real hope of stopping the spread of decision-making by powerful scientific and technological elites.

Habermas's search for an emancipatory science is driven by his gloomier view of the efficacy of reliance on citizens' initiatives. His worry is that the spread of scientific method has proceeded so far already that such isolated movements stand little chance of success against the interlocked system of science, business interests and government.
which has evolved. This system already has enough power to resist the demands for freedom to develop raised by nonscientific traditions. And unless an effective stand is taken against this system, the very possibility of opposition may be lost. It is because he fears that the piece-meal criticism of the scientific tradition offered by other isolated traditions cannot stand up to the power of science, that Habermas even attempts to develop an alternative comprehensive view.

iv. The Emancipatory Interest

Habermas is chary of limiting the scope for the rational criticism of traditions primarily because this limitation makes them practically unassailable; and traditions often conceal relationships of domination. This danger in the uncritical acceptance of traditions is a constant theme throughout Habermas's works. His original criticisms of the positivistic and Popperian methodological schools aim at demonstrating the limits they impose on the scrutiny of science. But this demonstration is important primarily as part of the fabric of a larger critique of society. What is objectionable about the limits chosen is that they effectively prevent inquiry from uncovering and critically assessing the interests guiding research.

As long as the methodologists of these schools restrict their concern to the natural sciences, the concealment of the knowledge-constitutive interest remains unproblematic. However when they recommend that the social sciences adopt the successful methods of the physical sciences, the concealment does raise problems. Social sciences developed according to this advice would share the physical sciences' constitutive interest; but given their different object domain, it would manifest
itself as an interest in the technical control over human beings. The limitations on critique inherent in the common conception of the methodology of science prevent this interest from being uncovered or discussed; thus they serve, however indirectly, to preserve the structures of domination present in society.

It is worth stressing that this talk about "concealed interests" and "limitations imposed on inquiry" is apt to mislead because of its intentional connotation. Habermas does not argue that the proponents of positivistic methodologies make a conscious effort to suppress certain issues because of their desire to control society. Rather, the concealment is embedded in the tradition itself and may remain unnoticed by its proponents. The development of an emancipatory science, with the tasks of uncovering such limitations and showing their effects, aims to enlighten these adherents as much as anyone else.

Habermas originally developed his notion of an emancipatory science by turning to Freudian psychoanalysis which he represents as the only extant science guided by an interest in emancipation. The model of psychoanalysis he constructs portrays it as a science which stands between the natural and the hermeneutic sciences, borrowing elements of method from each. 36

Psychoanalysis begins as a hermeneutic science. Habermas notes that "Freud always patterned the interpretation of dreams after the hermeneutic model of philological research." The psychoanalyst begins his inquiry as an attempt to understand the speech and actions of his patient; and in its initial stages this attempt, simply parallels the methods used by any two people to achieve mutual understanding through ordinary discussion. In everyday life, understanding is often achieved
without much difficulty. And all competent speakers know various techniques for eliminating those misunderstandings which do occur. It is only when these mundane methods fail—as happens, for instance, when the people speak different languages or come from different cultures, or when one of the partners is an historical text—that the more refined methods of the hermeneutic sciences must be employed.

Habermas points out the fact that in mundane instances the communicating partners take account of more than just each other's overt utterances:

The grammar of ordinary language governs not only the connection of symbols but also the interweaving of linguistic elements, action patterns, and [bodily] expressions. In the normal case, the three categories of expression are complementary, so that linguistic expression "fits" interactions and both language and action "fit" experiential expression. 37

While the psychoanalyst makes use of the methods of understanding common to all speech and may occasionally use the more sophisticated methods of hermeneutics, he is primarily interested in those cases where these methods fail to yield understanding. The psychoanalyst moves away from the instances of complementarity and focusses instead on those situations in which "actions and non-verbal expressions belie what is expressly stated." And from these situations he selects those in which the discrepancy is not, and cannot be, noted by the speaker himself.

Psychoanalytic interpretation is concerned with those connections of symbols in which a subject deceives itself about itself. The depth hermeneutics that Freud contraposes to Dilthey's philological hermeneutics deals with texts indicating self-deception of the author. 38

This desire to understand a communication which is distorted by his discussion partner's self-deception forces the psychoanalyst to look outside of the everyday methods of interpretation. It is this move away...
from ordinary language understanding that drives the analyst to look for
lawlike regularities in the distortions. And it is in this search for
patterns of distortion and their causes that the analyst borrows most
heavily from the methodology of the natural sciences.

A brief sketch of the methods of physiology will help us understand
the peculiar status Habermas attributes to Freudian psychoanalysis, since
physiology is the branch of science that analysis most closely resembles.
The Freudian analyst treats those portions of everyday communication
which cannot be understood according to normal rules as possible symptoms
of an underlying condition needing attention. In this he follows the
lead of the physiological pathologist who considers any deviation from
the norm as possibly indicative of a hidden somatic pathology. In order
to recognize such symptoms, however, the physiologist must already have
in his possession an accurate description of the body's normal functioning.
He must recognize a norm if he is to notice deviations from it. The psycho-
analyst must also have a conception of the norm if he is to recognize
deviations. But herein lies the first major diâââsanaology Habermas notes
between physiology and psychoanalysis.

While the physiologist may construct a description of normal bodily
functioning through observation of many actual bodies, it is not clear
that the analyst can construct his model of the norm in the same way.
On Habermas's view, the psychoanalyst's conception of the norm is an
idealization of a state which is not commonly attained. The state Habermas
posits as the norm accepted in Freudian psychoanalysis is that of indi-
vidual autonomy. 39

The notion of an ideal state of individual development is more
problematic than Habermas indicated in Knowledge and Human Interests,
In the first place, several critics have questioned his suggestion that the norm of autonomy is in fact accepted by Freud, arguing that it fails to account for the importance of the instincts in his theory. An evaluation of this exegetical dispute is peripheral to our present interests. 40 A more central concern turns on the normative status of the ideal. This concern will take on a more definite shape as we complete the sketch of physiology's method and note how psychoanalysis's differs from it.

Once the physiologist has isolated complexes of symptoms, he seeks to discover lawlike regularities regulating these to specific somatic abnormalities, and then to discover the causes of these abnormalities. Insofar as human physiology is an adjunct of medical practice, the discovery of causal, etiological laws is seen to be particularly important because the laws may offer the doctor technical means of manipulating the patient's diseased body and bringing about its return to the normal state.

The psychoanalyst's research procedure seems to parallel the physiologist's closely here. His discipline is akin to medical practice in that the analyst too wants to cure pathological conditions, thereby returning his patient to a normal state. And this therapeutic goal drives him to search for the causes of various pathologies and for effective means of treatment.

The general theoretical framework Freud developed explains pathologies as being caused by disturbances in the normal course of childhood development. Habermas gives this framework a linguistic formulation, interpreting childhood development as a process of socialization through the acquisition of communication skills and pathologies as problems in communicative speech and action. On this linguistic view, in-
stances of an individual's inability to communicate are indicative of a developmental process gone awry.

The physiologist's inquiry into the genesis of somatic pathologies ends with the discovery of causal laws allowing him to predict outcomes under given initial conditions. The psychoanalyst ends his search with general interpretations which take the form of historical narratives. Like causal laws, general interpretations allow the assimilation of particular cases into the general:

Every historical representation implies the claim of uniqueness. A general interpretation, on the contrary, must break this spell of the historical without departing from the level of narrative representation. It has the form of a narrative, because it is to aid subjects in reconstructing their own life history in narrative form. But it can serve as the background of many such narrations only because it does not hold merely for an individual case. It is a systematically generalized history, because it provides a scheme for many histories with foreseeable alternative courses.

It is Habermas's contention that, despite surface similarities, the procedures of the physiologist and the psychoanalyst are radically different. He tries to substantiate this by demonstrating related differences in the way the two scientific disciplines test their hypotheses and then utilize the information gleaned from testing.

Particular cases are used to corroborate or falsify both the putative causal laws generated in physiology and the putative general interpretations in psychoanalysis. If the prediction made on the basis of a law fails, then the law is to be rejected. Similarly, if the historical narratives of the lives of particular patients reconstructed using a general interpretation prove to be inadequate, then that general interpretation is to be rejected. But on Habermas's view, evaluating an historical narrative's adequacy differs significantly from evaluating the success of a prediction,
To demonstrate the difference, Habermas assumes a simplistic Popperian picture of hypothesis testing in the physical sciences. The researcher in those sciences formulates questions and nature responds unequivocally. Her answers are plain for all researchers to see. And while a successful prediction may not lead to the hypothesis's acceptance, a failed one leads to its rejection.

Turning to the evaluation of general interpretations, Habermas finds two major disparities. In the first place, neither the individual analyst nor the community of analysts has the ultimate authority to decide whether a particular psychoanalytic interpretation is correct. The patient in analysis maintains veto power over any proffered interpretation: "analytic insights possess validity for the analyst only after they have been accepted by the analysand himself."42 "It depends on the hermeneutic understanding of the person providing the material whether an element of his life history is adequately interpreted by a suggested theoretical expression."43 Habermas then argues that the patient's exercise of his authority, expressed in his response to an interpretation is equivocal.

[If a patient rejects a construction, the [general] interpretation from which it has been derived cannot yet be considered refuted at all....the experience of reflection is the only criterion for the corroboration or failure of hypotheses. If it does not come about there is still an alternative: either the interpretation is false (that is, the theory or its application to a given case), or, to the contrary, the resistances, which have been correctly diagnosed, are too strong.

Habermas then explicitly draws out the conclusion that the equivocal nature of the patient's response establishes the methodological uniqueness of psychoanalysis.
The criterion in virtue of which false constructions fail does not coincide with either controlled observation or communicative experience. The interpretation of a case is corroborated only by the successful continuation of a self-formative process, that is by the completion of self-reflection, and not in any unmistakable way by what the patient says or how he behaves. Here success or failure cannot be intersubjectively established, as is possible in the framework of instrumental action or that of communicative action, each in its way. Even the disappearance of symptoms does not allow a compelling conclusion. For they may have been replaced by other symptoms that at first are inaccessible to observation or the experience of interaction.44

His adumbrations on the ambiguous nature of the patient's response complicate Habermas's demonstration that psychoanalysis is a unique science; and this complication is needless. Critics of psychoanalysis (the most notable of whom is Popper) seize upon such claims as evidence that psychoanalytic theory is unfalsifiable, hence unscientific. Habermas clearly wishes to allay such doubts and establish the scientific credentials of Freudian analysis; yet he only fuels the doubts when he says that the outcome of the test situation "cannot be intersubjectively established."

This particular challenge could be met by arguing that it is not the patient's immediate acceptance or rejection which is decisive, but his long-term evaluation which is. If the patient does not come to accept the analyst's explanation at some time, then it must be rejected by the analyst.

The foregoing line of defense would provide Habermas with but brief respite. For the main target of those who would deny scientific status to psychoanalysis is surely the assertion that the patient in
pretation false. Even with the suggested emendation this notion of authority faces severe problems.

The idea that the patient is the final arbiter of an interpretation's correctness faces particular internal difficulties which remain even if we leave aside general arguments against private criteria. We can see these difficulties by considering the patient who initially rejects his analyst's construction. Problems of interpretation remain regardless of whether the patient persists in his rejection or eventually comes to accept the interpretation, since Habermas notes that both rejection and acceptance are equivocal. A rejection may "be the expression of legitimate dissent" while an acceptance may "be meaningless, or can even be described as 'hypocritical'."45

Consider first the patient who in time accepts the interpretation. The mere temporal order (initial rejection followed by acceptance) does not seem to be condition enough to guarantee that the acceptance is not hypocritical. How, then, is a genuine acceptance to be recognized, either by the patient himself or by the analyst?

Related to this central question are a number of queries concerning the mechanisms of coming to accept an interpretation. What is it that brings about the patient's change of heart? What role does the analyst play in this change? Habermas makes it clear that there are some techniques which a Freudian analyst sometimes employs in bringing his patient to accept an interpretation. How can the analyst do this and still avoid the charge that he is manipulating his patient? How are legitimate therapeutic techniques distinguishable from illegitimate ones?

Christopher Nichols focuses his attention on the patient who persistently rejects a construction in order to delineate the dilemma con-
fronting Habermas. He pinpoints the dilemma in Habermas' s attribution to the patient of ultimate epistemological authority concerning his own situation.

[1]f a psychoanalyst were not to persist with certain 'context-free' interpretations of new patients even when those patients resisted his interpretations, then it would seem that the analytic process would get nowhere. Pushed to the extreme, Habermas' s critique would entail the possibility that some patients do not have an Oedipus complex or that some psychotic patients are God, don't exist or still reside in the womb.

From the epistemological viewpoint, granting the patient veto power over constructions forces us to accept his interpretation of the situation; and from the therapeutic perspective, it forces us to abandon him.

Nichols continues on to suggest that the latter point indicates an unavoidable limitation of psychoanalysis in therapeutic application, but that the former is unacceptable and should be avoided by denying the epistemological authority of the patient.

Freud clearly appreciated the fact that, in some circumstances, resistance by the patient to a clinical interpretation required him, as a therapist, to repeat it even when rejected—and this does not mean, as could be stated in Habermas' s defense, that the patient must eventually come to accept an interpretation. It might mean the patient is unanalyzable, or unanalyzable in certain areas, whereas the majority of patients can achieve an awareness of themselves in a way that others cannot.47

The core of Nichols' s constructive criticism is the recommendation that Habermas should abandon his more dramatic pronouncements concerning the methodological peculiarity of psychoanalysis. This can be done by distinguishing more clearly its therapeutic and theoretical elements. Nichols calls on Habermas to recognize in particular that sometimes
the psychoanalytic construction of a patient's case must be taken as correct even if the patient himself never comes to accept it. On Nichols's view, Freud is right to hold that a particular construction derived from a general interpretation, which has itself been tested and corroborated, must stand even in the face of the patient's steadfast rejection. This is the limiting case where psychoanalysis provides a true diagnosis, but is unable to help the patient therapeutically.

Can Habermas accept this solution to the dilemma he faces here? In order to answer this question, we should recall how the dilemma arises.

Habermas wants to demonstrate both that psychoanalysis is deserving of the honorific "science," and that it is a unique science. His reason for insisting on its scientific status is to underwrite its claim to provide objective standards of criticism. Only if it is a critical science can psychoanalysis avoid the charge of relativism. He argues for the uniqueness of this critical science to avoid twin dangers: if it were a science like physics, it would lead to new techniques of domination, while if it were a hermeneutic science, its critique would be limited by the patient's self-understanding. In Knowledge and Human Interests the former danger appears as the more threatening. Granting ultimate epistemological authority to the patient prevents analysis, in its application from becoming a new dominating technique. It offers specific protection against behavioristic models in which both the validity of an explanation and the success of therapy are determined by seeing whether the patient can be made to behave in a manner chosen in advance by the therapist alone. It thus appears that Habermas would resist abandoning the patient's epistemological authority if doing so would put the patient in jeopardy of being dominated through a new
Protection could be afforded the patient by granting to him a veto power over the therapeutic goals of the psychoanalytic encounter. The patient could not be dominated if it could be shown that psychoanalysis must aim at a goal which he and the analyst recognize as correct, and one whose achievement both can recognize.

Once our attention is focused on the goal of psychoanalysis, it is easier to see how it can be a science and still avoid a technological application of its findings. The truth of general interpretations, like the truth of general theories, is to be tested against the results achieved in particular cases. An important difference comes in with the specification of what counts as a successful outcome. In physics success is defined simply as the occurrence of the predicted result. However on Habermas's account, success in psychoanalytical intervention must be defined as the patient's achievement of self-reflection. This difference in the specifications of success does not preclude the existence of public criteria of success. It must be possible to determine intersubjectively whether self-reflection has occurred. Focus on this goal also gives prima facie support to Habermas's oft repeated contention that in its application to particular cases analysis must reject certain manipulative techniques. Before any technique can be allowed into the analyst's arsenal it must be shown to assist the patient in achieving self-understanding. It is at best unclear how behavior modification or psychopharmacology could 'lead to this end.'

Locating the uniqueness of psychoanalysis in its goal—which must be shared by, and recognizable to, the patient and his analyst—raises novel issues for Habermas. The second condition requires that the end
aimed for should be specifiable with enough precision to allow public criteria of its achievement to be formulated. In Habermas's model, the goal of psychoanalysis is individual emancipation through self-reflection. He uses Freud's structural model to explicate this.

During the course of an individual's development, traumatic events may cause certain acts to fall outside of his conscious control; that is, to pass from the control of the ego into the control of the id. Psychoanalysis seeks to dissolve the causation of acts by the unconscious and return control to the ego:

In technical control over nature we get nature to work for us through our knowledge of causal connections. Analytic insight, however, affects the causality of the unconscious as such. Psychoanalytic therapy is not based, like somatic medicine, which is "causal" in a narrower sense, on making use of known causal connections. Rather, it owes its efficacy to overcoming causal connections themselves.49

A natural extrapolation from this is that Habermas sees the goal of the analytic situation to be the elimination of all unconscious causal control over the patient's acts. On this view, analysis strives to produce an individual who is in conscious control of all his own actions.

Nichols argues that such a goal is chimerical, and that Freud himself saw it to be so. Moreover, he argues that it seriously misrepresents Freud's perception of analysis's more limited goals by ignoring his understanding of the power of biological instincts.50

Nicholas's criticism makes it clear that Habermas must present a more carefully defined picture of the goals than he does in Knowledge and Human Interests. Even if we assume that it is possible to do this, that he can clearly describe the state of emancipation toward which psychoanalysis arrives, serious questions remain concerning his account.
We noted above that psychoanalysis could be prevented from becoming a new dominating technology if the patient retained an indefeasible voice in setting the goal of analysis. The goal of emancipation which Habermas postulates is an attractive one; one which patients as well as analysts might wish to pursue. However, Habermas advances a far stronger case for this particular goal. It is not simply one which might be chosen, rather it is one which must be chosen. This raises for him the problem of demonstrating that anyone ought to pursue the goal. Since psychoanalysis is necessarily a joint undertaking, two questions must be asked: Why should the patient first enter into and then remain in, analysis? Why should the analyst try to bring his patient to self-understanding rather than manipulate his behavior to achieve other ends?

If we assume with Habermas that the patient's reason for seeking help is the alleviation of his suffering, then it is unclear why he should be dissatisfied with any technique—including psychopharmacological ones—that provide symptomatic relief. Similarly from the physician's perspective, it remains unclear why he should try "to prevent the patient from prematurely replacing his symptoms with painless substitute-gratification."
Notes to Chapter V

1 "Knowledge and Human Interests: A General Perspective", inaugural lecture at University of Frankfurt, reprinted as the appendix to Knowledge and Human Interests, Jeremy J. Shapiro, trans. (Boston: Beacon Press, 1971), p. 308.


4 The Logic of Scientific Discovery, p. 104.


6 Ibid., p. 154.


8 Ibid., p. 207.


10 Thomas McCarthy, Habermas's sympathetic expositor, writes in his excellent commentary that Habermas "has not yet updated his general statement in the light of the most recent discussions of the development of scientific knowledge (by Kuhn, Popper, Lakatos, Toulmin, Feyerabend and others)." McCarthy then suggests that "this somewhat altered state-of-the-problem might provide equally fertile ground for developing Habermas's idea of a quasi-transcendental approach to the philosophy of science." The Critical Theory of Jürgen Habermas (Cambridge, MA: The MIT Press, 1978), p. 61.

11 "Reflections on My Critics", p. 264.

12 This defense, with slightly altered sets of criteria, appears both in "Reflections on My Critics," ibid., and in the "Postscript" added to the second edition of The Structure of Scientific Revolutions, p. 206.
13 Popper, "Three Views Concerning Knowledge" in Conjectures and Refutations, pp. 111-114. A clarification of Habermas's pragmatism is particularly important because some of the most trenchant attacks on pragmatism are found in the writings of the earlier generation of critical theorists. See, for instance, Horkheimer's Eclipse of Reason (New York: Oxford University Press, 1947).


16 "Knowledge and Human Interests: A General Perspective," p. 310.

17 In his later writings Popper gives up this commitment to understanding and explaining science's real practice, and embraces in its stead an idealized model of how science should be practiced. Part of this conversion involves replacing the scrutiny of real scientific communications with a study of a logically sanitized version. See his Objective Knowledge.


19 Ibid., pp. 58-59.


26 Ibid.


28 Against Method, pp. 303-304.

29 "Democracy, Elitism and Scientific Method," p. 11.

30 This is most forcefully argued in Science in a Free Society, pp. 16-24.

31 Ibid., p. 30.


33 Science in a Free Society, p. 80.

34 Against Method, p. 299.


36 Habermas's interpretation of Freud is developed in Chapters Ten and Eleven of Knowledge and Human Interests; the latter chapter is devoted to Freud's misunderstanding of his project. The second emancipatory science is, of course, Marx's social theory.

37 Knowledge and Human Interests, p. 217.

38 Ibid., p. 218.


41 Knowledge and Human Interests, p. 263.

42 Ibid., p. 261. Italics added.
43. Ibid., p. 265.

44. Ibid., pp. 266–267.

45. Freud quoted in Knowledge and Human Interests, pp. 267 & 269.


47. Ibid.


49. Ibid., p. 271.


51. Knowledge and Human Interests, p. 234.
CHAPTER VI

The task of establishing emancipation as the proper goal of psychoanalysis—as its constitutive interest—returns us to the central issue of Habermas's critical theory. How can it be shown that the three knowledge-constitutive interests he postulates ought to be pursued?

Once having rejected the view that constitutive interests are merely goals which actual individuals (e.g., a psychoanalyst and his patient) choose, there is one obvious move Habermas could make. He could join the major philosophical movement whose members argue that one tests the rightness of goals and norms by seeing whether they would be agreed to in free discussions. The key idea here is that a goal is shown to be right if it can be shown that it would be accepted by consensus. Habermas accepts much of this view, but he does not accept it in its entirety. More specifically, he apparently does not accept it as a way of justifying the knowledge-constitutive interests. The easiest way to see this is by returning for the final time to his critique of Popper's methodology.

1. Habermas and Popper

There is an area of substantial agreement between Popper and Habermas concerning the central role of consensus formation in science, in politics. Habermas accepts Popper's early characterization of science as a goal-directed social enterprise. In the scientific debate one tries either to produce an argument which will convince his opponents of their errors or show his own position to be wrong. Thus an intermediate goal in science is to establish a consensus. Both philosophers recognize that a variety
of strategies, such as deceit and indoctrination, can be used to bring about a consensus in any community, including the scientific. They therefore see the necessity of distinguishing proper means of achieving consensus from improper ones. Popper's methodological writings can be viewed as an attempt to make this distinction for the sciences. Those practices he decries as "unscientific" are stratagems for manipulating consensus formation, while his rules of scientific method describe proper formation.

Popper and Habermas begin to part ways over the value of any scientific consensus attained. Although his methodology spells out the rules which should be followed in settling scientific disputes, Popper remains deeply distrustful of any solution achieved. On his view, any actual consensus on a scientific matter, even one achieved despite the most scrupulous adherence to his methodological prescriptions, may be mistaken. An entire community of Newtonian physicists can be wrong, even if they have committed no methodological sins.

Popper envisages discussions in politics as being similar to debates over scientific matters. The only solution to a social or political problem which he considers even tentatively acceptable is one which can be criticized by all affected parties and which is nonetheless supported by consensus. So he suggests that the way to settle practical issues is through a free and open discussion in which all can participate.

In his own investigations of politics Habermas takes his turn at being skeptical about any actual consensus. The mere existence of a consensus over a social goal to be pursued, or norms to be obeyed does not guarantee the rightness of the goal or norms. There exist various illegitimate means of bringing about a political consensus, just as there exist means of manipulating a scientific debate. The use of force to suppress
dissidents and the official banning of some views from public debate are among the means used more commonly and prominently in politics than in science. Thus Habermas contends that not all existent political consensuses are acceptable, since an agreement achieved through the use of illegitimate techniques cannot itself be considered legitimate.

Both of these philosophers recognize, as the first task of their respective theories, the necessity of formulating and justifying criteria for distinguishing the genuine consensus. And both attempt to do this by stipulating conditions for the formation of consensus. Any consensus achieved in the correct manner deserves respect. But there is a fundamental disagreement between Habermas and Popper over the evaluation of the properly formed consensus.

Building on the history of consensuses in science now thought to have been mistaken, Popper rejects the idea that a properly achieved consensus is any indication of the truth of a theory. He drives deeper the wedge between the proper method for creating a scientific consensus and the proper method for attaining truth by his adoption of the correspondence theory of truth. His methodology tells us what moves are possible in establishing a consensus; but it remains ever possible that the theory, accepted in a properly constituted consensus does not correspond to reality.

Habermas sees a major embarrassment lurking here for Popper's version of critical rationalism. Popper can give scientists no rational account of why they should use the method of conjectures and refutations. His critical rationalism projects science as the epitome of rational goal-directed behavior and proclaims its goal to be the discovery of truth, or the elimination of falsehood. When Popper then prescribes appropriate scientific behavior one reasonably expects him to demonstrate that by behaving appro-
priately scientists will achieve their goal. This expectation, however, goes unrewarded. Assuming that scientists are simply seekers after the true description of the world, Popper gives them no reason to become members of the ideal scientific community he describes.

There is a more important criticism Habermas makes against Popper. Popper is self-admittedly unable to give any reasons why someone who is not already committed to the pursuit of truth should undertake this commitment. The choice of goals is, for him, beyond the pale of rational discussion and consensus formation. Even if there were an infallible criterion of truth, the decision to seek truth would remain an irrational one. Popper describes the scientist as the rare individual who is bound to the rationalist attitude of treating argument and experience as important; and then he writes of that attitude:

[N]either logical argument nor experience can establish the rationalist attitude; for only those who are ready to consider argument or experience, and who have therefore adopted this attitude already, will be impressed by them. That is to say, a rationalist attitude must be first adopted if any argument or experience is to be effective, and it cannot therefore be based upon argument or experience...

But this means that whoever adopts the rationalist attitude does so because he has adopted, consciously or unconsciously, some proposal, or decision, or belief, or behavior: an adoption which may be called 'irrational'. Whether this adoption is tentative or leads to settled habit, we may describe it as irrational faith in reason.

These difficulties are fairly obvious; and Popper perceives them, at least dimly. He notes, for instance, that "you cannot have a reasonable discussion with a man who prefers shooting you to being convinced by you." His way of dealing with this problem is to suggest an exclusion principle: one need not include those who do not have an attitude of reasonableness in discussions aimed at solving political issues.

Despite the initial plausibility of Popper's solution, there is a
general danger in the suggestion that some humans may be excluded from a forum which is to choose universal norms. It is a danger we brought to light in studying Lakatos's use of scientific consensus to determine what factual theories it is rational to believe. After admitting that it is impossible to demonstrate the irrationality of sticking with a degenerating research programme, Lakatos suggested that a consensus of scientists could be established favoring a progressive programme by the simple expedient of excluding recalcitrant researchers from the scientific community. The danger, of course, is that exclusion criteria can be arbitrarily manipulated to insure that a particular decision is taken by consensus.

The danger can be avoided by clearly specifying exclusion criteria and arguing for their acceptability. This, however, Popper does not even attempt. His contention that one cannot argue with an armed man is plausible; but in other places he strongly suggests that even critical theorists like Habermas lack the requisite attitude of reasonableness, and may therefore be excluded from discussions.

Despite his serious misgivings about the value of any extant consensus, Habermas accepts consensus as the only criterion we have of the truth of a scientific theory or the rightness of a solution to a political problem. These misgivings lead him to reject any pretense that the consensus existing at any given moment is the criterion of the true or the right; rather, the consensus which is important is much closer to the Peircean notion of "the opinion which is fated to be ultimately agreed to." It is an ideal consensus which interests him.

His reliance on consensus to avoid the embarrassments of Popper's critical rationalism and his concomitant mistrust of existent consensus
motivate Habermas's recent interest in the theory of communications. Out of this communications theory is to come the description of the ideal consensus.

Before embarking on an investigation of the substantive part of the theory of communicative competence it is important to review the tasks. Habermas hopes to fulfill with it. In the first place, he needs to show that the existence of a consensus is a suitable warrant for the rational acceptance of a factual assertion or a norm. Since he does not believe that just any consensus is a warrant of acceptability, his second task is to specify and justify criteria for identifying a genuine consensus. Included as part of the second task is the delimitation of those who should be included as participants in the discussions aimed at consensus formation. Finally, to avoid Popperian irrationality, he must show that all of the potential participants in these discussions ought, rationally, to participate in them.

ii. Problems with the Consensus

The centerpiece of Habermas's program of describing and defending the ideal consensus is his theory of communicative competence. This theory focuses on the form of social action he takes to be fundamental—interpersonal communication aimed at reaching understanding—and then strives to elucidate the "general and unavoidable presuppositions of possible processes of understanding." The starting point in the development of this theory is the observation that "competent orators know that every consensus obtained can in fact be deceptive; but they must always have been in possession of the prior concept of the rational consensus underlying the concept of a deceptive (or merely compulsory) consensus." Habermas takes this
observation a step further by claiming, in essence, that all normal
speakers are competent orators. "Reaching an understanding is a norma-
tive concept; everyone who speaks a natural language has intuitive know-
ledge of it and therefore is confident of being able, in principle, to
distinguish a true consensus from a false one.\(^5\) This sets the goal of
his theory of communicative competence: it seeks "to clarify the norma-
tive implications that lie in the concept of possible understanding, with
which every speaker (and hearer) is naively familiar."\(^6\)

The theory of communicative competence, like Noam Chomsky's narrower
type of linguistic competence, is a reconstructive one. It attempts to
explicitly reconstruct those rules which ordinary speakers implicitly follow
in their communication with one another. The aim of faithful reconstruc-
tion is taken by Habermas to dictate the methodology of reconstructive
sciences. The primary distinguishing feature is the method of testing pos-
tulated reconstructions. On Habermas's view, reconstructions of the rules
which underlie a practice must be tested against the intuitions of competent
practitioners. This means that the rules of communication he formulates
must be tested by asking normal speakers of a natural language whether the
rules accurately reflect their practice. We will return to the issue of
testing after we have outlined the theory Habermas puts forward.

The first part of the theory is a reconstruction and enumeration of
those elements without which communication would prove impossible. Habermas
describes the findings of this part of his investigations as follows:

I shall develop the thesis that anyone acting communicatively
must, in performing any speech action, raise universal valid-
dity claims and suppose that they can be vindicated. Inso-
far as he wants to participate in a process of reaching un-
derstanding, he cannot avoid raising the following—and in-
deed precisely the following—validity claims. He claims
to be:
a. Uttering something understandably;
b. Giving [the hearer] something to understand;
c. Making himself thereby understandable; and

d. Coming to an understanding with another person.

The speaker must choose a comprehensible expression so that speaker and hearer can understand one another. The speaker must have the intention of communicating a true proposition... so the hearer can share the knowledge of the speaker. The speaker must want to express his intentions truthfully so that the hearer can believe the utterance of the speaker (can trust him). Finally, the speaker must choose an utterance that is right so that the hearer can accept the utterance and speaker and hearer can agree with one another in the utterance with respect to a recognized normative background. Moreover, communicative action can continue undisturbed only as long as participants suppose that the validity claims they reciprocally raise are justified.7

Taken together, these varied elements yield a sketch of the ideal speech situation. In ideal speech the speaker claims that his utterance is grammatical, that it represents a fact and that it corresponds to what he intends to express. Moreover, he must also believe that each of these claims can be justified. The hearer, while he realizes that he may challenge any one of these validity claims, shares the speaker's belief that all of them can be justified. Finally, in the ideal speech situation the two parties recognize that their roles are perfectly reciprocal; that at any time the hearer may become speaker and vice versa.

Habermas contends that everyday communication implicitly contains the ideal speech situation in the sense that every competent user of a natural language knows how to make and to accept all four validity claims, and knows that communication cannot take place if they are not made and accepted. Actual practice, however, falls short of this idealized reconstruction in which the claims are justified and unchallenged. Habermas readily recognizes that the discussions aimed at producing agreement in everyday life are not instances of ideal speech. He notes that "typical states [of ordinary communication] are in the gray areas in between; on the one hand,
incomprehension and misunderstanding, intentional and involuntary untruthfulness, concealed and open discord; and, on the other, pre-existing or achieved consensus."8

There are several ways in which actual practice falls short of the ideal. If communication is to be possible, the speaker must make the validity claims and his hearer must assume that they are all justifiably made. The hearer remains free to question the justifiability of any one of them, and once he does this communication stops. At this juncture three ways lie open: the attempt at communication may simply be broken off, with the erstwhile communicants going their separate ways; either (or both) of the parties can undertake a different form of social action which Habermas labels "strategic action"; or the parties can jointly attempt to re-establish communication by invoking one of the various procedures for justifying disputed claims.

A direct challenge to a validity claim stops communication until the challenge is met in a way satisfactory to both parties. However direct challenge is not the only way in which genuine communication can be disrupted. For it remains possible that two communicants will continue a dialogue that falls short of genuine communication—a dialogue in which all four validity claims cannot be justified despite the participants' assumption, or pretension, that they can be. An obvious example of this phenomenon is the successful lie. In this instance the speaker must implicitly claim that he is expressing his intentions truthfully and, if the lie is successful, his hearer must accept the claim. Although there is no obvious, public rupture in the communication here (the dialogue continues), Habermas argues that this is not a case of genuine communication. Less obvious, but no less important, examples are those in which both
speaker and hearer assume that a true proposition has been uttered or that a rightful norm is in place, but where these validity assumptions could not be sustained were a challenge raised.

The fact that ordinary discussions cannot be classified as ideal speech does not render the idealization unimportant. It provides the competent speaker with a benchmark for judging any factually existing consensus or actual discussion. It is his knowledge of the ideal speech situation which enables the competent orator "to distinguish a true consensus from a false one." Thus his knowledge of the ideal tells the normal speaker when a validity claim should be challenged.

At this point Habermas begins to make bolder claims for the theory of communicative competence. He says that it reveals not only that competent speakers are able to recognize the ideal speech situation, but also that they know how to construct the ideal. While communicative competence "does not itself amount to a capacity actually to establish the ideal speech situation," it "does mean the mastery of the means of construction for the establishment of an ideal speech situation."9 The competent speaker knows how to attain the ideal, although the structures of the social system to which he belongs may prevent him from doing so.

The second part of Habermas's theory thus shifts attention away from the ideal speech situation, which is characterized by consensus, and onto the processes for bringing about agreement. This part of his theory remains reconstructive. In the quite ordinary conversations of everyday life there are ways of discovering problematic validity claims (e.g., suspected lies, or dubious assertions) as well as means for the resolution of disputes over them. The second part of the theory has as its task the reconstruction of these existing techniques for establishing
or re-establishing consensus.

We have seen that disputes can arise in ordinary communication when any one of the four validity claims is challenged by the speaker's audience. Given the diversity of the claims, it seems that different ways of recognizing when a challenge should be raised and different methods of dispute resolution may well be needed for each of them. As we shall now see, Habermas tacitly admits that this is at least partially correct. He integrates his earlier work into this part of the theory of communicative competence by arguing that the scientific testing of hypotheses is exemplary of the method for settling disputes over one of the validity claims while the psychoanalytic encounter is the means for resolving disputes over another. To date, however, his catalogue of methods for resolving disputes over validity claims remains incomplete.

The first validity claim, that of comprehensibility, refers to the speaker's ability to produce a sentence which is understandable by his listeners. All that is required for a sentence to be mutually comprehensible is that both speaker and listener have mastered the public rules of language.

A speaker may fail to produce comprehensible utterances for at least two reasons. He may simply be like the foreigner who is still learning a second language. He may not yet have assimilated enough vocabulary nor have mastered the techniques of assembling grammatically correct sentences. This case is not particularly interesting to Habermas, and he is content to leave its analysis in the hands of linguists. The incomprehensible utterances which attract his sustained attention are those wherein the incomprehensibility is systematic.

We mentioned earlier that the psychoanalyst is interested in
understanding and explaining a person's utterances which are incomprehensible to one remaining within the confines of everyday communication. Using the theory of communications it is possible to describe more precisely the cases that psychoanalysis treats.

Habermas says that the psychoanalyst must begin with "a pre-conception of the structure of undistorted everyday communication." This preconception is one which he originally has as a competent speaker and which can be more clearly reformulated through reflection on this competence. It contains two elements which are particularly important to our current interest in diagnostics:

a) In non-deformed language games there exists a congruence of expression on all three levels of communication; those utterances symbolized linguistically, those that are presented in actions, and those embodied in physical expressions do not contradict but complement one another...
b) Normal everyday language follows intersubjectively valid rules: it is public.

Conditions which cry out for psychoanalytic treatment are identified against this background.

When we consider the system of distorted communication as a whole it becomes apparent that there exists a characteristic discrepancy between the levels of communication: the usual congruence between linguistic symbolic, actions and accompanying expressions has disintegrated. Neurotic symptoms are merely the most stubborn and manifest evidence of this dissonance. No matter on what level of communication these symptoms appear—in linguistic expression, body-language, or compulsive behavior—it is always the case that a content, which has been excommunicated from its public usage, assumes independence. This content expresses an intention which remains incomprehensible according to the rules of public communication, and is, in this sense privatized; but it also remains inaccessible to its author.

Turning to the etiology of these pathological conditions, Habermas uses the notion of communicative competence to sketch a reinterpretation of Freudian metapsychology. He begins with the commonplace that one
must become a competent speaker: one must learn to use a language. Learning a first language involves far more than developing the ability to create well-formed sentences (i.e., linguistic competence). It is, for Habermas, a process of socialization in which one must also learn how to properly connect utterances with bodily expressions and with actions in order to express himself publicly. This process of socialization, however, can be disturbed. It is disturbed if the child faces an "intolerable conflict" from which he protects himself by means of repression. The result of the disturbance and associated repression is the appearance of systematically distorted communication.

This defense is connected with a process of desymbolization and of symptom-formation. The child excludes the experience of conflictive object-relations from public communication...it splits off the part of the representation of the object that is charged with conflict and, in a way, desymbolizes the meaning of the relevant object. The gap that appears in the semantic field is closed by a symptom, in that an unsuspicous symbol takes the place of the symbolic content that has been split off.13

Metapsychology seeks to discover the conflicts to which the child is susceptible, and to explain which conflicts produce typical symptomatic patterns of distorted speech.

Habermas's linguistic reformulation of psychoanalysis goes part of the way toward resolving some of the quandaries left over from the last chapter. There, we argued that the validity of his contention that psychoanalysis is a unique science turns on saying that its goal is unique. The twin problems of accurately specifying the goal and of justifying the claim that it is the proper goal of analysis arose at this point. Psychoanalysis can now be interpreted as the attempt to re-integrate split-off symbolic contents, which led to a privatistic narrowing of public communication, into the general usage of language. Analysis helps to achieve a 'resym
bolization’ by retracing, and thereby undoing, the process of repression”.\textsuperscript{14} And on this reinterpretation this goal is to be justified by reference to the speech situation. All speakers of a language, simply as speakers, seek to make themselves understood by their audience. Repression prevents the individual from attaining this by causing him to produce statements which are not publicly understandable. Insofar as the patient wishes to take part in human communication, he must desire to remove these impediments to public understanding.

When the second of the validity claims is disputed—that is, when the hearer doubts the truth of the proposition asserted—the parties may continue it at a different level. The communicants must concentrate on examining the problematic claim with the intent of re-establishing an agreement. This peculiar form of discussion Habermas calls the discourse.

Discourses help test the truth claims of opinions (and norms) which the speakers no longer take for granted. In a discourse, the ‘force’ of the argument is the only permissible compulsion, whereas the co-operative search for truth is the only permissible motive....The output of discourses...consists in recognition or rejection of problematic truth claims.\textsuperscript{15}

Habermas highlights two crucial aspects of discourse. Only when the need to act can be suspended does a discourse take place. This is important because it helps insure that the only motive any of the participants has is the "cooperative readiness to arrive at an understanding."\textsuperscript{16} The second important feature is that the discourse is, in multiple sense, an unrestrained discussion. In discourse no opinion is immune from criticism, nor is there reason to ignore any criticism since freedom from the constraints imposed by a need to act allows discourse to be temporarily unlimited. All parties to the discourse have equal opportunity to take up and defend, or to criticize any opinion. There is no prejudice toward
positions or toward parties.

This very brief depiction of the discourse is strongly reminiscent of the Popperian picture of the scientific debate. The unadulterated goal of deciding whether a claim is true, the acceptability of argumentation alone and the freedom of anyone to lodge criticism are all prominent in Popper's description of science. This similarity is not coincidental: Habermas interprets science as society's most formally institutionalized discourse. Nor should the similarity be surprising, since we have already seen that Habermas and Popper share a deep commitment to developing a normative methodology. This commitment drives both to try describing the proper forum for generating a consensus.

If there is a major difference here, it is that Habermas, unlike many methodologists, does not view the methods of discourse as a discovery of modern science or as the exclusive property of the scientific tradition. Rather, the scientific discourse is but a formalized version of the method of testing which is already a part of ordinary communicative practice. The scientific debate is therefore an interesting practice for analysis not because it contains conventions so different from those found in everyday life, but because it contains the same conventions in a more consciously reflected form.

The importance of this difference for Habermas can be seen by delineating the structure of his argument. The claim that a statement is true is to be justified by showing that that statement would be the object of a genuine consensus. Whether a consensus is genuine is judged, in its turn, according to the structure of the forum in which it arises. If the consensus is reached in a discourse, then the statement is true. This seems to shift the really crucial issue back one step: how can it be
shown that a particular forum—the discourse—is the proper one for discovering truth?

The critical rationalists face the same question. Many of them respond by arguing that a methodology is shown to be proper by showing that using it leads to truth. But as Lakatos noted, unless one has an independent criterion of truth, the argument fails. Habermas takes a different tack here. Instead of justifying the discourse pragmatically by showing its success, he justifies it by showing that it is patterned after an ideal which all speakers of natural languages must share. On his view, anyone who has developed communicative competence must recognize the discourse as the proper forum for settling factual or normative disputes.

The most contentious claim Habermas makes for the discourse is that it provides the model for settling normative issues as well as factual ones. Alongside the scientific discourse he places a normative discourse. This claim is far stronger than Popper’s similar one that the scientific debate should serve as the model for political and ethical discussions.

According to Popper, each individual embraces a diverse bundle of values and pursues various goals. These values and goals are beyond the reach of rational criticism in an important sense. No argument can be given to rationally motivate all individuals to adopt any one value or set of values. Indeed, there is no way of motivating all to accept the importance of rational argumentation itself. As a consequence, we cannot count on everyone’s acceptance of any given interest: whatever agreement exists is merely the result of fortuitous coincidence.

Habermas acknowledges that there are particular interests over which no consensus will be reached. It would seem that where such particular
interests are concerned the most a public discussion can do is to perform those functions attributed to it by Weber (and by Popper): it can investigate the logical consistency of a set of values, and it can show that cost of pursuing any given goal.

What Habermas denies is that these particular interests are the only extant interests. His central contention is that there also exists a set of generalized interests—interests shared by all— which are distinguishable from, and irreducible to, the particular interests of identifiable individuals. It is the set of generalized interests which the normative discourse serves both to identify and to legitimate.

Showing that the discourse can perform the first of these functions, that it allows for the identification of generalized norms where these exist, is straightforward. It is only in confrontation with others that one can discover whether his own interests are completely idiosyncratic, shared by some others, or shared by all. A public forum is necessary for testing the factual belief that one's interests are widespread; and the discourse situation Habermas describes is the perfect forum since it is open in principle to all speakers of a natural language.

The claim that any norm found through discursive testing to be generally held must be considered legitimate is also defended by appeal to the ideal structure of the discourse. This structure, he argues, removes all of the forces which distort communication in everyday life. It allows each individual to offer those interests he pursues as candidates to be general interests. The structure also insures that every participant is free to question any such proposal and to challenge all proffered arguments for it.

If under these conditions a consensus about the recommendation to accept a norm arises argumentatively, that
is, on the basis of hypothetically proposed, alternative justifications, then this consensus expresses a "rational will"... The discursively formed will may be called "rational" because the formal properties of discourse and of the deliberative situation sufficiently guarantee that a consensus can arise only through appropriately interpreted, generalizable interests, by which I mean needs that can be communicatively shared.17

Our account of the critical rationalist program has isolated the major impediment to this strategy of justifying norms by describing a forum for their discussion. Popper uses just this strategy, and on his account, even if we discover the proper method of holding a discourse, any prospective participant must make a decision to enter into the discussion and be bound by its rules. On his view, one can refuse to participate without being branded irrational.

It is clear from his account of when discourses can occur that Habermas too must grapple with this problem. Whenever a validity claim is challenged in everyday discussion each participant must choose from among the alternatives of breaking off communication altogether, engaging in a different form of action, or continuing communicative action at the level of the discourse. For his account to be superior to Popper's, Habermas must demonstrate that the rational thing to do in the situation is to pursue the third way.

Habermas attempts to foreclose the first of the alternative ways by noting that the complete rupture in communications it calls for can be achieved only at an unacceptably high cost.

Anyone who does not participate, or is not ready to participate in argumentation stands nevertheless "already" in contexts of communicative action. In doing so, he has already naively recognized the validity claim—however counterfactually raised—that are contained in speech acts and that can be redeemed only discursively. Otherwise he would have had to detach himself from the communicatively established game of everyday practice.18
The point he makes here is that, because the structure of the discourse is reconstructed from the rules implicit in everyday communication, one cannot reject wholesale the recourse to a discourse without at the same time giving up this everyday speech practice. And since man is a creature who struggles to survive through communicative action, the wholesale rejection means severing oneself from the human community's primary project.

While not giving a detailed exposition, Habermas suggests that the second alternative is to be foreclosed in a similar fashion—by showing that a wholesale adoption of it would lead to completely unacceptable consequences. In this vein he notes "It is absurd to imagine that a subject capable of speech and action could permanently realize the limit case of communicative action, that is, the monological role of acting instrumentally and strategically, without losing his identity."\textsuperscript{19}

There is one further instance of disputed validity claim Habermas touches upon, and that is the claim of authenticity. Inauthenticity is suspected if there is a discrepancy among the speaker's speech and actions or bodily expressions. Unlike the individual whose development is disturbed, the liar knows how to create a congruence among the three. In fact, he seeks to establish congruence in order to deceive the listener about his intentions. This makes it more difficult to recognize the lie. As Habermas notes: "The claim to authenticity can only be realized in interaction: in the interaction it will be shown in time, whether the other side is 'in truth or honestly' participating or is only pretending to engage in communicative action and is in fact behaving strategically."\textsuperscript{20}

Having noted how the lie can be detected in everyday speech and how difficult the detection is in practice, Habermas does nothing to show how the rent in communication caused by the lie and its detection is to be mended.
iii. Emancipatory Science

Habermas originally developed his model of an emancipatory science to serve two purposes. In the first place, he wanted to show that genuine knowledge of universal norms is possible; thereby combating the irrationalism hidden in the foundations of Popper's critical rationalism and the related relativism at the surface of the defense of alternative traditions. In the second, he wanted to produce a theory which could be practically effective in bringing about the concrete social change.

Our interest in Habermas's theory was piqued by its dual intents. We were looking for a method of critical inquiry into science which, unlike mainstream contemporary methodologies such as Popper's, seriously questioned the goals of the scientific enterprise. Feyerabend recognized the necessity and legitimacy of such questioning but, because he accepted the view that knowledge of universal norms is impossible, he was forced to argue for the proliferation of traditions with diverse goals. And this left us without any account of how the scientific tradition could be effectively controlled.

We must now see if our interest in Habermas's theory has been rewarded. That is, we must ask whether he has produced a social theory which both contains genuine knowledge of social norms and leads to social change. As we shall see, his critics have asked searching questions about both the theoretical and the practical sides of his theory.

Psychoanalysis is portrayed by Habermas as the prime example of an emancipatory science: analysis aims, not only at theoretical knowledge of the patient's repression, but also at his liberation from it. Moreover, he suggests that it is to serve as the model for the critical theory of society he wishes to construct. In order to test his theory we should
investigate the difficulties which beset a social theory built on the model of psychoanalysis.

At first sight it is difficult to imagine how psychoanalysis could possibly be useful as the model for a practical social theory. The disanalogies between therapeutic intervention and political action seem too stark to allow any common methodology. A number of commentators have found the dissimilarities to be so obvious and so telling that they have been tempted to dismiss Habermas's entire project. Clearly then, we must take some care in explicating how he perceives psychoanalysis as a practical model.

The first major disanalogy concerns the respective structure of the psychoanalytic encounter and of the typical political struggle. The reconstruction of psychoanalytic therapy by Habermas presents it as a two-person encounter with clearly defined and asymmetric roles for the analyst and the patient. This gives rise to a set of problems, since political conflict does not fit cleanly into such a pattern. We may assume that Habermas plans to cast the critical theorist in a role akin to the therapist's. Who then is his patient? If we envisage social conflict as a struggle between competing classes, the question becomes one of deciding which class is to be the patient.

An essential feature in the account of psychoanalysis is Habermas's proviso that the encounter must be freely entered into by the patient. The patient seeks out the analyst because he is suffering and desires relief. This suggests that the patient in social theory must be the oppressed class—the class which is suffering as the result of society's excessive repression. And Habermas agrees that this is one allowable interpretation of his model; the psychoanalytic model can be used, for
instance, "for normatively structuring the relationship between the Communist Party and the masses who let themselves be enlightened by the Party concerning their own situation." This interpretation, however, encounters a new problem when it is considered from the practical point of view. While developing the psychoanalytic model Habermas argued that genuine enlightenment itself produced significant change. But in social theory, since the locus of repression lies outside of patient, enlightenment may have no practical effect. Members of the oppressed class may accept the critical theorist's diagnosis of the situation without doing anything about it. Habermas admits this much and argues that a general theory of society should not be expected to provide techniques of social emancipation which can be used in all social situations.

Habermas goes on to note that this picture of the critical theorist as the therapist to the oppressed class is not the only one derivable from the psychoanalytic paradigm. To generate others it is necessary only to look more closely at the role of the analyst. The psychoanalyst in his model possesses two things. He has theoretical knowledge. That is, he has a validated theory of normal development (which includes a clear conception of the ideal state toward which this leads) and of the various forces that can affect this development. The second thing he possesses is a technique for counteracting the effects of the distorting forces.

Having denied that the critical theorist of society has a stock of techniques for effecting social change, Habermas concentrates his attention on the theoretical knowledge he possesses. "We must," he writes, "distinguish between the level of theoretical discourse and the organization of processes of enlightenment in which the theory is applied."
This distinction allows him to say that the critical theory (which is tested by the "usual procedures of scientific discourse") can be used "to derive an explanatory hypothesis, without having (or taking) the opportunity of initiating communication with those actually concerned." 24

The critical theorist's expertise is analogous to the psychoanalyst's knowledge of normal individual development and the forces which can influence this maturation. Just as the psychoanalyst knows the ideal stages of development the individual should attain at given ages, the critical social theorist knows the ideal stage of normative development a society should reach at given points of its economic development.

Habermas gives a sketch of how such theoretical knowledge is developed, and of how it can be used to diagnose, without treating, the distortions present in a society. The theory can provide the standard for evaluating social systems: it allows us to compare the "normative structure existing at a given time with the hypothetical state of a system of norms formed, ceteris paribus, discursively." It provides the standards of comparison by asking the question:

"How would the members of a social system, at a given stage in the development of productive forces, have collectively and bindingly interpreted their needs (and which norms would they have accepted as justified) if they could and would have decided on organization of social intercourse through discursive will-formation, with adequate knowledge of the limiting conditions and functional imperatives of their society?" 25

The thesis here is clearly that a consensus on social norms arrived at in forum approximating the ideal speech situation guarantees the rationality and the objectivity of these norms. And since this set of social norms is objective it can be used as the benchmark in criticizing existent norms. Critical theory thus has the practical effect of illuminating what changes should be made in a society even though it does not tell
one how these changes are to be effected.

The normative discourse plays the pivotal role in Habermas's system of generating and legitimizing the universal social norms which in turn serve as standards for existent norms. The description of the discourse's formal properties lists some features of the ideal speech situation which any forum must exhibit if the consensual decisions taken in it are to be considered rationally binding norms. This description of a social structure in which discussions occur does not exhaust the catalogue of conditions that must be satisfied for the discussions to constitute a normative discourse. This can be seen by asking who is to take part in this forum.26

It is tempting to suggest that all adult speakers should be counted among the participants in the discussions aimed at establishing norms. This suggestion, however, runs into serious objections. First, it is far from clear that any collection of actual adult people, with their diverse beliefs and special interests, will be able to reach an agreement over norms. An objection more telling to Habermas's project is raised by Roger Gottlieb:

In an ideal speech situation participants are bound only by the power of argument. But the ability to argue is itself a power, one unequally distributed in this society. This observation can be generalized to include the numerous ways in which people of different races, sexes, and economic classes are socialized to take unequal roles in a dialogue. Depending on one's sex, race or class, one is socialized to be active or passive, to act 'rationally' or 'emotionally'; to be aggressive or deferential, to conceive of oneself as an expert or nonexpert. And within our society there is a significantly and systematically unequal distribution of the training and expertise necessary for free dialogue: for example, of theoretical knowledge and organizational skills.27

Gottlieb's point is plain: even if we construct a situation allowing unconstrained discussion, populate it with ordinary people, and then
they manage to reach a consensus on norms, we have no guarantee that their consensus represents a universal rational will. The agreement may arise because of the rhetorical skills of some participants and the reticence of others, and both the skills and the reticence are the result of actual processes of conditioning.

Habermas is aware of these difficulties; indeed, his theories of psychoanalysis and of individual development implicitly recognize Gottlieb's point that not all people can participate actively in a discourse. His response is to accept the point concerning the actual socialization processes at work in the world, and then to sketch a picture of the ideal participant in the ideal speech situation.

It is relatively easy to negatively characterize the ideal participant against the backdrop Gottlieb paints. He (or she) is someone who has not been socialized to take an unequal role, act deferentially toward authorities, or to remain passive while decisions are being made. Rather he is someone who recognizes the equality of participants, knows how to evaluate and challenge the assertions made by experts, and is without any fear of doing this.

There is no doubt that Habermas recognizes the ideal status of any such description of the individual who has attained this level of autonomy. He writes:

The concept of ego identity obviously has more than a descriptive meaning. It describes a symbolic organization of the ego that lays claim, on the one hand, to being a universal ideal, since it is found in the structures of formative processes in general and makes possible optimal solutions to culturally invariant, recurring problems of action. On the other hand, an autonomous ego organization is by no means a regular occurrence, the result, say, of naturelike processes of maturation; in fact it is usually not attained.\textsuperscript{28}
Since individual autonomy is an ideal, moreover one which is rarely attained in everyday life, Habermas must contend with the suspicion that he has constructed the ideal to insure the agreement by consensus on predetermined norms where agreement on these norms is not forthcoming from real people.

Habermas meets this latest challenge by arguing that neither the description of the discourse's structure nor that of the individual who is to take part in it are constructed a priori. Rather, both descriptions are reconstructed out of the implicit presuppositions of actual speech, and as reconstructions, they are susceptible of empirical test.

The methodology of reconstructive studies has yet to receive the attention that has been afforded to other methods. Nonetheless several difficulties can be readily perceived in such studies. Some of these are of a general nature, methodological quandaries which seemingly beset all reconstructive sciences; while others are peculiar to studies, like Habermas's, which try to enunciate species competences. The former are evident in Lakatos's abortive rational reconstruction of the history of science.

Imre Lakatos portrays his methodology as a reconstruction of the rules working scientists implicitly follow in appraising articulated theories. And, like Habermas, he initially recommends that a reconstruction's accuracy be judged against the considered intuitions of competent users of the rules. For his methodology this means that the rules are acceptable only if they are recognized as correct by scientists.

Lakatos states that there are certain historical episodes which the members of the "scientific elite" agree in evaluating as landmark scientific achievements. As a first approximation, he suggests that
these evaluations serve as the scientists' intuitions in testing a reconstruction. Any reconstruction of the rules of science which shows these landmark episodes to be irrational should be considered inadequate.

"Let us propose tentatively that if a demarcation criterion is inconsistent with the 'basic' appraisals of the scientific elite, it should be rejected."²⁹

This suggestion is in trouble from the start (as Lakatos recognizes). Lakatos wants a description of method to use as a universal demarcation criterion; he wants a way to identify rational practice, one which is not open to the charge of relativism. But in his test for a proposed method he introduces the notion of a 'scientific elite', and this notion carries with it a suspicion of the relativism he most wants to avoid.

The members of the scientific elite are the competent rule-users whose intuitions justify the methodology. Lakatos, however, gives no way for selecting the members of this elite. If the elite is defined widely, so that it includes all members of the scientific community, for instance, the question of competence arises. Scientists pay little attention to the actual history of science either in the course of their education or in their working lives; so why should they be considered qualified to evaluate historical episodes? On the other hand, restriction of membership in the elite to those who seem competent by virtue of their special expertise in the history or methodology of science runs headlong into the problem of circularity. In either case Lakatos fails to show that there is some Archimedean point from which a reconstruction of scientific rules can be judged.

Habermas accepts the view that a reconstructive theory must be tested against the intuitions of practitioners. But he argues that his
theory of communicative competence avoids the kind of pitfalls which en-
snare Lakatos's reconstruction of scientific rationality. In the first
place, he avoids the difficulties inherent in specifying the membership
of an elite by denying that there is an elite which makes judgements.
Habermas assumes that most adults can be judges of the reconstructed rules
of communicative competence. The competent judge of his theory is anyone
who has mastered a language as a means of communication; and the adult
who has not done this is the exception rather than the norm. 30

Against the more general concern that one can be adept at a practice
without being able to articulate the rules of that practice or to criticize
articulations suggested by others, Habermas argues that the case of language
is unique. While it is possible to distinguish, for instance, scientific
practice from the discussion of the practice (so that an excellent scient-
tist may be a poor judge of methodologies), a similar distinction cannot
be made in linguistic communications.

Because of the reflexive character of natural languages,
speaking about what has been spoken, direct or indirect
mention of speech components, belongs to the normal
linguistic process of reaching understanding. The ex-
pression metalinguistic judgments in a natural language
about sentences of the same language suggests a difference
of level that does not exist. It is one of the interesting
features of natural languages that they can be used as
their own language of explication. 31

The cornerstone of Habermas's rejection of relativism is his conten-
tion that the competences he describes the autonomous individual as having,
are universal—that they are abilities which one must at least partially
master in order to speak a natural language. It is also particularly
crucial that he be able to show that ordinary speakers recognize the
ideal speech situation as the means of establishing the truth of factual
assertions and the rightness of norms. To vindicate these universality
contentions he needs empirical evidence gleaned from the intuitions of speakers of all natural languages.

The provisional and developing nature of Habermas's research project becomes most apparent here. To date he has done little more than set out, as an hypothesis, the structure of competences, and then indicate roughly how this hypothesis is to be tested. Since his is a reconstructive theory, it must ultimately be judged against the considered intuitions of competent speakers.

The necessity of demonstrating universality suggests a two-part schema for testing: Habermas's hypotheses should be tested first against the intuitions of speakers who are members of the Western rationalist tradition, and then against the intuitions of speakers from other traditions, including so-called primitive traditions. There are major problems with each part of this test.

Although Habermas has not personally produced data to support his theory, he suggests that corroborative data are available in the work produced by the developmental psychologists from Piaget's school. These studies have defined a series of stages of cognitive and moral development through which children are purported to progress on their way to adulthood. The important thing about these studies, for Habermas's purposes, is that their highest stages are closely parallel to his own description of the individual who has achieved an autonomous ego identity.

There are, however, serious issues involved in the evaluation of these studies and in Habermas's use of them. In the first place, the studies to not give support for the assertion that the ability to recognize the discourse or the autonomous individual as ideals is a universally distributed competence. As Thomas McCarthy notes, the developmental
psychologists so far have done too little research in non-Western traditions to allow any firm conclusions. Thus there is simply too little evidence to say that Habermas has uncovered species competences.

In the absence of any strong evidence in its support, a substantial skepticism concerning the universality claim is in order. This skepticism is expressed well by Raymond Geuss:

I find it quite hard to burden pre-dynastic Egyptians, ninth-century French serfs and early-twentieth-century Yanomamo tribesmen with the view that they are acting correctly if their action is based on a norm on which there would be universal consensus in an ideal speech situation. The notion that social institutions should be based on the free consent of those affected is a rather recent Western invention, but one which is now widely held. The notion that an action is acceptable or a belief 'true' if they would be the object of universal consensus under ideal conditions is an even more recent invention held perhaps by a couple of professional philosophers in Germany and the United States. The point is not that pre-dynastic Egyptians couldn't formulate the 'consensus theory of truth', but that we have no reason to think that they had any inclination to accept as legitimate only those social institutions on which they thought there would be universal consensus in ideal conditions.34

By assuming Geuss' skepticism to be well founded we can see how perilously close Habermas comes at this point to the relativism he strives so valiantly to defeat. Suppose that the primitive tribesman finds nothing familiar in a description of the ideal speech situation: that he does not intuitively recognize the discourse as the proper way of arriving at legitimate social norms. What conclusions must follow from such negative findings?

Habermas could accept negative results as a clear refutation of his theory. However if he does this, his only defense against relativism is destroyed. Alternatively, he could deny the importance of such a putative falsification by denying that the member of the primitive tradition is a
competent judge of the reconstruction.

The second alternative is undeniably plausible. Anthropologists have often used evidence of cross-cultural differences in beliefs about the world as evidence of one culture's primitiveness. And the developmental psychologists (on whose writings Habermas relies so heavily) often interpret the results of their cross-cultural investigations in a similar manner. Moreover, Habermas's own work on the historical development of social systems suggest strongly that he might entertain this alternative.

The dismissal of adverse empirical findings on the grounds that the members of some cultures are not competent judges of the reconstructions is nonetheless fraught with danger. The danger is the same one which confronted Lakatos's use of a reconstructive methodology to justify his theory of science. Once one begins to exclude the intuitions of the members of ill-defined groups from the reconstructive testing of an hypothesis, the suspicion inevitably arises that these people are being excluded because their intuitions do not support the hypothesis. It is not presently clear that Habermas could successfully allay this suspicion.

The paucity of relevant data makes it impossible to adequately assess the claim that competent speakers of all natural languages recognize the discourse as the proper way of arriving at binding social norms. Habermas faces a different problem when he tries to use the psychologists' data from Western societies to support his theory. These data show that the attainment of the highest stages by adults in these societies is not a common occurrence; the attainment of the highest stage is, in fact, quite rare. The fact that not all adult Westerners accept the discourse as the proper forum for arriving at binding norms can be seen very clearly in the criticisms of Habermas's program levelled by other philosophers.
Both Roger Gottlieb and Kai Nielsen, for instance, reject Habermas's implicit argument that the norms which would be agreed to in an unrestrained discussion are rational norms—norms which one must follow if one is to be rational. In this vein Gottlieb writes:

To call any claim rational, it seems to me, is to say (at least) that we violate our interests in disregarding it. If we could establish the rationality of a social norm, we would increase the degree to which people would follow it. This is the connection between rationality and motivation. But Habermas has not established this connection in the realm of social norms.35

And Nielsen, in a much more sympathetic review, writes that:

Habermas has not shown that intelligent, well-informed, conceptually sophisticated people must, on pain of irrationality, adopt such norms of justice and freedom. He has not shown that they, no matter how they are placed, on pain of intellectual mistake or false consciousness, must adopt the norms that would be conscientiously agreed on in an ideal speech situation—a situation of constraint-free consensus. Intelligent members of the ruling class or ruling elites know that, as a matter of fact, they are not in such a situation.... They might, without any failure of intellect or intellectual mistake, not accept the norms Habermas says are true and rationally required and ask, not without point, what intellectual mistake they could be tagged with for not accepting them. It is not evident that they must have made any...36

Nielsen and Gottlieb point toward a key issue which Habermas has not adequately resolved. The norms arrived at in a discourse may well be expressive of generalized interests, but many apparently competent speakers would deny that only actions in accord with these norms is rational. Many would agree with Gottlieb that the mark of an action's rationality is whether it advances the agent's individual interest. They would argue that, where an agent must choose between a course of action which advances his own interest and one which advances the generalized interest, the former course is the rational one to pursue. Others would argue, along with
Nielsen, that those who pursue this course cannot be charged with irrationality. Habermas could attempt to resolve this issue by asserting that those who argue thus are not completely competent speakers, that they have not yet reached a stage of ego autonomy. He could argue that fully competent speakers—those who have attained ego autonomy—recognize the rationality of action in accord with the norms, and then invoke his earlier claim that ego autonomy is an ideal state which few in Western society manage to attain.

A response of this sort would only intensify the basic tension in Habermas's program between theory and practice. Indeed, it would seem to tip the balance in favor of theory, at the expense of practical intent. And yet if he cannot show that there are generalized interests which take precedence over special interests, Habermas cannot overcome the relativism Feyerabend champions.

iv Summary

The issue raised by Gottlieb and Nielsen brings to a head the fundamental problem inherent in Habermas's entire project—that of the relationship between his idealizing theory and actual practice. As we have seen throughout the explication of his theory, Habermas repeatedly makes use of idealizations. His theory of universal pragmatics (which forms the foundations of his normative views) begins with an idealizing assumption about social action. There he claims that "action aimed at reaching understanding" is the fundamental type of action, and "that other forms of social action—for example, conflict, competition, strategic action in general—are derivatives of action oriented to reaching understanding."37

This assumption serves to focus the theory he develops, but it also
detracts from the theory's applicability. Much social action is not
aimed at reaching understanding; as Habermas readily admits, even a great
deal of linguistic action does not have this aim. But aside from des-
cribing such action as derivative from communicative action and showing
how it differs from action aimed at understanding, Habermas tells us
little about such action. Particularly worrisome in light of our reasons
for investigating his views is that his theory of undistorted communication
gives little guidance for action in social systems where such communication
is rare.
Notes to Chapter VI


2 "Utopia and Violence," in Conjectures and Refutations, p. 357.

3 See "Reason or Revolution?" in The Positivist Dispute in German Sociology.


6 Ibid.


8 Ibid., p. 3.


11 Ibid., p. 195.

12 Ibid., p. 192.

13 Ibid., p. 193.

14 Ibid., p. 199.

15 "A Postscript to Knowledge and Human Interests," p. 168.


17 Legitimation Crisis, trans. Thomas McCarthy (Boston: Beacon Press, 1975), pp. 107-108:
18 Ibid., p. 159, n. 16.

19 Ibid.


21 This picture is encouraged by the Freudian structural model which depicts the ego, id and superego as separate entities in conflict with one another.


24 "Some Difficulties," p. 31. It should be noted that Habermas adds an important qualification to the remarks quoted. He writes of such cases "that genuine confirmation of the critique remains unattainable - a confirmation which can only be gained in communication of the type of therapeutic 'discourse', that is, precisely in successful processes of education voluntarily agreed to by the recipients themselves." Italics added.

25 Legitimation Crisis, p. 113.


30 The difference between Habermas and Lakatos on this point can be explicated by noting that the process of socialization into a scientific community is one which only a few individuals undergo, while the process of socialization into the speaking community is one which all normal individuals undergo.

31 "Universal Pragmatics," p. 18. An analogous point could perhaps be made for rational reconstructions in science by saying that methodological disputes are a normal part of scientific debate.
32 See "Moral Development and Ego Identity."


CHAPTER VII

We began this inquiry by posing two principal questions: Is it legitimate for society to try to impose control over scientific research? And if social control is legitimate, is it even possible? The ensuing discussion ranged through the arguments of various contemporary philosophers of science. It is now time to draw together the diverse strands of argument to see if we can weave an answer to these questions.

The standard argument against social control, one offered by scientists and philosophers alike, is essentially pragmatic. It starts from the premises that science is a goal-directed enterprise and that the goals it pursues are valuable ones, then it contends that any external interference in scientific practice will disrupt the ability to achieve these goals.

This pragmatic argument is given a peculiar shape by the hands of the philosophers of science. They generally make two assumptions which severely weaken its chance of success. First they assume that the lone goal of science is truth; and then, following Max Weber, they declare that the choice of goals in science, as elsewhere, is an evaluative matter falling outside the sphere of rational discussion. The pragmatic argument can then be cast in the form of a directive: "Scientists pursue the truth, and their pursuit will succeed only if they are left alone. Therefore if you value truth, do not place any social constraints on research."

The first problem with the philosophers' form of the argument lies
in its hypothetical conclusion. The declaration that choice of goals is ultimately irrational places a limitation on the audience for the argument. It can hope to persuade only those who already agree that truth is valuable. In fact, it will be completely successful only with those who believe that truth is the primary goal of human endeavors. When posed to one who does not believe that truth is particularly valuable or to someone who believes that other goals are more valuable, the argument, as it stands, is unpersuasive. And since the evaluation of goals is postulated to be a decision beyond the reach of reason, further rational discussion with this dissenting audience is impossible.

The argument faces grave problems not only when presented to those who value goals other than truth, but also, when it is posed to someone who shares the faith in the primary importance of truth. Once the goal of truth is accepted, the onus falls upon the philosophers of science to demonstrate that a method of scientific research including structures on social control is more efficient at obtaining the truth than one which permits outside control. The mainstream methodologists we have studied do not even attempt this particular demonstration. However the more general methodological dispute among them gives ample evidence that the necessary argument will not be forthcoming. A large part of that dispute consists of pragmatic arguments in favor of the various methodologies. Each of the methodologists tries to show that the method he champions is superior to all others by showing that scientists who use it are, or would be, more successful than their colleagues in the scientific quest.1

Popper’s early defense of his critical rationalism is perhaps the epitome of this justificatory approach. He originally tried to support
his method of conjectures and refutations by saying that it was the method underlying the practice of exemplary, successful science. After he projects the discovery of interesting truths as the goal of science Popper embraces the correspondence theory of truth. This is where his real problems begin, for he also denies that we can ever know that a given interesting statement is true. How can his method be vindicated if we have no criterion of truth? Assuming that his method is actually the one used by scientists in the episodes he cites, how can we say that these are episodes of successful science if there is no way of knowing whether they attain the truth? In answer to these questions Popper suggests that the notion of truth is far less important than that of verisimilitude: while we cannot know when we have attained truth, we can know that we have gotten closer to it. But this suggestion fails to come completely to terms with the problem, for he also denies that we have any independent criterion of verisimilitude.

The difficulties facing Popper's program led some methodologists to question either the correspondence theory or the fallibilism which committed him to saying we lack criteria of truth or of verisimilitude. Kuhn toys with the former when he wonders whether anything of importance would be lost in abandoning the views "that there is some one full, objective, true account of nature and that the proper measure of scientific achievement is the extent to which it brings us closer to that ultimate goal." Lakatos flirts with the latter when he makes "A plea to Popper for a whiff of 'inductivism'."

Neither Lakatos nor Kuhn developed his idea sufficiently to allow an assessment of its promise in resolving Popper's dilemma. Nonetheless the complications confronting them are clear. Both men substitute some
demonstrably attainable goal for the Popperian aim of an accurate account of reality. This restores the possibility of choosing between competing methods by comparing their efficiencies in achieving science's end. But it does so at the cost of exacerbating the first problem we encountered, that of showing adherence to the method of choice leads to some valuable end. A true account of reality is a goal with great intuitive appeal; it is one to which many people are attracted. Having rejected this one, Kuhn and Lakatos offer no other with similar plausibility. And since both seemingly agree that over values there can be no rational discussion, the intuitive appeal of the goals they offer is all that their pragmatic argument can rely on for its persuasive power.

It is at this juncture that engaging Habermas in the methodological dispute proves useful. While he does not enter the controversy in the usual way—by criticizing details of the methods offered by the principals or by proposing a method of his own—he does make significant contributions to the background against which the dispute must be settled. What he constructs is a way of justifying the scientific enterprise as a whole, one which is consonant with the standard pragmatic strategy but which does not preclude the possibility of legitimate social control over some parts of it. Moreover, it is a program that distinguishes legitimate from illegitimate control.

Habermas's early writings differ in style and content from his more recent works, but a consistent program arises from the entire corpus. In the early works (including his exchanges with Popperian critical rationalists and Knowledge and Human Interests) his intent was to answer the question "What is the goal of science?" But the significance of this work for the methodological dispute can be easily
overlooked for various reasons. In the first place, Habermas inverted the standard approach. Most philosophers of science begin with the assumption that science has a specific goal and then search for a method that helps science achieve it. Habermas begins with extant methodologies and then tries to discover where adherence to the methods leads. His question is this: "Given its procedures, what ends can this science hope to attain?" In working through this question Habermas focuses on three methodologies—those of Peirce, Dilthey and Freud—and determines that there are three distinguishable sciences each aiming at a separate end, conceptualized by him as the knowledge constitutive interests. This constitutes the second difference of his program. Rather than postulate a single goal for anything deserving of the name "science", he ends up with a multitude of goals characteristic of multiple sciences. Finally, he differs from the pack in his specification of the goal of the physical sciences. Where Popper proclaims it to be the interesting true explanation of whatever anyone feels needs explaining, Habermas sees it as explanation which will ultimately prove useful in the struggle for survival.

Several lacunae in Habermas's preliminary work contribute to its underestimation by philosophers of science. His selection of Dilthey and Freud as proponents of reputable scientific methods runs counter to the accepted views in the field. Psycho-analysis has long been ridiculed by Popperians and others as the paradigmatic pseudoscience; and the protracted battle over the possibility of studying human action using the methods of the physical sciences has apparently been settled in favor of the unity thesis. Habermas's treatment of Freud and Dilthey as serious methodologists thus appears to be an attempt to revive mori-
bund, if not already dead, issues without producing any novel arguments.

As we noted earlier, Habermas does not deny the possibility either of successfully studying human action or of developing effective techniques for altering individual behavior by using research procedures borrowed from the physicist. This does clearly distinguish his program from that of writers like Dilthey and Winch. Although such research is possible, Habermas argues that the opportunity should be forsaken because it aims at an unacceptable end: technical control over humans. This line of argument is an original contribution to the earlier debate, but it fails to be persuasive because of the Weberian doctrine that goals are a matter of individual, subjective choice. It may be that Dilthey has outlined the proper method for reaching mutual understanding and Freud the method for achieving individual autonomy; but unless it can be shown that these ends are superior to that pursued by naturalistic social science, the importance of these methods to a scientist is doubtful. And the Weberian doctrine says that the superiority of ends cannot be shown. Thus the philosopher of science is free to say that, though these new methods are interesting, they are not methods of science. A true scientist pursues other goals.

Only in his later writings does Habermas turn to the tasks of defeating the doctrine that ends are irrationally chosen and of showing which particular ends rationality demands for the social sciences. These tasks are handled by his theory of communications, particularly the part of it detailing the moral discourse.

Habermas does not deal directly with our central issue of social control over science and therefore the implications of his theory of communicative competence for it remain somewhat clouded. The major
source of this cloudiness is the fact that his description of the moral discourse takes the form of a utopian idealization. How it might be applied in the current world is barely discernible.

On Habermas's view, evaluative questions are to be settled by consensus in a discourse. The conditions a discussion must meet in order to be a discourse are stringent. Some of them can be set out as descriptive characteristics of the forum for discussion: e.g., the rules of the discussion allow any claim to be challenged by any one of the participants, and the discussion is insulated from the need to act so there is no temporal constraint on it. Other conditions describe the character of the participants: they are people who have reached the stage of autonomous ego organization.

Difficulties in relating this idealized model to decision-making in the current world surround both sets of conditions. Discussion of important problems in the control of science, for instance, almost never occurs in complete isolation from some need to act. The debate over recombinant DNA research took place amid the clamor of scientists for a quick decision so that they could get on with research before their competitors in other countries. Deciding who should make decisions now is even more complicated.

Habermas conceives the moral discourse as a decision-making forum in which all humans have the potential to participate. In an ideal world all would be persuaded by the "force of the better argument" to accept a common set of goals. However, although nearly all humans have the potential to reach the stage of ego identity and so are potential participants in discourse, Habermas admits that "an autonomous ego organization is by no means a regular occurrence,...in fact it is not usually attainable." How are decisions to be taken in a world where only a few are able to participate fully in a moral discourse?
We have already seen some tentative suggestion of a tack Habermas could take in trying to apply his theory to the practical problems facing society. It is a tack worth following because it offers one answer to the question of how science might be controlled.

The suggestion is that in the real world decisions should be taken in a forum which has as close a resemblance as possible to the moral discourse. This means in the first place that free discussions should aim at setting social goals by consensus. Since defective character formation would prevent some individuals from recognizing the correctness of certain ends, consensus will be unattainable if these individuals take part in the discussions. Evidence of defective or incomplete character development could therefore serve as sufficient grounds for excluding someone from the consensus-seeking forum. All of this means that in practice decisions about the direction of science are to be made by those who have reached a requisite level of character development, meeting for free and open discussions.

As we saw in the preceding chapter, Habermas has yet to provide a completely satisfactory account of how the required character development is to be recognized. The main threat to this account is that the criteria used to recognize the man of character will fail to be independent of the decisions that man makes. The worry is that only a small number of people in advanced Western societies will meet the criteria. The evidence available is simply too scanty to support his claim that the criteria he seeks represent universal competences. Therefore the use of exclusion criteria continues to be dogged by powerful suspicions of circularity and ethnocentrism. Let us lay aside these theoretical concerns (hoping that they can be eased) in order to develop more fully
the nascent view of how science might be controlled.

When we turn to a specific case, such as that of recombinant DNA research, we discover that the decisions which need to be made in controlling science come in a variety of forms. And this variety spawns additional concerns which must be taken into account in trying to apply the model of the moral discourse. At the most general level lies the question of the broad social impact of pursuing knowledge in this field. Should research into recombinant techniques for gene manipulation be allowed? Should this research be viewed—as its proponents would have us do—as merely another means of manipulating the physical world to aid human survival in a hostile environment of famine and genetic disease? Or should we take the viewpoint of its opponents and see recombinant DNA technology as the latest and most potent means of dominating other people? Habermas's model seems best adapted to solving issues at this level. At the more specific and prosaic level lie the questions about whether particular experiments might be too dangerous to perform. Any theory about how science should be controlled must also give us an indication of how these specific questions can be answered. So we must determine whether the moral discourse, or something akin to it, is an adequate device for answering both kinds of questions.

If we wish to use the model of the moral discourse to resolve actual issues in the control of science, we are forced to realize that the list of characteristics of the ideal participants must be supplemented. Important portions of any debate over the direction of science turn crucially on recondite bits of information—information which only those well versed in a particular science have. This is obvious when the focus of the DNA debate was on the safety of particular experiments. Resolu-
tion of the safety issue in fact required the collaboration of bacteriologists, virologists and other specialists. They were needed to point out potential dangers and give accurate assessments of their probabilities. Although this is less obvious, scientific expertise is also crucial once the more general question about the direction of science is broached. Familiarity with the facts of scientific research is needed at least to set the agenda for discussion. One must know the research field to know what the impact of research is liable to be; to know whether purported social consequences are real and whether they are likely to be realized in the near or distant future.

The picture which is beginning to take shape is one of a decision-making mechanism manned by an elite whose members are technically competent in the particular scientific field under consideration. At first glance this method of control looks like the one scientists have recommended from the start: the scientific community should be an autonomous, self-directing one free of all external control save that imposed by the amount of financial support. However this appearance is misleading. It overlooks the fact that on the Habermasian view not all scientists, not even all scientists working at the cutting edges of their respective disciplines, would automatically be deemed competent to take part in the decision-making discourse. Only those who are simultaneously competent to deal with the scientific matters and who are of requisite character would take part. The picture, in fact, resembles more closely the one traditionally painted of the self-regulating professions like medicine than the standard picture of science.

"Profession" is a term with diverse meanings. In ordinary usage it refers to any activity from which one derives his livelihood, while in
the sociologists' lexicon it refers to only a subset of occupations. Even in that lexicon its meaning is not precise since sociologists differ in the characteristics they use to distinguish bona fide professions. It is therefore necessary to clarify how the term is being used here.7

One way of elucidating the characteristics of professions in general is by studying one paradigmatic profession; and commentators are nearly unanimous in their choice of a paradigm. As Eliot Freidson puts it, "if anything 'is' a profession, it is contemporary medicine." 8 This selection of medicine as the model is decisive for the recent discussion.

Starting with medicine it is possible to draw out two primary characteristics: the members of a profession share a specialized body of knowledge, typically gained during a prolonged period of study, and they share a commitment to an identifiable ethical idea. This ideal is often articulated in a code of professional ethics, and the individual's commitment to it is not uncommonly signified by the swearing of an oath upon entry into the profession.

The first characteristic is closely associated with the view that professions should function autonomously. Just as in the sciences, peer review and professional autonomy are seen to be the necessary consequences of intellectual specialization. In assessing whether a medical treatment has been performed properly it is not enough to look at its results alone. Patients die despite the best treatment offered by the most conscientious of physicians. Proper assessment relies on an intimate knowledge of the current state of medical science, and for all practical purposes, one must undergo the training partially definitive of the profession to have this knowledge. Because no one else has the ability to judge fairly the technical merit of therapy, the responsibility for doing
this falls to the profession itself.

The choice of medicine as the exemplary profession is decisive for the way in which the ethical commitment of professions is generally conceptualized. The ethical ideals imputed to physicians are the ones to which any genuine profession aspires. The core of the physicians' ethical ideal is practical service. This ideal is clearly stated in the first line of the World Medical Association's updated version of the Hippocratic Oath: "I solemnly pledge myself to consecrate my life to the service of humanity."9

Both specialized knowledge and commitment are essential characteristics of the individual physician. His technical training gives him the ability to intervene effectively in some disease processes while his commitment to caring for his patients leads him to use this ability in curing his fellow man. The physician can be charged with unprofessional conduct for letting slip either his skills or his commitment.

The two characteristics can also be linked in a compact between the profession and society. Members of the profession can perform tasks which society finds valuable. The performance depends on specialized skills which only one properly trained can judge. For this reason the profession's autonomy in assessment is deemed necessary. But in exchange for this grant of autonomy society demands a special dedication by the profession to the high ideal of service to the community.10

The idea that science might be a profession akin to medicine is not novel. Max Weber suggested it in the very title he gave to his seminal paper on science, "Wissenschaft als Beruf" --"Science as a Profession". The idea remains part of the background shared by the writers who follow him, particularly Popper and Mannheim. What is new is the closeness of
the affinity being suggested here. The difference in degree of kinship proposed by the older Weberian view and by this new one emerges in a little-read paper by Popper.

Popper overtly plays upon the similarities between medicine and science as professions in his article "The Moral Responsibility of the Scientist." He there proposes that scientists should adopt one of the rites of the recognized professions, the swearing of an oath to mark one's initiation into the professional community. The theme of similarity continues through Popper's recommendation for the oath, a proposal he describes as "a very tentative restatement of the Hippocratic oath." The content of this oath, however, reveals the gap between the traditional view of the medical profession and his perception of the scientific.

The Popperian oath recognizes the two duties of the scientists which have been at the center of our concerns: the duty to seek truth and the duty to protect society from the untoward effects of scientific information.

The first duty of every serious student is to further the growth of knowledge by participating in the search for truth—or in the search for a better approximation to the truth...

The student must be constantly aware of the fact that every kind of study may produce results which may effect the life [sic] of many people, and he must try to foresee and guard against possible danger and possible misuse of his results, even if he does not wish to have his results applied.

Whenever several duties are assigned to an individual it becomes possible for them to come into conflict with one another, and these scientific duties are no exception. Recognizing that research into a particular area might pose grave dangers to mankind if successful, a scientist may wonder whether he should protect society by refusing to
further the growth of knowledge in that area.\textsuperscript{14}

The codes of ethics of the medical profession clearly indicate how its members should act in the face of this dilemma. The acquisition of knowledge is of secondary importance. The medical researcher must first insure the safety of his patient before starting any experimentation. Once he is sure that the risks are minimal he must insure that the knowledge promised is worth even this level of risk. If both assurances cannot be given, the search for truth should not proceed.\textsuperscript{15}

Popper's proclamation that aiding in the growth of knowledge is the first duty of the professional scientist also serves to resolve the dilemma of conflicting duties. But it resolves it in the opposite direction. This duty is first in the sense that it takes precedence over all others. While Popper offers a clarion call for scientists to foresee the dangers of application, he does not even consider that the recognition of danger might militate against the pursuit of knowledge. At most he allows that such recognition might alter the direction of further pursuits. The professional scientist "should not only warn the people of the dangers but devote himself to the discovery of effective counter measures."\textsuperscript{16}

We are left with two views of the scientific profession. On the Habermasian view developed here the scientists—be he physicist, sociologist, or psychoanalyst—is dedicated primarily to the service of mankind just as the physician is. Each branch of science has its own distinctive way of serving man, and this requires the use of distinguishable methodologies. Physics serves by producing the information useful in controlling nature, sociology by increasing mutual understanding, and psychoanalysis by increasing liberating self-understanding.
It may turn out that the best way of achieving these intermediate goals is by prescinding from them; but it must be remembered that this methodological device is subservient to the goals. This is clear in Habermas's handling of the methodological dispute over social sciences. While admitting that treating man as part of the natural world and studying him with the methods of the natural sciences can yield reliable information, Habermas contends that this is a kind of research from which scientists ought to refrain insofar as it is inimical to the goal of social science.

On Weber's view as developed by Popper the professional scientist is distinguished from the medical professional precisely by his dedication to a different ideal. The scientists, regardless of his special discipline, is dedicated to the pursuit of truth. The knowledge this uncovers may have propitious or deleterious social consequences. The scientist ought to recognize the probable consequences of his work, promote the favorable ones and try to circumvent the unhappy ones; but he should do all of this only to the extent that it does not interfere with this performance of his primary duty—acquiring knowledge.

Despite their disagreement over the aims of the sciences, the traditional view of Weber and his followers and the Habermasian view agree over how control over science should be effected. Only the scientist has the detailed knowledge necessary for successful control therefore science, like medicine, must remain an autonomous profession.

The conceptualization of science as a profession closely akin to medicine is useful because the extensive sociological investigations of the medical profession undertaken in recent years have clearly delineated the dangers inherent in the autonomy of the professions. The critical
literature on the medical profession is diverse, but all of it can be focused on the question of whether in practice the compact between society and the profession properly serves the interests of society. A substantial part of that literature argues that the institution of professionalism serves the special interests of professionals rather than (and sometimes to the detriment of) the general interests of society. Earlier we noted that the compact takes the form of an exchange: society gives to the profession power to control its membership in exchange for the profession's promise to assure acceptable standards of practice in the pursuit of socially valued goals. In order to understand the literature and differentiate the various strands of criticism it is necessary to look somewhat more closely at the details of this bargain.

The autonomy granted to the medical profession is obviously not absolute; for example, membership in the profession does not excuse one from obedience to the strictures of the criminal law. But within the field of medicine the degree of autonomy is large, extending even to the definition of the boundaries of the field.

The organized profession's autonomy is exercised through the legal instrument of licensing. One who does not possess a valid license from the professional body is barred by law from the practice of medicine. In the first instance this is related to control over education. Only those with degrees from accredited medical schools are eligible for licenses; and accreditation of the schools has been taken over the professional organizations. The requirement common in many jurisdictions that holders of medical degrees pass additional examinations before being granted a license serves as another check: a check both on the individual applicant's ability to practice adequately and on the education system. The
medical school which wishes to attract students must insure that its curriculum prepares graduates to pass the professional examinations.18

The profession's sphere of control extends beyond the education system and entry into the profession, for the body which grants licenses also has the power to withdraw them. It can do this to physicians whose standards of practice fall below minimum levels for any reasons. Thus an older physician who has failed to keep abreast of advances in treatment or a physician whose personal conduct (e.g., substance abuse) adversely affects his performance ability are both liable to suffer the loss of their licenses.

By virtue of this control over who is eligible to practice, the profession gains control over how medicine is practiced. This control extends not only over the technical aspects of medicine but also over its non-technical, social elements. Licensing boards may decide to rescind an individual's right to practice if it is shown that his personal behavior has violated moral rules of professional conduct. A doctor who publicizes information about his patients violates the rule of confidentiality, and even if the treatment he provides is impeccable he may lose his license for unprofessional conduct. Similarly, one who seduces his patient may face disciplinary action even when the seduction has no direct bearing on her medical care. In this way physicians can be held to a higher standard of personal behavior than, say, automobile mechanics.

The rationale for allowing control over these sorts of non-technical lapses is derived directly from the terms of the compact between society and the profession. The necessities of practice place the doctor in a position of power over his patient. The patient must reveal things
about himself and allow a great deal of touching if his physician is to offer effective treatment. The organized profession's authority to censure individual members for their abuses of this power protects patients.

There is one last broad area of medical practice over which professional bodies have claimed regulatory authority. They have sought to control such organizational features as advertising by members, the financial terms of the contracts physicians may accept and the location in which services are delivered. The Canadian Medical Association's "Code of Ethics", for instance, holds that an ethical physician "will only enter into a contract, regarding his professional services, which allows fees derived from physicians' services to be controlled by the physicians rendering the service" and "will enter into a contract with an organization only if it will allow him to maintain his professional integrity." 19

The rationale offered for control over these areas is implicit in the last phrase. The organization of the health care delivery system can have great influence on the services offered, and the profession has a legitimate role to play in insuring that no individual member allows it to adversely affect the quality of care he delivers.

Sociologists and historians of medicine who have studied the profession's use of its self-regulatory powers have usually done so to determine whether the promised social benefits of licensure have been delivered. Their studies should give pause to a proponent of the Habermasian project for social control outlined above. Many of them conclude that, on the one hand, the profession has been less than zealous in policing its members' practices; while on the other, it has been extremely active in protecting its professional prerogatives, often at
cost to society. 20

Eliot Freidson, the American sociologist of the medical profession, has undertaken field studies regarding the fundamental issue of how the professional regulatory bodies go about assessing the technical quality of medical services rendered. His conclusion is that the profession's record in this area is poor simply because it has failed to make any serious effort to evaluate on a regular basis most of the services provided by physicians. He reports that evaluation by peers is a regular feature of certain treatment locales such as hospitals, which generally have quality review committees. Teaching hospitals in particular are apt to have rigorous and formal review mechanisms which insure that the care offered within their walls is of suitably high quality. However once a doctor leaves the hospital environment, for the most part he leaves formal reviews of his work behind. The profession has developed no procedures for regularly reviewing services offered by the doctor in private practice. Instead of searching for deficiencies in this way it reacts to specific complaints against a doctor. Even in group practice, which presumably offers the best opportunity for peer review and formal sanctioning of inadequate individual standards, Freidson was able to find little evidence of either. If services delivered outside of the hospital setting represented only a small portion of all medical services, this failure to assess them might not be too serious. Freidson notes, however, that they compose the bulk of medical practice. Thus the major portion of practice remains outside the pale of regular formal review by the profession's own regulatory arm. 21

The problem Freidson discusses could be remedied in fairly straightforward ways without substantially altering the system of professional
autonomy. But a number of other authors have argued that, whatever its impact on the quality of treatment, giving the medical profession broad powers of self-regulation inevitably has gravely deleterious social and economic effects on the health care system. The granting of autonomy has these consequences because it effectively makes the profession into a monopoly. The argument is most forcefully and concisely presented in Jeffrey Berlant's *Profession and Monopoly*.

Berlant's contention is that the organized medical profession (whether by conscious design or not) has managed to stifle competition, thereby decreasing the availability of alternative forms of treatment and increasing both the cost of health care and doctors' incomes. It has done this through the use of its legal power to license physicians and to prevent unlicensed persons from practicing medicine. And it has justified its actions by arguing that they were needed if it was to perform its duty of protecting the public from quacks and charlatans.

Monopolistic control begins with the profession's authority to decide who is eligible for a license. Because it sets the accreditation requirements for the medical schools, the profession is in a position to control directly the content of medical education and indirectly the kind of medical services available to the public. Many critics have argued that through its control over education the profession has caused doctors to concentrate on expensive and invasive treatment of disease while virtually ignoring preventive measures. Berlant argues that its control over education puts the profession in a position from which it can even more directly manipulate the marketplace. It can limit the supply of physicians offering service through the simple expedient of setting "spiraling standards" for entrance into the field. Limiting
the number of physicians lessens the probability of price wars among them. 22

It is generally agreed that licenses which have been granted should be revokable for unethical professional conduct as well as for malpractice. If the definition of "unethical conduct" is left entirely in the hands of the profession, however, a further element of monopolistic control becomes possible. Medical bodies have often deemed participation in certain payment schemes to constitute unethical conduct deserving revocation of the privilege to practice. 23

Licensing powers can thus clearly be used to make price competition among physicians unlikely and to smother whatever competitive schemes do begin; and they can also be used to eliminate competition between physicians and other health care professionals. Richard Brown traces the history of the American medical profession's successful competition with herbalists, homeopaths and other alternative practitioners. The strategy consisted of two parts. Physicians who consulted with alternative healers or referred patients to them were threatened with sanctions for such unprofessional conduct. Then in some instances the non-physician practitioners were themselves accused of practicing medicine without a license. In this way the patients available to those outside the profession, and the range of techniques available for use by them were both limited. 24

Brown's history describes how some alternative health care practices available in the nineteenth century were eliminated or driven to the fringe by the early decades of the twentieth. But his account is of more than merely historical interest since the strategies he describes can be used as effectively now to prevent alternative modes of treatment from
becoming available. Some have suggested, for instance, that these strategies are being used by the profession to keep birth under the control of physicians in hospitals instead of allowing it to return to the control of midwives in other locations.²⁵

The apparent failure of the medical profession to insure that its members live up to its self-proclaimed ideals has caused a flurry of interest in ways of achieving the goal. The alternatives suggested for the most part parallel the suggestions we have already seen for controlling science.

Often it is assumed that the problems caused by inadequate internal control are rooted in the education system. Medical students must learn an enormous amount of factual information and master a good number of complicated procedures in a short period of time. The development of this technical competence places great demands on the prospective professional's time; and these demands leave little time for careful consideration of ethical issues. Moreover, the very form of instruction in medical schools seems to downplay the importance of such consideration.

Once this view is accepted two paths are opened. One is essentially Habermasian: it calls for educational reform. It claims that, to the extent that medical education fails to inculcate the proper sort of values into neophyte physicians, it fails in its task of producing true professionals. It fails to develop those elements of character one needs if he is to take part in the moral discourse. Educational reform should take its cue from this and aim at insuring that physicians possess those virtues traditionally attributed to them.

The most serious criticism of this reform proposal is directly related to the theoretical problems inherent in Habermas's project in
moral theory. Even a cursory look at the literature on moral issues in medicine reveals that contemporary society lacks any consensus on a host of basic issues such as the value of life. In the face of such pluralism in values, what sort of moral education should professionals receive?

The second path assumes that the goal of the education reformers is unattainable; the traditional notion of the professional is chimerical. Instead of trying to produce physicians who are both technically competent and virtuous—and who could therefore safely be left to control themselves—efforts should be made to open up the control mechanisms which already exist within the profession. Rather than leaving the membership of regulatory bodies composed entirely of physicians, it ought to become interdisciplinary.

The second path stands vulnerable to exactly the same serious criticisms which bedevil the first: it offers no solution to Habermas's theoretical problems. The lack of a general consensus in society on the direction control should take may well be reflected in an interdisciplinary committee's deliberations, leaving it unable to reach any unanimous decisions. And where it does reach a consensus the suspicion may remain that this is an artifact of cleverly contrived membership selection.

There is a third response to worries about the pervasive influence of the organized medical professions that is more radical than the earlier ones, and essentially Feyerabendian in nature. It calls for the abolition of most of the current structures of control and the abandonment of the idea of professional privilege which undergirds them.

The primary target of this response is the amount of control that physicians' professional organizations exert over the health care field.
While usually conceding that a doctor's technical skills are best judged by other doctors, its proponents contend that no single profession should be established in a position of power from which it can dictate the economic terms of practice and declare methods of care unacceptable. The latter power stunts the growth of new treatment technique while the former increases unjustifiably the cost of standard treatments. In these realms the only form of control which should be countenanced is that exerted by the market forces generated through the choices of health care consumers.

The standard argument against the replacement of professional control by market mechanisms has long been that it places the consumer at a disadvantage. The individual consumer is simply unable to make the decisions called for. He has neither the time nor the training needed to make a rational choice between courses of treatment based on different theories of illness and disease; as, for instance, between treatment offered by a regular physician and an osteopath. Nor is he able to choose rationally between the standard treatment within one medical tradition and a radically new treatment within the same tradition.

Contemporary critics of the medical profession are quick to point out that this defense is no longer viable. It has been made obsolete by the changes in the structure of the medical marketplace. The picture of the isolated consumer deciding how to spend his health care dollar misrepresents the existent social system. In the vast majority of cases payment of medical expenses is arranged through a third party, either a government agency or a private insurer. While individuals may be unable to choose reasonably between alternatives, organizations representing many individuals can develop the expertise needed to do so; and
if they are large enough, they can effectively negotiate economic terms with organized professions.

The third scheme for controlling medicine is similar to Feyera-
bend's scheme for controlling science; and the reason for deploying it are identical to his reasons. 27 Scientists, like doctors, use the rhetoric of professionalism to convince the general public that they are interested not in personal glory or wealth, but in the social good of knowledge. They also claim that as part of their professional training they have mastered the difficult method of distinguishing good research (i.e., research that is genuinely knowledge yielding) from the mediocre and the bad. This training makes them the proper arbiters of which research projects should proceed. While the power to direct science puts them in a position to further their own interests, scientists' moral commitment to the goal of knowledge insures that they will remain stalwart in the face of temptation and will act in society's interest.

Feyerabend argues that the history of science gives no more reason than does the recent history of medicine to believe that the story of the professions is anything but a self-serving myth. There is no single method which distinguishes good science from bad, and scientists are as apt to use force to advance their aims as is any other group. These arguments drive him to his call for the abolition of the scientific professions's control. Within science research projects should be allowed to proliferate, and in society non-scientific traditions (i.e. traditions which pursue alternative goals) should likewise be allowed to proliferate. Individuals should then remain free to choose among the alternatives.

There are some important differences between medical practice and
scientific research which may cast doubt on the foregoing analogy and, in turn, on the practicality of Feyerabend's marketplace of traditions. Medical practice can readily be characterized as a service which is controllable through market mechanisms, but scientific research seems to defy characterization as a market commodity. Research is, after all, the activity of inquiring minds. This disanalogy is not as serious as it first appears. Modern scientific research is a costly undertaking. Researchers must be trained and their hypotheses must be tested, and this costs money. This forces the scientist to look for sponsors willing to support his work; and the need for sponsorship pushes science into a marketplace.

This marketplace of medical practice and the marketplace of scientific research are structurally different in one crucial respect. Medicine's ability to affect any given individual depends on his own choices. If he does not seek the attention of a practitioner, or having sought attention, declines treatment, there is little that can be done to or for him. But the effects of a particular bit of scientific research may extend far beyond the contractual relationship between the scientist and his sponsor. If a scientist wishes to release a genetically altered organism into the environment he might be stopped by his failure to find a sponsor. Once this hurdle is overcome, however, there are no others. The effects of such research, particularly if they are detrimental, are liable to be felt by all, including the members of traditions which stand opposed to any sort of genetic tinkering.

The Feyerabendian model for controlling science as it stands has no mechanism to prevent the scientific tradition from affecting adherents of other traditions in cases such as this. This is where it stands in
starkest contrast with the critical theory propounded by Habermas. One of Habermas's earliest realizations was that in the contemporary world science has become a part of the system of production, and one of his motivations has long been to insure that this system is not entirely under the control of special interests.

The argument against social control over science that we have investigated is not the only one to be found. We have singled it out for careful consideration because of its ubiquity, but also because the difficulties which it encounters also bedevil the other arguments that are proffered. Many of these difficulties find their roots in an anachronistic view of the place of science in modern society. The pragmatic argument seems better suited than most arguments against control to take account of the changed relationships between society and its science.

One traditional argument runs directly into trouble because of the changed relationship. It is sometimes argued that any form of control is a violation of the scientist's fundamental right to freedom of inquiry, and is therefore unacceptable. As long as scientific research remained a private undertaking this argument was persuasive, but in the modern world it is beset on two sides. Rights are not unlimited; their exercise can legitimately be restricted if it adversely affects others. So when research predictably has consequences on others these must be taken into account, and they may constitute sufficient reason for restricting the inquiry. And most research is now of some consequence. On the other hand, the growth in the cost of research introduces another avenue by which legitimate control can enter science. It is not possible to finance all of the research projects which anyone might wish to pursue,
and this scarcity of resources necessitates the curtailment of some projects.

The pragmatic argument recognizes the inevitability of some controls over science, but it tries to insure that control is internal rather than external—that control remains in the hands of scientists. All of those who use the pragmatic argument implicitly recognize the scarcity of resources. They recognize that all lines of inquiry cannot be followed. Then they argue that proper scientific method should serve as the standard for judging research programs. If an inquiry can be conducted according to the canons of method, then it is a candidate for resources; otherwise it is not. It is of course possible that the number of reputable scientific projects will still exceed the resources available. In that case preference should be given to the most "promising" candidates. The core of the pragmatic argument is that these judgements of scientific merit and of promise are properly within the purview of scientists.

The philosophers who invoke the pragmatic argument tend to overlook an important part of science's social situation. They assume that the goal of all science worthy of its name is the revelation of truth (or the growth of knowledge) and that the overwhelming value of this goal is obvious to everyone. The goal of truth is unassailable. While most follow Popper in holding that the choice of goals is nonrational—a subject for "cocktail chatter" rather than for serious philosophical discussion—they are quick to dismiss anyone who questions this goal as an incorrigible romantic, a playful skeptic, or simply a "silly bugger." The scientists must confront the social situation more directly. When they try to secure funding to carry out their work they must regularly
give some argument that their results may prove useful. Indeed, they must often try to show that the use of funds to support their research is better than any alternative expenditure. This forces scientists to walk a thin line. When trying for funding they must link their projects as closely as possible to potential benefits, and when trying to avoid outside control they must detach them from any potential harms. Stated another way, they may describe their research as applied science with potential technological utility or as pure scientific inquiry. Which description they use depends on their immediate need. 29

The pragmatic argument successfully precludes all elements of external control only if two of its presuppositions are correct. Only if it can be shown that there is a scientific method which yields truth and that the attainment of truth is the preeminent goal of human striving does the argument work.

Feyerabend and Habermas have cast both of these presuppositions into doubt.

Feyerabend takes the protracted dispute in philosophical circles over the methodology of science and the evidence to be gleaned from science's history and works them into an argument that there is no such thing as the scientific method. Those who are recognized as successful scientists have in fact used a great variety of methods in their work. And in his later works, after having received no answer to his query "What's so great about truth?", he took it that there is no rational response to be found. There is no reason to believe that truth is the preeminently valuable goal of humans.

Habermas's early work considered methodology. Like Feyerabend he determined that there is no single method. Instead of a single method
characteristic of all science, he found three with each one suitable to the attainment of a different goal. Where Feyerabend and the majority of more orthodox methodologists accept that over goals there can be no rational decisions, Habermas set out to develop an argument that the three goals he projected are the ones which any rational man would pursue. In his theory of communicative competence he tries to show that they are the universally binding goals of human endeavor.

In the end Feyerabend and Habermas agree that social control over science is not necessarily illegitimate, and that control may be necessary if domination of man is to be avoided. But out of their theories come different approaches to control. Feyerabend can countenance no general theory or practice of control. He must rely on the hope that individuals will be able to develop traditions around their values choices and that these traditions will be powerful enough to do what earlier ones have failed to do: stop the growth of currently dominant scientific tradition. Habermas has the rudiments of a theory of the direction control should take. He must hope that this can be developed into a complete and sound theory. Then he must hope that a practical means of control can spring from it. He must hope that those who have the knowledge necessary to science leads will also develop the virtues needed to lead it wisely.
Notes to Chapter VII

1 The reason why the specific issue of outside interferences does not arise in the methodological dispute is that strictures against external control are part of all of the methodologies.


3 Revolutions, p. 171.

4 "Popper on Demarcation and Induction", in Philosophical Papers, vol. 1, pp. 159 ff.

5 "Moral Development and Ego Identity", in Communication and the Evolution of Society, p. 70.

6 This ignores the device of an interdisciplinary regulatory body, some of whose members are scientists.


8 Profession of Medicine, p. 4.


10 See Freidson, op. cit., p. 137.


12 Ibid., p. 331.

The research team headed by Jonathan Beckwith at Harvard faced this dilemma before the recombinant DNA debate began, as had an earlier generation of atomic physicists.


"The Moral Responsibility of the Scientists," p. 335. Popper returns to the analogy between medicine and science in a recent paper he coauthored with a physician, Neil McIntyre: "The Clinical Attitude in Medicine: The Need for a New Ethics" (British Medical Journal 287 (24-31 Dec. 1983): pp. 1919-23.) The interesting point about that paper is that its authors invert the suggestion being made here. Instead of arguing that the scientific profession should be seen as similar to the medical in its goals, they tacitly treat the medical profession as having exactly the same goal that Popper attributes to science: the growth of knowledge.

See the papers gathered in Law and Human Behavior 7 (1983).

For a detailed discussion of how licensing came to be linked with control over education in American medicine see Brown, Rockefeller Medicine Men.


This discussion ignores those writers who attack the concept of professional ethics in general or the particular ethical codes adopted by the medical profession.


Ibid., Ch. 2, "Medical Ethics and Monopolization".

Brown, op. cit., Ch. 2.


The grounds for this assumption may be quite varied. They may be
the theoretical ones outlined above. But they may equally well lie in
doubts about the efficacy of moral education in general, or in doubts
about the possibility of introducing moral education into the already
crowded medical curriculum.

27 Feyerabend is aware of these similarities. In *Science in a Free
Society* he treats modern medicine as a part of the dominant scientific
tradition.

28 I am indebted to Professor C.B. Martin for stating the position
in this colorful manner.

29 When the dangers of research are too immediate to be ignored
scientists sometimes use the idiom of professionalism to escape from
outside intervention. Note Stanley Cohen's statements cited in the
Introduction above.
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