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Scientific Authority: A Critical Reappraisal of the
Study of the Role of Science and Technology
in Contemporary American Society

By

Henry Milner, B.A., M.A.

A thesis submitted to the Faculty of Graduate
Studies in partial fulfillment of the
requirements for the Degree of
Doctor of Philosophy

Department of Political Science

Carleton University

Ottawa, Ontario

April, 1976

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The undersigned recommend to the Faculty of
Graduate Studies and Research acceptance of the thesis

**"Scientific Authority: A Critical Reappraisal of the Study
of the Role of Science and Technology
in Contemporary American Society"**

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the degree of Doctor of Philosophy

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September 1976

ABSTRACT

This work examines the impact of science and technology on American society by refining and applying the concept of scientific authority. Beginning with historical treatment of the development of U.S. science and its place today, it critically examines some works dealing with social and political implications, surveying the vast literature of "science and society." The works are categorized according to conceptual framework and underlying normative goals.

Essential differences in perspectives are clouded by the various writers' inability to agree upon a suitable analytical framework--which seriously hampers the debate. The result is that thinkers in each group tend to find their own normative views confirmed since they conceptualize essentially different realities. There are three basic modes of analysis and these coincide roughly with the three fundamental programmatic positions: "established," "reformist," and "radical." Some conceptual framework which might help to bridge these levels of analysis and clarify the debate on this important subject is needed.

We attempt this first through an analysis of the scientific community's actual operations, utilizing insights of philosophers and sociologists of science, and then by suggesting how their findings may be extended to questions concerning how the scientific community relates to society. In this context we introduce the scientific authority concept. Applying the standard political science literature on power, influence and authority and the perspective of "political linguistics." We found it necessary to extend "authority" beyond its normal usage--which generally limits it to formal organizations. To demonstrate this contention, the Milgram experiments on obedience are described and re-examined. The relationship between scientist-experimenter and non-scientist-subject can be best understood as embodying scientific authority which lies outside formal institutional confines. This expanded and case-supported concept, combined with more precise conceptualization and application of "power" and "influence," seems to provide rudiments of an analytical framework which potentially reconciles differing approaches characteristic of major positions.

This latter statement emerges from an investigation of criticisms continually aimed at the Atomic Energy Commission through most of its history. The AEC was an American scientific institution par extraordinaire; we conclude that the perspective of scientific authority helps to explain its development and especially those practices which were most subject to criticism. It is hypothesized that scientific authority contributed to some of the failings of the AEC in the assumption by the public and politicians that the AEC scientists' concern was first and always to protect the public from unnecessary dangers which was not always the case. This presumption must be related to a context whereby the authority of AEC scientists

was defined more widely than their perspective should have permitted. In addition, the authority of science was extended even to encompass AEC policies and decisions which were not scientific at all, but rather established on political-bureaucratic grounds. The AEC case is impure; power and influence as well as authority--itself not always the authority of science--were at play, as compared to Milgram's laboratory. However, a striking and dramatic parallel does emerge. In several important cases we find analogies between important decisions of laymen AEC officials, acting in accordance with what they understood to be the goals of the AEC, and the behavior of Milgram's subjects who obeyed the experimenter by "punishing" supposedly innocent victims.

In a brief conclusion we speculate as to which major positions on the place of science in society tend to be strengthened or weakened by the findings and how remaining differences might be further reconciled.

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It goes without saying that final responsibility rests entirely with this writer.

Henry Milner

Montreal, Quebec

April, 1976

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Introduction

At about thirteen years old, in a boys club, this student asked his twenty or so comrades all to stand up from the table and to raise the chairs they were sitting on up high over their heads and hold them there. Their immediate reticence and incredulity was met by the assertion that this was a scientific experiment. All twenty chairs then went up including that of the 25 year old leader. Having soon thereafter left the room, he found out only a while later that it was a matter of several minutes before the chairs slowly began to come down.

The conclusion from this at the time was that people will do almost anything if they think there is a good reason, and furthermore, that the cause of science seemed to be a pretty good reason to people.

It seems quite fitting now, many years later, to take up this question in a doctoral dissertation. In retrospect, what was exercised that night was what may be termed scientific authority. It certainly was not power. For those few moments science set the context and standard of behavior for the group. It is this phenomenon, scientific authority, which is to be investigated in this thesis.

The year of this minor experiment was 1960. In that same year, retiring President of the United States, Dwight D. Eisenhower delivered a farewell address which was unexpected in its depth and insightfulness. Perhaps the most remarked upon statement he made was the following:

The prospect of domination of the nation's scholars by federal employment, project allocations, and the power of money is ever present and is gravely to be regarded.

Yet, in holding scientific research and discovery in respect, as we should, we must also be alert to the equal and opposite danger that public policy should itself become the captive of a scientific-technological elite.

Since this statement was uttered there has been a remarkable increase in interest and concern over the impact of science and technology upon society. Political leaders and their advisors in many nations have made advancement in science and technology a central political goal, tying it directly to national power. Of more direct interest is the abundance of both academic and popular literature relating to the entire question of the role of science and technology and its effect on society. This interest has been recently marked by a proliferation of serious academic works. Political scientists are to some extent included in this group, but in general they have given the subject proportionately little attention.¹

This study is in large part a critical evaluation of some of the more important theoretical positions and contributions on this question. The method is first, to categorize each position according to several key elements on which writers differ, second to consider these elements in light of an analytical framework for assessing the relationship of science and technology to society which we shall outline, in order to, third, provide grounds for choosing among the positions.

Four central issues relating to the various opinions which shall be considered in detail are briefly depicted as follows. Writers holding these divergent views will later be identified.

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1) One question is whether, or perhaps to what extent, the present situation is fundamentally new or qualitatively different. Some scholars argue that contemporary (Western and, usually, specifically American) society has reached a new stage. Industrial society has been replaced by a "Technotronic" or "Post-Industrial" or "Technological" or "Post-Modern" state in which the relationship of science to technology (and thereby of both to society) has been fundamentally altered, and this has resulted in a qualitatively different society. Yet, not all scholars would agree. Many suggest that no fundamental transformation has occurred and that the role of science and technology, though enlarged, is fundamentally unchanged.

2) A second and highly important distinction is to be found between those who welcome or fear an increased influence and more significant role for science and technology (the "technophiles" and the "technophobes").

3) A third difference in perspective lies between those whose primary concern is the preservation of the traditional academic independence of scientific activity in the face of the pressures of contemporary society, and those whose concern is to preserve the traditional democratic forms of decision-making from--to use Eisenhower's words--"a scientific-technological elite."

4) Finally, there is a distinction which can be made between the analytical perspectives of the different theorists with regard to the role of science in society. A basic distinction, it would seem, is that some are primarily concerned with what they see as an accumulation of power by scientists and technical "experts" in their roles particularly in government and industry. Others are more concerned by what they describe as the spread of scientific or

technological methods and perspectives in various areas of thought and decision-making. They argue that the outcomes of many individual decisions shaped, or at least influenced, by these perspectives has a cumulative effect of eliminating humanistic and qualitative factors. The former group tend to speak in terms of technocracy; the latter in terms of a "technological imperative." The first group stresses concrete structural factors; the second focuses attention on generalized cultural values.

Unfortunately, the different works on this subject, many of which represent good scholarship and are highly suggestive, have not yet congealed into any sort of discipline.² There is little sign of agreement on the part of the various scholars even on what constitutes an appropriate method for investigation of this range of concerns. Because of this, works on this subject seldom build directly on previous efforts. The lack of an accepted conceptual framework means that even those who are in agreement too often appear to be repeating each other rather than critically developing previous insights and findings.

Perhaps because of the very broad range of concerns, no generally accepted analytical framework has emerged. In fact, the usual approach has been to ignore the need for such a framework, relying instead on general statements followed by random examples. Such work often makes for convincing reading but provides little direction as to how the given analysis can be extended--or, in the end, verified. This is not surprising, especially since in analyzing something as all-encompassing as science, it becomes very difficult to disentangle one's own scientific method from the subject being investigated. Hence it becomes less objectionable to suggest no clearly identifiable

scientific method need be followed. It is here that a contribution, however modest, may be made. An analytical approach which may provide a framework in which to view and potentially gauge the impact of science on society will be outlined. Central to it is the concept of scientific authority.

The opening chapter considers the development of science in America, and the major changes it has brought, particularly in the relationship between science and technology. We briefly trace the development of science in Western society and in the United States, and outline the organization and activity of American science in the postwar era in order to show that major changes in the organization and pattern of scientific activity have taken place in the U.S. Relying mainly on summarizing the general conclusions of students of this question, we seek not to resolve the rather abstract question of whether there has been a societal transformation, but simply to gain a clear picture of significant trends in their broad contours. This will provide a general yet nonetheless concrete frame of reference in which to place the more speculative assertions which follow. It also begins to show divergent views.

Chapter Two takes up the great "science and society" debate on the moral, philosophical, and social implications of the situation described in Chapter One by looking at various important theoretical positions in the debate. The authors are divided into two major camps: those who fear and those who welcome a new role for science and technology. For purposes of discussion, we initially separate those whose concern is for the preservation of science from society from those who seek to preserve society from science and technology. We then construct a matrix in an attempt to clarify what the various

positions have in common and what they do not. This matrix is progressively simplified by combining categories where the differences between them turn out to be irrelevant to the basic issues. A main task is to investigate whether there is some relationship between the positions taken on questions "two" and "four" above. It is hypothesized that those who use the language of technocracy are more likely to welcome the impact of science and technology. Validation of this hypothesis leads to the conclusion that a major weakness in the debate is the general failure to make a clear distinction between differences in perspectives used to conceptualize what is from differences in philosophical positions as to what ought to be. In an attempt to overcome this weakness Chapter Three surveys the work of those who have attempted to explain systematically the workings of contemporary science.

Chapter Three looks briefly at the work of Thomas Kuhn on "normal science" and then at the school of thought from Robert K. Merton to Norman Storer termed the sociology of science, setting out each approach. The contributions of each toward possible attainment of a conceptual framework needed to overcome the theoretical weaknesses in the debate are outlined. On the whole, the work clearly provides useful direct insights into the workings of science as it affects scientists, but only indirect insights into the relationship between science and society.

The latter part of the chapter considers a recent work by Dean Schooler, Jr. intended to study the influence of science upon public policy. Schooler's method, though faulty, is instructive. The chapter concludes with postulation of certain general requirements for any necessary framework.

Chapter Four contains an argument that scientific authority is a key theoretical concept, and attempts to define and apply this concept. First authority is distinguished from power and influence. Then the properties of scientific authority are outlined, demonstrating how the elaboration of this concept takes one beyond inadequate conceptualizations of authority which typify the literature in political science. This point is illustrated through study of scientific authority at play in a description and reinterpretation of the famous Milgram experiments on human obedience. It is suggested that a framework which includes not only scientific power and influence but also scientific authority as defined, provides the basis for a conceptual framework wide enough to include the perspectives of the various positions noted in Chapter Three.

The final chapter contains an attempt to evaluate the usefulness and applicability of the approach developed through a case study.

This pilot project into the value of the proposed conceptualizations studies the workings of a key American scientific institution, and particularly the kinds of criticisms levelled at its operations. There is an attempt to discern any link between problems in its operations and the role of science by looking for manifestations of scientific authority and explaining their ramifications.

This institution is the Atomic Energy Commission of the U.S. which at many important levels was operated by scientists. We investigate the suggestion that scientific criteria or standards skewed the activities and decisions of the AEC. More important, we look into the question of whether its being defined as a "scientific" institution with a scientific *raison d'être* may have lent "scientific authority" to AEC policies and claims among non-scientists.

politicians and the general public, even when these policies were based on grounds other than scientific ones.

In the conclusion, we make no strong claims about the value or the threat of science and technology; our assertions are far more tentative and low-keyed. Nevertheless our findings do give some ground for being more skeptical about certain positions which have been taken on this question than others. We conclude by suggesting that scientific authority when conceived in the manner suggested can provide a useful means for study of the impact of science and technology upon society, and thus, for potentially lending more coherence to this far ranging and disparate debate.

Notes

1. The 1968 Biographical Directory of the American Political Science Association listed from among its then about 10,000 members 18 individuals interested in "science and government," and of those, only two, Victor Ferkiss and Carl Stover, were also interested in political theory.

2. In fact "Science and Public Policy" has been described by Brewster C. Denny as "A Literature in Search of a Field." Public Administration Review, Vol. XXV, 3, (September, 1965), pp. 239-48.

Science has served mankind faithfully and well. It has drastically extended the average life-time, shortened geographical distance, increased industrial productivity, reduced poverty, and in the long trial of war contributed significantly to the cause of freedom.... If science and technology were to founder or stagnate many of our hopes would collapse. To the extent that we neglect this source of our greatness and to the extent that we fail to preserve the conditions and openness and order that make our progress possible, we are living off the land of civilization without refertilizing it.... Instead we must bring about a new dawn of scientific progress.

Richard M. Nixon
5 October, 1968

CHAPTER ONE

The Development of Science and Views thereon in America

Science is important today in the entire developed world, and especially in the United States. But just exactly what does it mean to state this rather obvious fact: important in relation to what, and to when, and to where? Before we can begin to analyze American science, then, we must understand something of its origins. To do so we shall start with the establishment if not birth of modern science in the period of the Renaissance and Enlightenment and, very briefly, trace developments which have led up to the present stage.

Before doing this, however, we propose to provide justification for our selection of the United States as a setting for a general and somewhat theoretical analysis of scientific authority. In the process we provide some justification as well for the selection of

this subject for consideration--though the very great interest in it by statesmen and scholars is of obvious importance.

Arnold Toynbee expressed what for many is a simple fact:

"Power in the world today means good scientists and good technicians."² Leaving aside the objective validity of this position, it certainly appears to be accepted as true by political and intellectual leaders of states of different ideologies and socio-economic structures.

Here are a few examples:

It is clear that it is necessary to produce always more and always better, to save and to invest constantly and, even more, to push relentlessly our technological and scientific research, in order to avoid sinking into a bitter mediocrity and being colonized by the activities, inventions, and capacities of other countries.

Charles DeGaulle--Address to the Nation, 1964³

Vig, in his study of British science and technology, reports that "Between 1959 and 1964 the extraordinary wave of political excitement about scientific and technological revolution helped sweep the British Labour party into office."⁴ Furthermore, in the USSR the status of science underwent significant change in 1961. Under Stalin science had officially resided in the superstructure, making it a secondary and dependent variable. Suddenly it came to be considered a major element among the nation's productive forces, thereby being accorded a crucial role in shaping social reality. Khrushchev spoke of science as "the shovel to bury capitalism."⁵ In China too there has been great ideological emphasis on scientific achievement and progress.⁶

Science and technology, and large scale projects based on them,

have an important symbolic as well as practical role in nations today. For example, the role of the "Concorde" project for the French is described by Robert Gilpin.

Though condemned by many as a worthless search for prestige, it is in the area of aeronautics the functional equivalent of the space and nuclear weapons programs - namely, an instrument by which to modernize the French aviation industry. In the words of one of France's top aeronautical engineers, 'The project has been very important for us, very stimulating. It was a unique occasion to have money to push ahead on all these technical fronts. All was permitted for Concorde. "It is for Concorde? Okay."'

While French social philosopher Raymond Aron's words may typify the attitude of his countrymen, it appears as well that political leaders of many different nations accept his assertion that "Nations which deliberately reject scientific development are choosing to leave the path of history and to stagnate in a backwater. Those which reject it unwittingly would appear doomed to final annihilation."⁸ Yet not only do the nations of the world tend to accept that power and prosperity depend on a nation's scientific capacity, but they associate this capacity with the United States as the scientific state par excellence--to be both feared and emulated.⁹ Statistics bear out this assertion. In the 1930's, for instance, Europe's combined spending on science was 50 to 80 million per annum as was that of the U.S. In the late 1960's, Europe's spending had increased to \$5 billion but that of the United States to \$20 billion.¹⁰ The U.S. produces one-third of the world's scientific output in published research papers, while the second ranking USSR is only half as prolific.¹¹ While American scientists were awarded only 10 of the 103 Nobel prizes in science between 1901 and 1940, of the thirty-three handed out from 1956 through 1965, Americans won or shared in

eighteen, including eight of the eleven physics awards (physics being regarded within the scientific community as the "ultimate" or purest science).¹² My own tabulations reveal that of the 37 scientists who won or shared in Nobel prizes between 1966 and 1972, fully 24 were American.¹³

The result of this⁶⁷ condition is more of the same. As with wealth, so with scientific quality and quantity, the general rule seems to be "those who have, get." As the popular French magazine Réalités put it: "Clearly the United States has a magnetic attraction for scientists--somewhat like the attraction Italy and France held for artists and painters in the past."¹⁴ Between 1955 and 1963 some 1,500 scientists and engineers, approximately 5½ percent of the European annual output, migrated to the United States each year and it is reported that these scientists tended to be "better than average in quality."¹⁵ During this period the U.S. acquired an envied position due to its scientific and technological eminence. One detailed study of American international science policy concluded:

The font of American power has been the advance and application of scientific and technological understandings and creation; the forge of power has been the convergence of domestic and international technology in foreign national security affairs.¹⁶

Even within the United States, regions compete for scientific expenditure and immigration for the same reasons as do nations. In the words of Dr. Lloyd V. Berkner, Director of the Graduate Research Center of the Southwest:

Only those regions will be economically healthy that have the intellectual power to exploit the new science and consequent industry. Most certainly, those regions that fail intellectually will fail economically and become chronically poor and colonial to the intellectually advanced regions. This is the social certainty

that the technological revolution of our century has made clear.¹⁷

The implications of the kind of thinking evident in this statement are important and we will come back to some of them. For now, Berkner serves to close this series of indicators of the scientific pre-eminence of U.S. society. Canada, too, the homeland of the author, accepts the leadership of the U.S.A. in science as in many other areas.¹⁸ In sum, it seems clear that a mainly theoretical study of scientific authority such as ours should be best served by focusing on the U.S. as the showpiece and mecca of science. The abundance and accessibility of secondary material furnishes any additional justification which may be needed.

If the U.S. today is, as it has been called, the "nation of science,"¹⁹ where do we find the origins of this state of events? How did such great financial, personal, and administrative investment in scientific activity become institutionalized there?

The history of science goes back, one may say, as far as man's curiosity, but Western science in the form we know it took shape roughly during the Renaissance and Enlightenment.²⁰ Yet the groundwork for--as well as an impediment to--the development of science lay paradoxically in Christianity. Harvey Wheeler points out:

Prior to the advent of Christianity there had been no secular view of nature in our tradition. On the contrary, as with practically every other known culture, society and nature were intrinsically sacral.... But the early Christians ... narrowly restricted the sacral to the attributes of their own remote triune God.... St. Augustine proceeded to establish the dualistic doctrine of the divine heavenly city and the corrupt city of this world as the official world view of the Western tradition. This was ... the doctrine that made it possible for Western science to take an a-moral approach to nature.²¹

However, distinction subsequently arose in the Middle Ages between

black magic or science and white magic or religion; the former dangerous and evil, carrying out the aims of Satan, the latter holy serving the wishes of God. The development of Western science, earlier made possible, was thus impeded.

Probably Francis Bacon in England and Rene Descartes on the continent were more instrumental than any of their contemporaries in altering this basic state of affairs. Bacon forcefully argued that science was not in opposition to religion. Science investigated a far narrower range of phenomena, (in the process, of course, defining and restricting the limits and philosophy of science). Bacon added that a science allowed to function freely would gradually give birth to a better world. Descartes, living in a less tranquil and tolerant climate (France of the religious wars), sought the "benign neglect" of the state and Church authorities and went to great lengths to convince them that science was no threat to the established order.²² Bacon's efforts in particular bore fruit in the establishment of the Royal Society in England. The limited objectives and "low profile" of this group were distinctly Cartesian and so it offered some moral encouragement but little financial help to scientific activity. The Royal Society did certainly help to make the pursuit of science an increasingly respectable occupation or, more commonly, avocation. Benign neglect became the rule in England and to some extent in the other European nations of the Enlightenment.²³ Lewis Mumford argues that implicit in the thinking of the scientific pioneers of this era was a comprehension of science which carried with it a scientific conception of the world--the consequences of which have born fruit in the role of science and technology of our own era. Galileo is a case in point for Mumford:

Galileo traded the whole of human experience ... for that minute portion which can be observed within a limited timespan and interpreted in terms of mass and motion, while denying importance to the unmediated realities of human experience, from which science itself is only a refined ideological derivative.... Galileo divided experienced reality into two spheres, a subjective, which he chose to exclude from science, and an objective sphere, freed theoretically from man's visible presence, but known through rigorous mathematical analysis....

For the better part of three centuries scientists followed Galileo's lead.... They suppressed every evidence of human and organic behavior that could not be neatly fitted into their mechanical world picture. They thus committed, in reverse, the error of the early Christian Fathers who had suppressed any interest in the natural world in order to concentrate upon the fate of the human soul in eternity.... Under the new scientific dispensation it was the organic world, not least man himself, that demanded redemption.²⁴

Mumford, like other "technophobes," (as we shall see in Chapter Two) is fearful of the effects of technology and science on contemporary society, and views the development of science in this light. Nevertheless, though perhaps overstated, his arguments provide useful links by tracing the progress of ideas--particularly the ideas of science as applied to man and society--through history, thereby relating present and past. Mumford notes, for instance, the towering pre-eminence of Isaac Newton, which helped to ensure that science in the seventeenth and eighteenth centuries remain essentially Cartesian in its actual role and achievements.²⁵ Soon, however, the defenders and glorifiers of science began to develop a more and more Baconian outlook and philosophy. This is particularly noticeable in France which, according to Ben David, had achieved by the "early decades of the nineteenth century ... [a] monopolistic position in advanced training and the evaluation of discoveries."²⁶ Condorcet (1743-94), though more concrete, extreme and optimistic in his views of the future

of science than most, represents the attitude of the French philosophes toward scientific endeavour. For Condorcet, man was perfectable through scientific rationality; hence, he predicted that in the soon to come "tenth epoch" science would bring about a genuine democratic utopia. No longer would ignorance and superstition flourish fostered by the Church and a corrupt aristocracy. Science would be available to all and this new science was synonymous with reason and progress. This new era would soon succeed the contemporary period ("the ninth epoch"), which had begun with Descartes, and its imminent arrival was to mark the culmination of human history.²⁷

Similar though less utopian views dominated the French National Institute of Science and Art in the Napoleonic period and were taken up also in the writings of Henri de Saint-Simon (1750-1825) and his students. Among other claims, Saint-Simon heralded the coming of a new age and prescribed for it a new religion, the religion of Newton. Saint-Simon turned at one time to the scientists and technical experts and at another to the capitalists to effect this new technocratic state.²⁸ His students, Comte in particular, advanced the pursuit of social science, regarding it as the positive science of society and therefore part of a unified field of scientific enquiry. In outlining his positive philosophy, Comte restated and compiled many of the prevailing theoretical assumptions he saw underlying the scientific method which had developed since the earliest days.

Comte's philosophy of science can be reduced to methodological rules, all of which are supposed to be covered by the term 'positive:' the 'positive spirit' is linked to procedures that guarantee scientific objectivity (Wissenschaftlichkeit).

The contrast between the factual and the merely imaginary provides the criterion for a strict

separation between science and metaphysics. Our intelligence should direct itself at 'really attainable objects of research while ... excluding unfathomable mysteries.' Positivism wants to eliminate orientations of inquiry that are meaningless because they are undecidable; this is to be done by restricting to 'facts' the object domain (Objektbereich) of possible scientific analyses. But Comte does not attempt to distinguish between facts and fancies directly by means of an ontological definition of the factual. Everything that can become the object of rigorous science counts as a fact.... At the only level positivism allows, science can be defined only by the methodological rules according to which it proceeds.²⁹

The Saint-Simonians expected that an expertise grounded on this kind of science could better organize the rising industrial system. The legacy of Bacon and Galileo was thus borne and disseminated by philosophers and now by "social" scientists. Practising natural scientists appear to have been little affected.

During this period scientists were engaged in research and experiment, the most famous of which radically altered conventional thinking in many fields of natural science. These men and women were no doubt too preoccupied to pay much attention to the grand goals outlined for them by Condorcet, Saint-Simon and the others. Still, though their efforts changed the facts of science, it was industrialism which was changing the face of Western society and there was in fact little to link the scientist and his work directly with the social and technological changes brought on by the industrial innovators. "Until the latter part of the nineteenth century, science and technology were not only separate enterprises but the cultural matrix that has supported a flourishing scientific enterprise has not usually supported a progressive technology and vice-versa."³⁰

Neither Saint-Simon nor Comte ever found much of an audience among the scientific experts to whom they appealed. Perhaps this is

because one would be hard put to pinpoint a scientific grouping with any real organizational permanence or group identity. As Lord Ritchie Calder has noted:

The world 'scientist' did not appear in English, nor, as far as I can determine, in any other language until 1840 and, as late as 1890, the London Daily News was still deploring it as 'an American innovation.' H.G. Wells always preferred 'man of science' to 'scientist'. The distinction is important because the '-ist' meant the arrival of the specialist, and the development of specialization, each with its own self-propagating terminology. Previously, a man of science had meant a person of learning, especially inquisitive about nature but capable of sharing and explaining his findings with other educated men.³¹

Very gradually both the actual and potential connections between science and technology through "applied" science and engineering were coming to be perceived. This is the main reason why the extreme Baconian ideal of positive science as the new religion and of a ruling elite based on scientific and technological expertise never "caught on," except among essentially fringe groups such as Technocracy Inc.³² in the U.S. in the period just after World War I. This ideal was adopted by few social thinkers, the outstanding ones being Edward Bellamy and Thorstein Veblen.³³

The key scientific development in the latter part of the nineteenth century and early twentieth century was a growing self-consciousness on the part of scientists of themselves comprising a distinct community; they began to set up different national and international organizations of scientists. A parallel and no doubt related development saw scientists dividing themselves into increasingly separate and autonomous disciplines within science. Social scientists, too, were soon attempting to emulate the natural scientists. Germany led the way as the 'spiritual home and center' for world scientists during that period.³⁴

In the United States, science (as distinct from invention) remained at a comparatively low level throughout the nineteenth century.³⁵ The prevailing attitude, which de Tocqueville had described so well, featured distrust of any group with claims to special or privileged knowledge unavailable to the individual free citizen. The untrained tinkerer and inventor was respected but scientific research received little support of any kind from the people or the state. The popular response that greeted the setting up of scientific agencies such as the Geological Survey characterized this general feeling.

It could only appear to critics, such as the editors of the Popular Science Monthly, whose Social Darwinian principles usually reflected the public's point of view, that an effort was being made to establish 'scientific pontiffs' in Washington and that scientists were merely trying to gain some private advantage. The editors were willing to tolerate whatever government science activity was indispensable, but, in general, the less there was the better.... For the most part, however, their anti-centralization, anti-governmental feeling was focused on the scientific bureaucrat in Washington. The same rules should be applied to the scientific bureau as to any other, the editors reasoned: 'An unsupervised and irresponsible scientific department at Washington would be run in the interests of its sharpest managers, would be filled with sinecures, give the least results at the grandest expense, while the results would be aggravated by the sense of exemption from criticism.' The claim that scientific work should be exempt from public criticism ... was an outrage to the sense of justice of an uncomprehending public that, through both cultural heritage and training, opposed the concept of elites of any kind. The novelist, so one critic said, does not demand that he be read only by novelists. Painters, sculptors, and historians make no such demand. Why, he asked, should only the scientist insist that no one except 'his own brethren' be allowed to form an opinion of him and of his work? This was a particularly un-acceptable position, so a correspondent of Science urged, when public funds committed to allegedly public purposes were involved. Any public

matter, including the Geological Survey, 'is a proper subject of criticism, by any citizen.'³⁶

As long as this attitude prevailed, the development of American science was to be forestalled and curtailed, for such an attitude runs directly counter to a central tenet of Western science that scientific work should (and sometimes must) be subject only to the criticisms of scientists. While science grew in the latter half of the nineteenth century, the popular disdain for a leisured class and the insistence upon immediate practical results from public investments ensured that this growth was to be a slow one. For example, the American Association for the Advancement of Science (AAAS) which was founded in 1848 and whose "function (in its first 50 years) as watchman and spokesman for American science was properly exercised"³⁷ had an original membership of 640 which climbed to only 2000 by 1900.³⁸ In sum, one must conclude that in the nineteenth century, scientists largely scorned practical problems, and practical Americans broadly scorned science.

With the turn of the century there were indications of some change in the offing. As the relationship of science to technology and invention was becoming a little clearer as well as closer, thinkers and spokesmen of the Progressive movement seized upon contributions that (applied) science was to make to the future society. They heralded and sought to bring this to fruition:³⁹

The partnership that developed between science and government during the Progressive Era has therefore been an uneasy one and its legacy is still with us.... The other legacy of the Progressive Era--the image of science as wonder-worker--is also still with us, and it too has continued to create problems for the scientist in his relationship with the public.... However naive the Progressives were in their enthusiasm, they were right in their basic assumption. America in the twentieth century has become, as W.J. McGee observed two years before this century began, 'a nation of science.'⁴⁰

After World War II this legacy was finally fulfilled. The place of science in the United States in the early part of this century was ambiguous and in a state of flux. Until World War I, scientists for the most part declined the title of expert and thus disclaimed a central role in the coming society as had been envisaged by the Progressives. Any applied or popularized scientific work was still looked upon with some disdain by most scientists of this period.⁴¹ The mantle thus fell on engineers and managers.

World War I temporarily changed this state of affairs, but not momentarily. In 1916 the national Research Council was established as a separate but interrelated part of the National Academy of Sciences (NAS).⁴² While most elements of the military and military planning sections of the government were cold to scientific involvement, scientific groups made a contribution of some significance (notably, for instance, in the General Anti-Submarine Council). After the war some leading scientists such as Nobel physicist Robert Milikan strove to create a new relationship between American scientists and the public. This group called for increasing industrial and governmental support for and usage of science and for educating the public to the benefits science had brought. They sought to have this accomplished through popular science journalism and through changes in schooling techniques and curriculum.

There was much discussion along these lines in the early 1920's-- especially when the hero of the Progressives, engineer and manager par extraordinaire, Secretary of Commerce Herbert Hoover, united the individualist populist vision of America with the accomplishments of science:

The days of the pioneer are not over. There are

continents of human welfare of which we have penetrated only the coastal plain. The great continent of science is as yet explored only at its borders, and it is only the pioneer who will penetrate the frontier in the quest of new worlds to conquer.⁴³

The relationship of science to U.S. industry was extensively altered in this period. Du Pont, A.T.T., General Electric and Kodak had set up science laboratories during the fifteen years before the war (only some thirty years later than their German counterparts) but "the greatest growth of industrial research laboratories came in the 1920's...."⁴⁴

Nevertheless the goals of the scientists and Progressives could not be said to have been realized. For example, the attempt to set up a national endowment for scientific research failed miserably. The Einstein theory controversy turned many away--convinced that science would become largely the preserve of the anointed few who could understand and use the relativity concept, being able to view the physical world in a manner totally different from the normal space-time perspectives of ordinary mortals.⁴⁵ The Great Depression helped to shatter the dream of a flourishing and socially respected national science. "Technocracy Inc." faded quickly, with its early vision carried on by only a few diehards.

Still, more foundation had been laid. Many people, while still viewing science as a mysterious world open only to the chosen, could not deny the easily seen technological changes which science seemed to have brought about. Here was the basis for the "nation of science," which remained for the most part unrealized even as late as 1940.

Science in 1940 bore some resemblance to science in 1640. Social and economic conditions around the sixteenth century had brought into existence a mobile population, some of whom found in the

concepts of natural science a cognitive structure consistent with their interest in a "changing, pluralistic, and future oriented society."⁴⁶ Scientific advances led to some acknowledgement of the scientist's role "as equal in dignity and superior in the scope of its applicability to that of the traditional philosopher, theologian, or literary man."⁴⁷ By 1940, the development of science similarly was largely dependent upon the congruence of social and economic interests with those of the state. The major change to a society guided and directed by scientific means, as had been anticipated by Saint-Simon and other technocrats through the centuries, remained only an ideal of a few.

Nevertheless, a stage of sorts had been set for the development of science in the context of a wider socio-economic change: laissez-faire, entrepreneurial capitalism was giving way to a highly industrialized, bureaucratized ("rationalized," to use Weber's term) system. This system was increasingly coming to be integrated with the state--and with state-supported educational institutions. Duverger describes this politically as the change from liberal (or "pluto") democracy to "technodemocracy."⁴⁸ A basic element of this emerging system was a form of social organization better structured to make use of the applications of scientific research, to mobilize scientific efforts for specific ends, and potentially to incorporate the very organization of scientific activity. By 1940 the U.S. had established a well developed scientific infrastructure which had partially integrated university, industrial and governmental scientific activities. This integration was to be vastly enhanced by the exigencies of World War II and the Cold War which followed in the wake of that cataclysm.

We thus arrive with unseemly but necessary haste to the contemporary period, which we roughly date from World War II. To what extent can

we describe this era as different? Clearly a quantitative change has taken place as the charts below indicate, one which has occurred in geometric rather than arithmetic progression.

Figure 1-1

EXPONENTIAL GROWTH, PLOTTED
LINEARLY AND SEMI-LOGARITHMICALLY

Source: Hilary and Steven Rose, Science and Society
(Hammondsworth: Penguin, 1971), p. 4.

Figure 1-2

TOTAL NUMBER OF SCIENTIFIC
JOURNALS AND ABSTRACT JOURNALS
FOUNDED, AS A FUNCTION OF DATE

Source: Hilary and Steven Rose, Science and Society
(Hammondsworth: Penguin, 1971), p. 6.

As we can see from the above charts, the changes are curvilinear rather than linear. For instance, the membership associated or affiliated with the AAAS went from 2000 in 1900 to well over 2,000,000 in 1961.⁴⁹ This merely reflects the often repeated fact that 80 to 90 percent of all scientists who ever lived are alive today. In figure 1-1, only in the contemporary era does the curve basically alter direction. Does this fact suggest a qualitative change?

Many observers have accepted in one form or another the idea of a "second scientific revolution"⁵⁰--or a synonym, and the analysis that goes with it. There are various levels of this analysis. Globally, it is described by Lewis Mumford as the arrival of the Neotechnic phase. (In this terminology, the earliest stage is the paleotechnic while the second stage, dating roughly from the Enlightenment to this century, is the Neotechnic.) In the Neotechnic period, "... the concepts of science, hitherto associated largely with the cosmic, the inorganic, the 'mechanical' were now applied to every phase of human experience, and every manifestation of life. The analysis of matter and motion, which had greatly simplified the original tasks of science, now ceased to exhaust the circle of scientific interests: men fought for an underlying order and logic of events which would embrace more complex manifestations."⁵¹

The most important aspect of the second scientific revolution is the transformed relationship between science and technology--a partially unconscious realization in present reality of many of the aspirations of Bacon, Condorcet, Saint-Simon and Veblen. In the words of C.P. Snow:

By the industrial revolution, I mean the gradual use of machines, the employment of men and women in factories, the change from a population

mainly of agricultural labourers to a population mainly engaged in working in factories.... One can date it roughly from the middle of the eighteenth century to the early twentieth. Out of it grew another change, closely related to the first, but far more deeply scientific, far quicker, and probably far more prodigious in its result. This change comes from the application of real science to industry no longer hit or miss, no longer the ideas of the odd 'inventor', but the real stuff.... I should put it not earlier than thirty to forty years ago ... when atomic particles were first made industrial use of.⁵²

This transformation has affected the organization and expense of scientific research and the working relations among scientists. Here is a concrete instance:

In 1881, a young American physicist ... received a grant of \$200 from Alexander Graham Bell to conduct an experiment on ... the reality of the ether.... Albert A. Michelson doubted its existence, and he designed a relatively simple experiment which he thought might resolve the question unconditionally.

With his \$200 provided by Bell, Michelson made a machine of his own design ... and he took it to the observatory at Potsdam for the crucial experiment. His conclusion, published in the August 1881 issue of the American Journal of Science was that 'the hypothesis of stationary ether is erroneous'.... It was the first experiment which, as Albert Einstein remarked, 'showed that a profound change of basic concepts of physics was inevitable'....

If one contrasts this picture ... with that of the modern American research laboratory, the enormity of the changes that have come about in American science in the twentieth century will be evident. To cite one example, the discovery of the 'omega minus' which added only minutely to knowledge about atomic particles, was announced in a joint publication by 33 researchers who worked with one hundred thousand photographs taken with the aid of a thirty-million dollar atom smasher. And, since the ratio of research physicists to total laboratory staff is now about 1 in 7, a piece of research with 33 authors involves on the average 231 people in the laboratory.⁵³

A more restrictive change in the structure of scientific activity has been exemplified by a physicist at the University of California

Radiation Laboratory interviewed by Warren Hagstrom: "The only machines that could do the experiments were in Berkeley at the time. Before the machine was ever completed, physicists knew which experiments should be performed first." Hagstrom concluded:

The same form of monopolistic position exists in other branches of science. There is only one two-hundred-inch telescope, and astronomical discoveries that require such an instrument can be made only by those having access to it. Similarly, a theoretical physicist was doing meteorological research with a large computer. He was not at all concerned about the possibility of being anticipated by others because, he said: 'It's simply mechanical. The other individuals working on similar problems still await computers and won't get them for another year or two.'⁵⁴

The very high degree of contemporary specialization has been described by Director A.M. Brues:

Basic research is no longer the avocation of the professor whose job allows him time for contemplation and experimental work. It is now a well-financed industry, with the contemplation somehow getting lost from it.... It has even come to pervade universities as we see clearly from the attempts that are made to quantitate the output of those faculty members who have not gained tenure....⁵⁵

For historian Roderick Seidenberg the organizational transformation of science is crucial, and a direct consequence of the transformation of society which science had itself helped to bring about:

For though science is destined to play a dominant role in the ultimate socialization of mankind, constituting a transformation of society comparable in scale and scope only to its original formation and its subsequent evolution into civilizations ... it will itself become in time subject to the all-pervasive and converging pressure of universal organization. Having passed its effective maturity, science, like the aged, can only hope to become the ward of its own offspring. Despite the promise of its infinite reaches, it will be confined by the forces that nourish it; and under the aegis of its

social function it will no longer enjoy the venturesomeness of an earlier but passing millennium. For in this domain too, as in all others, the individual as such will have become engulfed and superseded by organized groups and phalanxes whose social direction will be held within the prescribed limits of established norms.⁵⁶

A most sweeping statement has been made by Robert Gilpin who points out why these changes are associated with World War II:

The essential aspect of the third phase of the industrial revolution is the transformation of scientific, industrial, and political institutions in order to accelerate the advancement of basic scientific knowledge and the exploitation of this knowledge for the innovation of novel and powerful technologies. This development began in Great Britain just before World War Two when natural scientists were taken into government to work on such technical problems as radar. But its full manifestations were not to be seen until during the war itself when the United States mobilized its entire scientific population and institutions for the successful exploitation of science for the military effort. The result was a change in the quality of scientific organization and in the role of science in society.⁵⁷

The substance of this mobilization has been, according to William R. Nelson, a "Scientific Revolution which ... has unfolded in three major phases ... atomic development, missile development, and computer development...."⁵⁸ World War II, with its massive expenditure on science and the corresponding massive and highly organized⁵⁹ involvement of scientists in the war effort, resulted in what has been called the "loss of innocence of American Science."⁶⁰ In addition, certain lessons about Western science have been drawn from the war-time experience. Nazi Germany had demonstrated that science and totalitarianism are not necessarily incompatible. Haberer has shown that the German scientific community was prepared to and did make almost any "Cartesian" compromise arrangement with the Nazi

government which at least minimally suited both.⁶¹ The most "ethical" of professions, medicine, along with medical research was easily perverted, turning the humanitarian hypocritical oath into a license to perform gruesome experiments on human beings.

American scientists not only built the atomic bomb, but were implicated in the decision to use it--not against the Germans who had already been defeated and in fear of whose own atomic capabilities the decision to develop it had been originally justified, but against the Japanese. In reconstructing the events of the period, historian Gar Alperovitz finds that under the circumstances the only real justification of the political decision for using the bomb against the advice of the military command was it might end the war quickly, thus keeping the Russians out of any Pacific involvement. Why then did the scientists, who built the bomb only because it was made known to them that the Germans were developing their own, go along with Hiroshima? Alperovitz states:

... the great majority of scientists ... who diligently worked in their laboratories ... made no effort to apply any diligence, intelligence, and systematic hard work in finding out what its use was to be or in organizing themselves if they did make a decision against the use of the bomb. In this aspect of their lives they did quite the contrary of that which they did ten or twelve hours a day in their laboratories.... Oppenheimer believed what was not the case because 'he had been told that' ... and on that thin belief Oppenheimer also asked other men not to go forward in their activities to stop the bombing or to have a test of demonstration.⁶²

The knowledge of the scientists' new roles and the attendant, inherent dangers have led to some soul searching but have not yet importantly affected the exercise of those roles. A bargain, however dubious, had been struck and it was to be kept. For the moment the scientists reaped the rewards of victory. In the words

of Samuel K. Allinson, Director of the Manhattan Project Metallurgical Laboratory in Chicago: "Suddenly physicists were exhibited as lions at Washington tea parties, were invited to conventions of social scientists where their opinions on society were respectfully listened to by life-long experts in the field, and were asked ... to give simplified lectures on the nucleus to Congressional committees."⁶³

The Manhattan project symbolized and spurred on a process far wider than military in its implications. The project seemingly demonstrated in starkest terms that given vast sums of money, a high level of scientific expertise and sufficient organizational capacities, then "pure" science even as abstract as the atomic theory can be rapidly applied to create a revolutionary and far-reaching technology. We have already cited the insights of Mumford and Snow as to the general implications of this fact. On a smaller scale this change has been noted in many areas. For example: "The transistor ... of all the really active cases of technical advance, this one probably contains a classic of planned research-- as a deliberate effort by the telephone company to find a replacement for the radio tube."⁶⁴ One technologist drew larger implications:

Two things happened, just about 1950. First we invented the organization of invention.... Where we don't have a body of existing knowledge, we now know how to invent it, as General Electric did when they invented the whole business of the behavior of liquid sodium in a high temperature range. They took a couple of months off and invented a branch of physics.⁶⁵

With this new phase thus came a growing role and a new recognition for scientists; also, a growing, though relatively ineffective, self-awareness among scientists of the realities that awaited them.

Science itself as actually practised and understood within the scientific community remained a mystery to the American public, but what science could do to shape the world, its powers and potentialities, became part of the conventional wisdom of the epoch.

It seems clear, then, whatever label we choose to use, that important developments were occurring in this period which not inconsiderably affected U.S. science, whether its practitioners wished this to be so or not. Science could not be easily separated from its applications, nor could the scientist toil alone, in isolation from the non-scientific world. While the image of the scientist individualist at work (like Michelson in his solitary laboratory) has been preserved, the applicability of this image to contemporary reality has become extremely limited. A Stanford physicist, looking at a two mile accelerator tunnel, mused:

We go to Congress and say, 'this is pure science. It has no practical applications that anyone can foresee....' They say, 'Yes, that's fine.' But they're really thinking about Einstein and $E = MC^2$, and about the possibility that somehow some new superweapon might come out of this. And down inside we know that's what they're thinking. Of course we take the money anyway.⁶⁶

Since science has become organized and expensive, with a direct impact on society, it is also clear that the general cultural attitudes toward science have altered as well. A picture of science, however distorted, is deeply embedded in the dominant general culture.⁶⁷

The contemporary period has witnessed a great increase in industrial use of scientists, the establishment by scientists and engineers of many successful high technology firms, a very impressive increase in the salary and pay scales of scientists in the universities as well as industry and government,⁶⁸ and an increasing use of

scientists as highly paid consultants.⁶⁹ The postwar years have seen in addition the setting up of numerous governmental agencies manned by scientists, of many scientific advisory councils established in the executive branch, and of increasing federal expenditure on science from \$74 million in 1940 to \$15.5 billion in 1965. The \$11 billion spent by the various states on science raised the 1965 total to \$28 billion. Recently federal spending alone has been estimated at approximately 17.5 billion dollars, 40 percent in research, 60 percent in development.⁷⁰

One very important indicator of the transformation which took place in this period is to be found in the organization of American science and particularly in the establishment of organizations to link science to the U.S. governmental apparatus. We will recall that in the 19th century the AAAS and NAS were founded. The AAAS represented scientists as a whole and functioned largely as an interest group/spokesman. The NAS was composed of elite scientists and served, rather infrequently, as consultant to the federal administration on particular questions requiring scientific expertise and data. During World War I the NAS added a special department, the National Research Council (NRC), specifically to fulfill this consultant function in wartime. Both organizations remained relatively small with limited activities in the interwar period. In retrospect, during the first part of this century the more important changes in American science resulted from the founding of industrial research laboratories, and from the separation of science into separate and relatively isolated sub-disciplines--with a consequent establishment of specialized organizations and journals along a model earlier developed in Europe.

During and after World War II, well established organizations such as the AAAS and the NAS grew in size and influence. The AAAS became a confederation whose constituent elements were associations such as the American Chemical Society, etc. The Federation of American Scientists (FAS), formerly the Federation of Atomic Scientists, grew out of concern about the use of nuclear weapons. Scientific university faculties mushroomed, especially after Sputnik (1957). Scientists were sought in ever greater numbers by industry and government. The so-called R. and D. industry experienced unprecedented demand in the 1960's.

While the role of science expanded and altered in industry and education, its changed relationship to government was perhaps most remarkable--and is certainly most relevant to note for purposes of this study. Let us, therefore, list and briefly describe some of the more important governmental or quasi-governmental institutions set up in this period to carry out essentially scientific tasks. A logical beginning is the military related areas. As Daniel Greenberg notes, "Perhaps the most striking feature of the first post-war decade was that the military services, recalling their prewar neglect of science and technology ... were now eager for an intimate relationship with the men and works of science and technology."⁷¹

In 1940 President Roosevelt had established a National Defense Research Committee to support and coordinate necessary scientific research on weaponry; among its members was the President of Bell Laboratories. To do the actual work recommended by this group the Office of Scientific Research and Development (OSRD) was created. The OSRD was dissolved at the war's end but was soon replaced by the Research and Development Board in 1947 to advise the Joint Chiefs of

Staff.⁷² This Board too was disbanded in 1953 as the government strove for tighter controls. The Assistant Secretary of Defense for Research and Development was made responsible for all "R and D" carried out by and for the Department of Defense (DOD). A supplement was found to be needed so the Advanced Research Projects Agency was created in 1958 and authorized to carry out research projects with its own funds independent of any given branch of the Armed Services.⁷³ That same year the National Defense Education Act was passed in response to Sputnik. It authorized grants to science students in state educational institutions. The combination of the second and third words in the act is rather telling, but few scientists protested the requirement that recipients sign loyalty oaths and affidavits.⁷⁴

Another military oriented area of scientific activity comprises the so-called think tanks. A logical place to begin is "Mother" Rand. The Research and Development Corporation was set up privately in 1948 to conduct research for the Air Force. In the mid-1950's it began to attract other military and federal clients but over half its work has remained with the Air Force. Rand is divided into disciplinary departments (physics, engineering, mathematics, social sciences, environmental sciences, etc.), and had over 1000 employees and an annual budget of over \$25 million by 1971: "In the more than twenty years since it became a corporation it has produced over 11,000 books and reports as well as countless memoranda, briefings, and communications."⁷⁵ Because the most important part of Rand's work is classified it is difficult to measure the enormous influence of its scientists and experts. Back in 1960 Joseph Kraft asserted: "Though little known, it has had an enormous impact on the nation's

strategic concepts and weapons systems, and in one way or another it has affected the life of every American."⁷⁶ Two of Rand's offspring are the Systems Development Corporation and the Hudson Institute, each with its own character but both providing a similar kind of service for government and military decision makers.

Another think tank directly linked with the U.S. military is the Institute of Defense Analysis (IDA) founded in 1956. IDA's work differs little from that of Rand except an even greater percentage is totally secret, and it appears to specialize more in operations problems which must be solved quickly. A unique entity within IDA is its Jason Division. Jason is composed of perhaps 45 leading university scientists, including several Nobel laureates. Employed mainly as tenured professors at perhaps a dozen elite universities, they spend much of their spare time consulting for IDA. In the summer, Jason members are taken to a remote resort area under heavy security to tackle important military problems as a group. The names of Jason's members are not released to the public.

Other military controlled think tanks are the Human Resources Research Office (HUMRRO), the Center for Research in Social Science (CRESS), and the Research Analysis Corporation (RAC)--an Army run think tank, as is the Army Research Mathematics Center at the University of Wisconsin (ARMC). Four other important Air Force linked think tanks are the Lincoln Laboratory at MIT, the huge Aerospace Corporation in El Segundo, California, the MITRE Corporation and Analytic Systems Inc. (ANSER), the Air Force's "short-order" think tank in Washington, D.C.⁷⁷ There are numerous other think tanks--from TRW to the Logistics Management Institute and so on--which are

in various ways closely involved with military and other U.S. governmental scientific research and development.

In sum, the new relationship of science to technology and the military demand for the newest technology, both of which had become manifest during the war, led the military and some non-military sectors of government to hire scientists and others in think tanks where they would put their scientific capabilities to work solving technological problems.

Think tanks were only one method. Since the work of science was now finally accepted as inherently useful, governmental structures had to be designed to support the growing expense of research which scientists deemed to be important for their own, that is, scientific reasons. Struggles over setting up the National Science Foundation after the war mark the point of major departure for this policy. The controversy began with the publication in 1945 of the report by leading scientist Vannevar Bush entitled, "Science, the Endless Frontier." The Bush report may be said to represent the views of most members of the then newly developed science establishment, consisting of leading spokesmen for the active wartime scientists and the large industrial corporations that were involved in scientific R. and D.

Bush's major recommendation was for creation of a virtually autonomous National Science Foundation. This recommendation was incorporated into the Magnusen bill. Congress passed this bill rather than the Kilgore bill which would have placed the NSF under much more direct government authority. The Kilgore proposal had the support of a less elite group of scientists, of small corporations and inventors. President Truman, agreeing with sentiments of this

group, vetoed the Magnusen bill. He admonished: "This bill ... would in effect vest the determination of vital national policies, the expenditure of large public funds, and the administration of important government functions in a group of individuals who would be essentially private citizens. The proposed National Science Foundation would be divorced from control by the people to an extent that implies a distinct lack of faith in democratic processes."⁷⁸ Bush's conception nevertheless won out when the NSF was finally set up in 1951.

During the delay, the National Institute of Health (NIH) began to receive large congressional appropriations for disease-oriented research, hence medical research has remained separate from the NSF. With the continuing political priority placed on medical research, the NIH has been almost as important a source of funding of science and the NSF. It might be noted at this point that although scientists were named to be director and members of the board, the NSF, was not to "... knowingly make or continue a grant to a person who is an avowed Communist or who has been established through judicial proceedings as being a Communist...."⁷⁹ Such infringement of scientific autonomy apparently did not disturb the scientific community; the AAAS in 1954 went so far as to endorse this policy for all government sponsored funding agencies.⁸⁰

A different form of government agency in which scientists have played a key role is the Atomic Energy Commission. The Commission, set up after much debate in 1946 to be in charge of development and use of atomic energy in the U.S., was composed of a board of five civilians (scientists and laymen) who reported directly to the President. The Joint Congressional Committee on Atomic Energy

was given and did exercise great power over the new commission. The AEC annually spent 2 billion dollars, most of it directly to finance research conducted by private industry and universities. The AEC also operated six large "multi-program" laboratories such as those at Oak Ridge and Livermore. Oak Ridge alone employs 5000 people.⁸¹ (In Chapter Five we shall look at the AEC more closely, in the process explaining why in early 1975, the AEC was replaced by two new agencies: the Nuclear Regulatory Commission (NRC) and the Energy Research and Development Administration (ERDA).)

Another important scientific agency is the National Aeronautics and Space Administration (NASA). Until Sputnik, space was a military area and a scene of intense competition among the services. In immediate response, the Department of Defense set up the Advanced Research Projects Agency (ARPA) to unite military space efforts. However, Congress was not satisfied and sought an ongoing civilian space program. A Joint Committee was set up to oversee space policy. The government responded, heeding the recommendation of PSAC (see below) to create NASA out of the existing National Advisory Committee for Aeronautics (NACA) which had been established in 1915. It was to be headed by a director and an advisory board (NASC) with a carefully laid out liaison structure with DOD, the AEC, and other government agencies.⁸²

There are myriad other essentially scientific tasks administered by bureaus within the U.S. federal executive. Some of the more important are: the Surgeon General's Office and Public Health Service in the Department of Health, Education and Welfare, the Food and Drug Administration, the National Bureau of Standards, and the Environmental Science Services Administration which is responsible for

all activities related to meteorological forecasting. Other departments have created scientific sub-divisions. In 1963, the Department of Commerce initiated the Civilian Industrial Technology Program to be guided by a newly created Assistant Secretary of Commerce for Science and Technology.⁸³ The State Department in the early 1960's created the position of Director of International Scientific and Technological Affairs. By 1966 he had more than twenty science attaches stationed abroad.⁸⁴

Sputnik prompted the creation of science advisory machinery within the Executive Office of the President. In 1957, the President appointed the first Special Assistant for Science and Technology, and installed the Science Advisory Committee of the Office of Defense Mobilization into his own Office under the new Assistant. This created the prestigious President's Science Advisory Committee (PSAC), headed by the Presidential Science Advisor to formulate science policy. (Due to its audacity in publicly recommending against supersonic transport development--contrary to President Nixon's own position--the PSAC was firmly quashed in 1973.) A Federal Council on Science and Technology (FCST) was also created, closely linked with the NAS and composed of high level government personnel from different departments such as Commerce, Interior, etc. and agencies like the AEC and NASA involved in one way or another with science policy.⁸⁵ In 1962 the Office of Science and Technology (OST) was set up as the executive arm of the Presidential Science Advisor.

The above is a partial inventory of federal machinery, giving only some suggestion of the size and importance of science in the U.S. today. In Chapter Three we will try to integrate theoretically

some of this information in attempting to define the limits and structure of the scientific community. Furthermore we should not assume the influence of science can be perceived merely in its governmental role. We have already noted the large number of university scientists and their high prestige and employment as consultants, as well as the importance of scientists to modern industry.⁸⁶ For example, four of every ten American scientists with doctorates in physics and six of every ten in chemistry are working in private industry.⁸⁷

Still, a partial catalogue of organizations does provide one very important aspect of scientists' behaviour: there is certainly an unofficial, vague and unspoken, but nonetheless real hierarchy of American science, beginning with an elite group at the top and descending in various layers through various organizations.⁸⁸ The composition of the elite is no doubt amorphous and to some extent unknown, but the phenomenon is there. During World War II, U.S. science was mobilized by integrating elite scientists into the state and industrial apparatus. The organizational arrangements which evolved in the past thirty years appear to have allowed continued, though more varied, mobilization and integration while at the same time providing for the continued growth of scientific activity and influence.

While we have made no attempt to determine whether the developments frequently described as a "second scientific revolution" do indeed constitute a revolution, it is clear they have significant and real consequences. One may assuredly say there have been far-reaching changes in:

- 1) the impact of scientific activity on U.S. society in general;

- 2) the actual form and content of that activity; and
- 3) the attitudes of public and politician toward the role and authority of science.

In the past few years science and technology have been under increasing though still relatively weak attack from various sources. The U.S. government, beginning in 1967, has become less generous in increasing scientific budgets,⁸⁹ and scientists are still receiving some bad press due to the recent discovery of the "underside" of modern technology in the various forms of pollution, etc. The youth revolting in the 1960's tended to include the scientific and technological establishments among the culprits creating and maintaining the "system" they were determined to overthrow.⁹⁰ Nevertheless, the effect of these factors, in comparison to, say, the changes in the 1940's, has been rather slight; while recent challenges may presage a new stage in the relationship of science to American society, it does not yet describe one. In fact, scientific budgets have again been climbing since 1971.⁹¹

In sum, the words of Don K. Price in his description of the role of science in the U.S. appear still to be apt and to capture the essence of the developments outlined above. For Price, the United States has a scientific establishment in a way very much analagous to established religions of other nations at other times: "The plain fact is that science has become the major establishment in the American political system; the only set of institutions for which tax funds are appropriated almost on faith, and under concordats which protect the autonomy, if not the cloistered calm, of the laboratory."⁹²

Having briefly described the historic development and major

transformation of American science, we now turn to the social, political, and philosophical implications of science-society dynamics, beginning with an assessment of the work of some of the more important of the many scholars who have written on this question.

Notes

1. Quoted in Philip Handler, "The Federal Government and the Scientific Community," Science, Vol. 171 (Jan. 15, 1971), pp. 150-51. Nixon, it should be noted, was widely regarded by scientists as unusually unreceptive and unsympathetic to science.
2. Quoted in R.V. Banks, "The Thaw and the Future of Eastern Europe," Encounter (August, 1964), p. 27.
3. Cited in Robert Gilpin, France in the Age of the Scientific State (Princeton: Princeton University Press, 1968), p. 39.
4. Quoted by N. Vig, Science and Technology in British Politics, as reviewed by A. Allen in Minerva, Vol. 8, No. 4 (October, 1970), p. 602. See also R. Williams, "The Technological Society and British Politics," Government and Opposition, Vol. 7, No. 1 (Winter, 1972), pp. 56-84.
5. S. Dediger, "Soviets Take a New Look at Science," Bulletin of the Atomic Scientists (March, 1965), p. 41. See also D. Hollaway, "Scientific Truth and Political Authority in the Soviet Union," Government and Opposition, Vol. 5, No. 3 (Summer, 1970), pp. 345-67.
6. See Quotations (from Mao Tse-Tung) (Peking: Peking Press, 1968), e.g.: "For the purpose of attaining freedom in the world of nature man must use natural science to understand, conquer, and change nature and thus attain freedom from nature." (p. 204)
7. Gilpin, op. cit., p. 339.
8. R. Aron, "Rationality of Modern Society," Bulletin of the Atomic Scientists (January, 1964), p. 23.
9. Gilpin, op. cit., pp. 5-6.
10. Ibid., pp. 26-27.
11. Ibid., p. 32.
12. See Spencer Klaw, The New Brahmins (New York: William Morrow, 1968), p. 129.
13. The World Almanac (New York: Newspaper Enterprise Association, 1973), pp. 402-03.
14. Quoted in ibid., p. 133.

15. Gilpin, op. cit., p. 29.
16. M.W. Wachs, "The Force of Power: American International Science Policy and Administration" (Ph.D. Dissertation, American University, Washington, D.C., 1968), from the abstract.
17. Cited in C.V. Kidd, "The Growth of Science and the Distribution of Scientists Among Nations," W.R. Nelson, ed., The Politics of American Science (New York: Oxford, 1968), p. 89.
18. See G.B. Doern, "Scientists and the Making of Science Policy in Canada" (Ph.D. Dissertation, Queen's University, Kingston, 1969). Doern notes (p. 417) that: "While our scientific community has prided itself ... on the international reputation it has ... the appropriate degree of 'internationalness' depends on whose point of view one has in mind. From another point of view Canada has become a scientific colony. While the entire object of science policy-making is to make national science policies, this study has stressed the degree to which it appears that Canadian society does not see science as Canadian science.... Canada has been especially exposed (albeit, without much objection) to the immediacy of American science and technology.... The Science Council's present emphasis on space is more a reflection of the cultural saturation of the American space success than it is of an urgent Canadian goal."
19. See below, page 23.
20. "The takeoff into continuous accelerating growth of science occurred ... between the fifteenth and seventeenth centuries." Joseph Ben David, The Scientist's Role in Society (Englewood Cliffs, N.J.: Prentice Hall, 1971), p. 170. As Joseph Needham's works on Eastern science, e.g., Science and Civilization in China (Cambridge: Cambridge University Press, 1954, 1965), have shown, until the 16th century the West lagged behind the Orient in science and technology.
21. Harvey Wheeler, "The Constitutionalization of Science" (Unpublished Manuscript, Santa Barbara, Calif., 1965), p. 4.
22. On this subject see Gerald Haberer, Politics and the Community of Science (New York: Van Nostrand-Reinhold, 1969).
23. See Sanford A. Lakoff, ed., Knowledge and Power (New York: The Free Press, 1966), pp. 11-14.
24. Lewis Mumford, The Myth of the Machine: The Pentagon of Power (New York: Harcourt, Brace, Jovanovich, 1970), pp. 58-59.
25. Ibid., pp. 87-88.
26. Ben David, op. cit., p. 172.
27. Lakoff, op. cit., pp. 14-17.

28. Ibid., pp. 19-22. See also Alvin Gouldner, The Coming Crisis in Western Sociology (New York: Basic Books, 1970), pp. 96-98.
29. J. Habermas, Knowledge and Human Interests (Boston: Beacon Press, 1971), p. 74.
30. Thomas Kuhn, quoted in Gilpin, op. cit., p. 22.
31. Lord Ritchie-Calder, "Science and Human Rights," Montreal Star (November 19, 1971), p. 9.
32. A formal movement, organized in the United States to further this position early in this century, is described in Henry Elsner, Jr., The Technocrats: Prophets of Automation (Syracuse: University of Syracuse Press, 1967).
33. See Lakoff, op. cit., pp. 31-36.
34. Ben David, op. cit., p. 172.
35. "The general indifference to basic research displayed in the United States during the greater part of the nineteenth century is a significant phenomenon in the history of modern science. In contrast, the record in applied science--in technology, inventions, surgical practices, was a distinguished one." R.H. Shyrock, "An Indifference to Basic Science During the Nineteenth Century," B. Barber and W. Hirsh, eds., The Sociology of Science (New York: Free Press, 1962). See also G.H. Daniels, American Science in the Age of Jackson (New York: Columbia University Press, 1968), p. 33.
36. Daniels, ibid., p. 287.
37. Henry Fairchild on the first half-century of the AAAS in Science (1924), quoted in "A History of the AAAS," Science for the People, Vol. II, No. 4 (December, 1970), p. 16.
38. Ibid.
39. See R.C. Tobey, The American Ideology of National Science (1919-1939) (Pittsburg: University of Pittsburg Press, 1971).
40. G.H. Daniels, Science in American Society (New York: Knopf, 1971), p. 315.
41. See Tobey, op. cit., pp. 3-17.
42. See Don K. Price, "The Republican Revolution," Nelson, ed., op. cit., pp. 5-25.
43. Quoted in Tobey, op. cit., p. 178.
44. Ibid., pp. 6-7.
45. See ibid.
46. Ben David, op. cit., p. 170.

47. Ibid.

48. Maurice Duvenger, Modern Democracies: Economic Power vs. Political Power (Hinsdale, Ill.: Dryden Press, 1974). For a highly suggestive theoretical discussion of this change based on an interpretation of Weber and Marx, see Jurgen Habermas, "Technology and Science as Ideology," in his Towards a Rational Society (Boston: Beacon Press, 1971), pp. 81-122.

49. Cited in G. Haberer, "Politics and the Community of Science," American Behavioral Scientist, Vol. 9, No. 10 (May, 1967), p. 22.

50. This expression was coined by Norbert Wiener in The Human Use of Human Beings (Boston: Houghton Mifflin, 1950).

51. Lewis Mumford, Technics and Civilization (New York: Harbinger, 1963), p. 58.

52. C.P. Snow, The Two Cultures (Cambridge: Cambridge University Press, 1964), p. 30.

53. Daniels, Science in American Society, op. cit., pp. 316-17.

54. W.O. Hagstrom, The Scientific Community (New York: Basic Books, 1965), pp. 76-77.

55. Quoted in R. Brady, Organization, Automation and Society: The Scientific Revolution in Industry (Berkeley: University of California Press, 1961), p. 298.

56. R. Seidenberg, Post-Historic Man (Chapel Hill: University of North Carolina Press, 1950), pp. 198-99.

57. Gilpin, op. cit., p. 24.

58. Nelson, op. cit., p. 3.

59. "We find, for example, that during World War Two, the 30,000 scientists in the office of Research and Development were controlled by about thirty five men in senior positions: this minority ... generally ran things according to 'convenient, authoritarian military liasons.'" Brady, op. cit., p. 51. Until the 1930's, it should be noted, the major experiments in atomic physics could be accomplished at little cost; thenceforth, the ability to think big and to spend big, became a sine qua non. The ability of the U.S. in this direction, along with the acquisition of emigre European scientists thanks to Hitler, largely accounts for the success of the Manhattan Project and the postwar scientific preeminence of the U.S. See R.W. Reid, Tongues of Conscience (New York: Walker and Co., 1969), pp. 154-85.

60. "Hiroshima is often spoken of as the scientists' equivalent of the apple in the Garden of Eden, the eating of which ends innocence by imparting a knowledge of good and evil." Victor Farkiss, Technological Man (New York: George Braziller, 1969), p. 45.

61. For a discussion of Descartes' compromises as well as a summary of Nazi science, see Haberer, Politics and the Community of Science, op. cit.
62. G. Alperovitz, "History of Atomic Diplomacy," J. Allen, ed., March 4; Scientists, Students, and Society (Cambridge: MIT Press, 1970), pp. 166-68.
63. Quoted in J.S. Dupree and S. Lakoff, Science and the Nation (Englewood Cliffs: Prentice-Hall, 1962), p. 104. "Leading scientists were suddenly thrust into the limelight and emerged as public figures of an almost heroic cast." Lewis Coser, Men of Ideas (New York: Free Press, 1970), p. 305.
64. Quoted in "International Science and Technology" (magazine), ed., The Way of the Scientists (New York: Simon and Shuster, 1962), pp. 12-13.
65. B.J. Muller-Thym, "The Management of Business," R. Kostelanetz, ed., Beyond Left and Right (New York: William Morrow, 1968), p. 97.
66. Quoted in Klaw, op. cit., p. 265.
67. "The most striking element was science." This sentence, in the Columbia Journalism Review (Summer, 1962), summarized the major finding of change in a selective sampling of United States' newspapers dated January 11-12, 1947; and January 18-19, 1962. The dates were those of the President's annual budget message to Congress. Recounted in H. Kriehbaum, Science and the Mass Media (New York: New York University Press, 1967), p. 166.
68. See Klaw, op. cit., pp. 75-77. See also Harvey M. Sapolsky, "Science Policy in American State Government," Minerva, Vol. 9, No. 3 (1971), pp. 321-48.
69. Klaw noted that the usual fee paid to a full professor as a consultant was \$250 per day in 1966-67. The average total salary for a professor was \$26,300 at a state university and \$30,200 at a private university in that academic year. Klaw, op. cit., pp. 276-77.
70. D.R. Fleming, "The Big Money and High Politics of Science," Nelson, ed., op. cit., p. 299. The latest Federal Government figures are from: Statistical Abstracts, 1974 (Washington, D.C.: United States Department of Commerce, 1974), p. 533.

AGENCY	RESEARCH AND DEVELOPMENT (in millions of dollars)		
	1960	1971	1974
Total.....	7,300	15,300	17,588
Departments:			
Agriculture.....	121	312	389
Commerce.....	30	114	192
Defense.....	5,553	7,562	8,676

Health, Educ. and Welfare....	292	1,287	2,191
National Institutes of			
Health.....	256	900	1,643
Interior.....	63	176	283
Transportation.....	-	469	342
Atomic Energy Commission.....	762	1,303	1,431
Environmental Protection Agency	-	101	180
National Aero. and Space Admin.	347	3,338	3,104
Office of Economic			
Opportunity.....	-	75	-
National Science Foundation....	58	335	460
Veterans Administration.....	15	61	85

71. Daniel S. Greenberg, The Politics of American Science (London: Penguin, 1968), p. 167.

72. Dupree and Lakoff, op. cit., p. 10.

73. Ibid., pp. 35-37.

74. Ibid., pp. 54-59.

75. Paul Dickson, Think Tanks (New York: Atheneum, 1972), p. 53.

76. Quoted in ibid.

77. See ibid., pp. 133-71.

78. Quoted in James L. Penick, Jr., ed., The Politics of American Science, 1939 to the Present (Chicago: Rand McNally, 1965), p. 87.

79. "A History of the AAAS," op. cit., p. 16.

80. Ibid.

81. Klaw, op. cit., p. 162.

82. Enid B.K. Schoettle, "The Establishment of NASA," in Lakoff, ed., op. cit., pp. 162-270.

83. Penick, op. cit., p. 239.

84. Eugene B. Skolnikoff, "Scientific Advice in the State Department," Nelson, op. cit., pp. 315-368.

85. Penick, op. cit., pp. 67-68. See also C.W. Fisher, "Scientists and Statesmen," Lakoff, op. cit., pp. 315-368.

86. "Much of the recent growth in research and development activities has been due to the nation's military and space efforts, but an increasing portion of it now stems from other needs. New products in the computer industry, the aviation and drug industries, in radio and television and in the food industry are primarily directed toward the civilian market." (then) Presidential Science Advisor, Jerome

Weisner, Where Science and Politics Meet (New York: McGraw-Hill, 1963). pp. 146-47.

87. Klaw, op. cit., p. 180.

88. See Haberer, "Politics and the Community of Science," op. cit., pp. 22-23.

89. "The U.S. budget for science has fallen from 2.5 percent of the gross national product in 1965 to 2 percent in 1969." Alvin Weinberg, "In Defense of Science," Science (January 9, 1970), p. 141. A further drop resulted from disillusionment with the space program--which took 4.3 percent of the federal budget in 1965 but fell to an estimated 1.1 percent by 1975. Sources: The Budget of the United States Government, 1974 (and 1975) (Washington, D.C.: Government Printing Office, 1973, 1974).

90. See below, Chapter Two, footnote 86.

91. Carl Kayser, "Governmental Scientific Research: Some Unanswered Questions," The Public Interest, Vol. 24 (Summer, 1971), pp. 80-85; The Budget of the United States Government, 1974, op. cit., p. 69; Statistical Abstracts 1974, op. cit.

92. Don K. Price, "The Scientific Establishment," R. Gilpin and C. Wright, eds., Scientists and National Policy Making (New York: Columbia University Press, 1964), p. 20.

CHAPTER TWO

Science and Society: A Review and
Analysis of Differing PerspectivesINTRODUCTION

In light of the structural and cultural changes outlined above, it comes as no surprise that the postwar period has seen an extremely significant surge of intellectual interest in the entire question of the interrelation of science and society. This chapter describes, compares, and classifies some of the major statements on this subject. Obviously, the discussion here must be extremely terse-- this cannot be avoided, since a comprehensive treatment would require a work of encyclopedic magnitude.

For present purposes we are primarily interested in major ideas which span many different works. We shall set them into a tentative framework, from which we can derive conclusions pointing to some of the more useful avenues of exploration which may lie open in this field, as well as noting some likely dead ends. As noted above, while much of this work is of great interest it has not congealed into a distinct field of investigation. There have been developed no suitable models, methods of investigation, and generally accepted principles enabling successive works to build directly on the findings of already existing work, testing, discarding the invalid, and further elaborating and clarifying scholarly conclusions. Instead, too often the various works seem to start from point zero. Consequently,

thinkers whose positions are in direct conflict too often miss each other completely in their arguments, since they lack even the rudiments of a jointly accepted framework from which to contend.

These generalizations will be made more concrete below as we consider some of the key works, and begin to construct a classification of positions, in order to isolate some of the debate's more central aspects. This will enable rudimentary development in the chapters to follow a conceptual framework which will contribute to a debate made crucial by the very real changes we have seen to take place in our time.

Apart from the wealth of excellent material on this subject, there is a great deal more that is far less useful. This issue has become something of cause célèbre for the very good reason that it has attracted a tremendous amount of public attention. This has led to a rather unfortunate state where the student is obliged to wade through reams of mediocre material, much of it repetitive. There are many books, for example, which are simply transcripts of numerous symposia on "Science and the Future of Man," "Science and the Environment," "Technology and Man's Future," etc., or panels of "experts" on more specific issues such as atomic energy, technological forecasting, etc. Here and there in this material are to be found fragments of great interest, but the terrain is largely barren. Self-styled experts and a non-field of "pop-sociology" have proliferated. Were the subject not of such crucial importance, and were this writer not convinced that a useful contribution can still be made, the subject would have been abandoned long ago due to its sheer popularity.

To begin to break down this sizeable body of literature into recognizable analytic positions, we will begin by dividing the topic

into two separate general issues--but will also show they are inter-related. On the one hand there is much concern with the influence of science and technology on society, on the social structure, and on its cultural values. On the other hand there is a smaller body of literature dealing with the way in which, if at all, the organization, structure, and values of society affect or should affect activities of scientists and the workings of the scientific community.

To focus upon differing perspectives whereby attempts have been made to place the subject area into intellectual contexts requires examination of a long standing and wide ranging intellectual discussion. The intent is to provide analysis enabling us to distinguish major points of disagreement from those less important, to allow some clarification of the bases for these disagreements, and to provide a rationale for selection of those elements most useful for purposes of understanding scientific authority. Initially a rather complex matrix will be formulated in order to provide some assurance that all positions which may have received support with some apparent logical consistency are not inadvertently omitted.

A survey of the literature reveals three basic issues or elements on which exponents of varying positions on both questions tend to be divided. These will be conceptualized and illustrated through two eight-box matrices, one for each question. Each is derived from setting out all possible combinations of positions on the three basic elements to be defined. The sixteen initial positions all have at least an apparent logical self-consistency and distinctiveness and so each can be distinguished from the others. This we do at the outset of each part of this chapter.

Brief exposition of the main debate's contributors' ideas is required, and each is included under one of the sixteen categories. Having completed this survey, we are then in a position to reevaluate each position. This will involve judgment as to whether positing them as viable and distinctive is actually warranted or whether they should be excluded. There are three general criteria upon which this decision will be based. The first is simply whether such a position has in fact been articulated and has thus been introduced into the debate. If we have found no serious exponents of a given position, then there is a good reason to exclude it from further consideration. The second criterion is whether the position is both internally consistent as well as logically distinctive from the other positions. A position that turns out to be clearly logically inconsistent or to be easily encompassed under the discussion of another position is one for which exclusion is also justified. Finally, even if a position is widely held, distinctive, and not self-contradictory, it can be excluded on a third ground--that of irrelevance. An argument which cannot be refuted by evidence because of its high level of abstraction, or 'mysticism'; and is thereby ultimately beyond debate, is thus also to be excluded. Normally, we shall find a rough correspondence between the three factors. A position which calls for exclusion on one ground tends to do so on others. But this need not be and is not always, the case.

Such a procedure may seem a long route to cover a short theoretical advance, but it does have the advantage of exposing not only the content of these positions but also the vacuity of some. We are concerned also with their derivation and the general way each is articulated and defended so as to ensure insofar as possible that

the range of perception and conception in the area of science and society relationships is not arbitrarily slighted, as has heretofore generally been the case.

In the concluding section of the chapter we shall bring together the remaining positions on the two questions--attempting to consolidate them further in terms of specific strategic alternatives to the central issue, somewhat more narrowly defined. Then we will be better able to draw conclusions on the debate, its achievements and limitations, and to set out more clearly our theoretical goals to be explored in following chapters.

PART ONE

We investigate here the less comprehensive question concerning the effect of society on science. The first task is to build upon the modicum of agreement which does exist by distinguishing two basic positions which we label Baconian and Cartesian. The exponents of these two philosophical viewpoints are further divided according to their views as to present conditions. Do they see the contemporary situation as desirable in that it meets their Baconian or Cartesian goals? If so they are labelled as "approving," or "optimistic." Conversely are they pessimistic in that they express disapproval when measuring what is against what they believe should be? There are also those whose assessment of the present situation places them in an intermediate position. They see their goals being realized but not sufficiently, and are therefore concerned.

Finally, we add another dimension by dividing the exponents of each position between those who view existing conditions in micro-cosmic and more voluntarist terms and those who see them in macrocosmic

and hence more deterministic terms. The former tend to be specific, concrete, and immediate in their descriptions and prescriptions while the latter see reality and change in more global and all encompassing terms.

The most basic and "polar" distinction in viewpoint and perspective, as many writers have observed, is to be found between the Cartesian and Baconian positions with respect to science. The Baconian sees science essentially as a handmaiden of society, providing for its needs, and serving its articulated goals. The Cartesian (others have termed it "Newtonian") sees science as self-justifying, essentially irrelevant to the greater society, and essentially providing inner rather than utilitarian truth. Applying first this focal distinction, and next the two others mentioned above onto a matrix, we arrive at the following configuration of possible analytical positions on the issue of society's effect on science:

Figure 2-1

	VOLUNTARIST (MICRO)		DETERMINIST (MACRO)	
	Approve	Oppose	Approve	Oppose
	(Optimistic)	(Pessimistic)	(Optimistic)	(Pessimistic)
B A C O N I A N	(1) Baconian Activism	(2) Baconian Oppositionism	(3) Baconian Determinism	(4) Radical Oppositionism
C A R T E S I A N	(5) Cartesian Activism	(6) Cartesian Oppositionism	(7) Cartesian Determinism	(8) Scientific Primitivism

It should be reiterated that these positions are initial analytical categories and we shall see that many writers are equivocal, or take intermediate stands. With this proviso, let us spell out minimally the content of each position:

1) Science in America today serves many useful purposes, providing the basic knowledge applied to create the technology which the society requires. While the application is not always optimal, it is generally positive and can be improved through making vigorous and proper use of normal societal decision making mechanisms. American society has wisely chosen to utilize science beneficially. Let us call position '1' Baconian Activism. (Note that we use the term "activism" not to denote militant activity, but rather active affirmation.)

2) The situation described under position '1' is possible and indeed desirable but its attainment is still in question in the U.S. today. Needed scientific research and applications to meet genuine social needs are not being sufficiently carried out. Instead too much scientific work with dubious, even harmful, effects takes place. To achieve situation '1' a reorganization of societal priorities vis-a-vis science is required. Scientists must apply their expertise wisely and widely. Let us call position '2' Baconian Oppositionism.

3) Position '1' is ethically and historically accurate because the natural development of science to date has been such as to require this role. The positive duty of all scientists is to remain true to their historically preordained mission to bring the world out of the paganism of politics and into the coming rule of scientific rationality. This uncommon view is termed Baconian Determinism.

4) Position '1' is descriptively false. In fact, the role science is forced to play within the prevailing social system will

continue to result in gross misuse and misapplication of science and technology. The entire system must therefore be radically reformed or overturned, and with it the entire scientific structure, if science is to be permitted to provide genuinely humane service for the society. This position is termed Radical Oppositionism.

5) Science, according to these Cartesians, may indirectly make some contribution to society but it is exceedingly dangerous and thus undesirable for Baconian tampering with the workings of science. Science is justified internally as leading to truth--which is inevitably sought by man. To direct it toward serving societal needs would be to violate its nature. Applications come more or less randomly, as the spinoffs from curiosity-oriented scientific research. Not advocating any significant oppositional role for science in the U.S. today, these writers indicate some satisfaction that what they wish conforms to reality--a contention not supported by our conclusions in Chapter One. We call this position Cartesian Activism.

6) View '5' unfortunately does not coincide with the facts. The prevailing order has so prostituted science that strong action must be taken to restore the essential autonomy and integrity of the "Republic" of pure science. This view will be called Cartesian Oppositionism.

7) The very nature of science ensures that it will remain independent of societal control. This is as it should be, for built into the workings of scientific activity is independence and purity. If research is scientific it cannot be "mission oriented." This rather unusual position is Cartesian Determinism.

8) Subservience to voracious societal demands is built into the very nature of large scale, organized scientific inquiry. As

society has evolved to its present complexity and interdependence, inevitably science has come to be socially and politically controlled. To restore pure science requires the virtually impossible restoration of a less complex and thus purer society. This radical view we call Scientific Primitivism.

Of the eight positions some are more popular than others, some more consistent with the facts as we know them, but each has some internal logic and, in almost every case, at least a few exponents. We now turn to the different writers on this subject by approaching their work according to the above categorizations.

1) Baconian Activism

This outlook, has had exponents through the centuries, but apparently only with the "second scientific revolution" has this position come into its own, and is now held by many leading scientists. We shall concentrate here, as in all of Part One, mainly on opinions of scientists since few non-scientists are concerned with how society affects science.

A survey of important thinkers in this area may begin with John Desmond Bernal, a highly respected British scientist, best known as the author of The Social Function of Science,¹ which has for many years had a marked influence on scientists. Bernal's thesis was that henceforth science would have to be planned, democratically to be sure, but nevertheless planned. Rational scientific strategy requires that teamwork replace individuality as the basic organizational form.

Science could not remain the private domain of a separate caste; rather it must be ultimately for everyone because properly planned science improves the lot of mankind. Bernal returned to

Bacon for his inspiration, rejecting in the process what he called the scientific vision of Newton in which the lone scientist picks up a pebble here and there on a stony beach, to be carefully and curiously examined.

Bernal's 1939 book effectively predicted the important role scientists, (among whom Bernal himself, his closest colleagues, and their American counterparts) were to play in the war effort. As noted, "Science the Endless Frontier," the report of leading U.S. wartime scientist Vannevar Bush, probably best captured and attempted to justify the newly formed relationship. Bush did not fail to mention the value of science for its own sake, but the brunt of his report was directed toward political establishment of a new policy for science as a vital instrument with which to achieve social goals. Chief among these goals, as articulated by political leaders at the time, was "national security" or defense. As Bush testified to the House Committee on Military Affairs: "In keeping the nation vigorously able to defend itself, there is no more important factor than that of scientific research.... Continuing research is, therefore, essential if we are not to be hopelessly outclassed in technology, weapons and equipment...."²

Bush's testimony, according to many accounts, was of considerable historical importance. Similar words have been uttered countless times by scientists justifying military or other government funding to support research projects--and for good reason, as President Kennedy's science advisor, Jerome Weisner, stated:

"While the scientist and the engineer follows his career ... because he enjoys it as a creative activity, and a pleasant way to spend a man's life as a constructive member of society, I don't believe this is why the

country is prepared to expend these vast sums of money, because we don't support art, music, or other creative, enjoyable activities to the same extent. We do so because we have learned that science makes it more and more possible to do the things we do.³

Such arguments are usually less the expression of a clear philosophical position than a dependable rationalization--which is sometimes accurate. To depict this position for the United States today, we must therefore be highly selective, treating only acknowledged spokesmen whose work is sufficiently comprehensive for our purposes. Two scientists who clearly meet this requirement are Alvin Weinberg and John Platt.

Alvin Weinberg, long-time Director of the Oak Ridge National Laboratory of the AEC and a leading atomic scientist, is most widely known for expounding the "technological fix" concept. In a now famous essay, he argues that resolution of social problems through traditional methods of social engineering is extremely costly, time-consuming and usually fails. This is because it required forcing large numbers of people to think and operate rationally, even to change their motivations. Modern science, however, often allows "technological remedies to be found for social problems without first having to remove the causes of the problem."⁴ He cites examples of the hydrogen bomb, the IUD, and nuclear energy as real or potential technological fixes to such social problems as war, overpopulation, water shortages and air pollution.

In the technological fix, he argues, lies the major justification for science. Weinberg rejects the Cartesian view of science because it "hardly explains why science deserves more support than any other part of culture does. I have therefore tended toward the Baconian view that science is a necessary overhead activity that creates the

reservoir of knowledge upon which our new technologies draw."⁵ Admittedly, technological fixes often merely buy time, and some applied technologies themselves sometimes result in massive social problems, but this merely furthers a need for improved science and technology. He sees no possible solution to large scale social maladies which can exclude technology and the science on which it is based. To resolve major problems, Weinberg envisages huge "problem-oriented inter-disciplinary socio-institutes" of natural scientists and engineers, as well as some social scientists.

Weinberg's historical outlook is shaped by his understanding of and commitment to science and technology: "When history looks at the twentieth century she will see science and technology as its theme; she will find in the monuments of Big Science--the huge rockets, the high energy accelerators, the high-flux research reactors--symbols of our time just as surely as the Notre Dame is a symbol of the Middle Ages."⁶ In a major work, he categorically describes ideal science as hierarchically organized and motivated to resolve "extrinsically" posed questions: "Science is simply a means to help achieve non-scientific politically defined aims."⁷

Another important thinker who shares the general views of Weinberg is John Platt. Platt published The Excitement of Science in 1962 and has since often written on the theme that today's problems create the need for Baconian Activism among scientists.⁸ "These problems will require ... something very similar to the mobilization of scientists for solving crisis problems in war-time.... We are going to need large numbers of scientists forming ... research teams and task forces."⁹ Platt admits that technology may have "expanded" some of these problems but probably only through "more

scientific understanding and better technology" can they be resolved.

He declares:

The task is clear. The task is huge. The time is horribly short. In the past, we have had science for intellectual pleasure, and science for the control of nature. We have had science for war. But today, the whole human experience may hang on the question of how fast we now press the development of science for survival.¹⁰

Platt judges that ten percent of all scientists and a budget of one billion dollars per year will have to be allocated to "efforts towards looking for the deep, long-range solutions to our social crisis problems."¹¹

While many other scientists share these views, they are far from a majority. Nevertheless they are given serious attention, for instance in editorials of Science, and in other acknowledged forums of scientific opinion, so that on the whole, they have received extensive exposure.

Reference should be made here to the numerous writings and compilations dealing with the uses of technology, its future, etc. While generally and therefore far more superficially optimistic, than the work of Weinberg and Platt, at the base of much of this writing lies a similar belief in the utility and need for application of scientific knowledge on the widest scale. An edited work, for instance, containing essays by leading scientists and technicians interested in applications of science through technology, has a preface which asserts: "technology lifting the people of the world to a better and more meaningful life may well be the unlocking of the Gordian knot."¹² The contributors extol possibilities inherent in applications of scientific knowledge in such fields as aerospace, ocean development, interplanetary exploration, transportation,

systems engineering, biomedical engineering, nuclear energy, electrical communication, computers and more.

In general, then, societal needs of our time are used by activists to justify a Baconian role for the scientific community. In specific cases Baconian arguments are used continually to justify individual scientific and technological projects. Such justifications are sometimes symbiotic where not only is more basic scientific research justified as necessary overhead for technology but highly expensive technology is justified as leading to spinoffs in basic scientific knowledge.¹³ For example, the success of NASA's "Apollo" program was heralded as a scientific breakthrough and leading Texas scientist Lloyd Berkner, has said:

There are those who protest that space exploration is too expensive, that diversion of resources to this or that use would serve man's purpose. But they miss the point that without the effort to meet the challenge of these aspirations, neither space exploration or their favorite causes will be served. Human society ... rises out of its lethargy only under the stimulus of deeply inspiring and commonly appreciated goals.¹⁴

Aerospace conglomerate LTV celebrated Apollo by purchasing a full page of the New York Times which said in part: "Whenever and wherever man has gone looking before, to see what was there, he has been right to do so. And he has never gone unrewarded."¹⁵ Grand phrases such as these tend to cloud the important difference between Baconian and Cartesian rationales.

It is not surprising that Baconian Activism thinking should be particularly common among scientists holding high governmental or quasi-governmental posts. (Bacon himself was long-time minister of the Crown.). Perhaps an extreme example would be physicist Harold Brown who became Assistant Secretary of Defense. For Brown there

appears little distinction to be drawn between scientific, political and particularly military goals:

I think the behavioral sciences are only one element of that problem. I think the engineering and physical sciences also have a role here. It is worth thinking about what you can do with an individual soldier or a very small military unit by making a very large investment in them, their training and their equipment, with large firepower, good target acquisition capability ... and see how much their effectiveness can be improved.¹⁶

It would be unfair to equate superficial ("let us have more technology") views with the more thoughtful such as those of Platt; they are, however, analytically akin. If one posits a role for science as developer of needed technology and sees in the social dynamics of science no inherent obstacles to technology being put to good use, then one is, according to our classification, a Baconian Activist.

2) Baconian Oppositionism

This newer group shares a fear of the present applications of science and the entire technological drift of the society. Their deep opposition to what they see, unlike Cartesians, is not to the utilization of scientists in the realization of social goals per se. Instead, it is expressed through calling upon scientists to take more seriously their Baconian role. Fellow scientists must not simply accept any imposed direction or application for their work; they must insistently lead the way to its being applied wisely and well. Like Baconian Activists, this group holds science and scientists to have major social responsibility--but such responsibility must be expressed in opposition to the status quo. Still, they are voluntarists, neither fatalistic nor revolutionary, forming instead a "loyal" opposition.

This group may also be distinguished from Baconian Activists in

that they are more likely to act and speak collectively through an organization--which may be due to their oppositionist role. We will briefly discuss: 1) The Federation of Atomic Scientists (FAS) and the Pugwash movement; 2) the "March 4th" groups; 3) the "Crusaders;" and 4) the "Constitutionalists."

Normally, a scientist adopts this position if his specific branch of scientific knowledge is being used and applied in ways he finds unacceptable. His opposition tends first to be specific and only later, if at all, does he arrive at a more general and theoretical position of opposition. The FAS, which arose shortly after World War II to lobby against a proposed atomic energy control system (May-Johnson Bill), is a case in point. Their concern widened first to the danger of nuclear war which they proceeded to stress, particularly through publication of the Bulletin of the Atomic Scientists, but then they gradually began to widen their concern to the entire use of science and scientists. The FAS soon became the Federation of American Scientists, though the Bulletin did not add the sub-title, "Science and Public Affairs" until the late 1960's. The closely related Pugwash movement also consisted of "established dissenters" as Don Price has called them.¹⁷ Pugwash was organized in 1957 to allow scientists from different nations to meet for discussion of ways to limit dangers of nuclear destruction and, more recently, to consider other socially relevant issues in which scientists had a role to play. While the American members of Pugwash were never openly defiant of law or public policy, their general attitude and their counter-nationalism made them an oppositional force, particularly in the earlier period when cold war fears were at their highest.¹⁸

More recently, primarily as a reaction to the Viet-Nam war, a

new grouping of oppositional scientists developed. It sprang quickly into action with a "research strike" planned by the Science Action Co-ordinating Committee (SACC) at MIT for March 4th, 1970, to protest use of university scientific research by the Department of Defense for military purposes. In a letter to President Nixon's then new scientific advisor, SACC urged Dr. Lee DuBridge "to call for closer university ties not with the Department of Defense, but with the Departments of HEW, HUD and Transportation; and to promote scientific endeavor ... for its potential to alleviate human suffering...."¹⁹

Nobel biologist George Wald gave the key address to scientists assembled at MIT on March 4th. Professor Wald deplored the defense establishment's misuse of scientists--and the willingness of leading scientists to be so used. The fact that the president of the NAS was simultaneously the chairman of the Scientific Advisory Board of the Defense Department he called "shocking." He denounced two meetings of the Institute for Biological Sciences: "The first ... wasn't called 'Defoliation', no it was called 'Leaf Abscission'.... The second ... had nothing to do with biological warfare and virus infection--Oh no,--it was called, 'The Introduction of Foreign DNA'."²⁰

The March 4th actions symbolize two differing conceptions of the role scientists should play, as was made evident in a split of the organizing committee. Faculty members predominantly took a position of loyal oppositionism and were upset by the Radical Oppositionist views expressed by some of the student organizers. The faculty members were not pleased with the letter mailed to Dr. DuBridge nor the interpretations given to it. Soon afterwards this group split off from SACC, forming the Union of Concerned Scientist (UCS). The

goals of the UCS are perhaps as clear a statement as can be found of Baconian Oppositionists' views:

We call on scientists and engineers at MIT, and throughout the country, to unite for concerted action and leadership: Action against dangers already unleashed and leadership toward a more responsible exploitation of scientific knowledge.

With these ends in mind we propose:

1. To initiate a critical and continuing examination of governmental policy in areas where science and technology are of actual or potential significance.
2. To devise means for turning research applications away from the present emphasis on military technology toward the solution of pressing environmental and social problems.
3. To convey to our students the hope that they will devote themselves to bringing the benefits of science and technology to mankind and to ask them to scrutinize the issues raised here before participating in the construction of destructive weapons systems.
4. To express our determined opposition to ill-advised and hazardous projects such as the ABM system, the enlargement of our nuclear arsenal, and the development of chemical and biological weapons.
5. To explore the feasibility of organizing scientists and engineers so that their desire for a more humane and civilized world can be translated into effective political action.²¹

Central to the ideas that unite the Baconian Oppositionists is the view that scientists have a professional responsibility to society. A major influence in the recent spread of this position has been Ralph Nader and his associates. This spread can be seen in the rise of "social conscience" committees formed by several professional associations such as the American Chemical Society's Committee on Chemistry and Public Affairs. It can also be seen in the rise to importance of individuals sometimes referred to as "Ralph Naders of science," highly respected scientists so disturbed by applications of their fields of knowledge that they practically abandon scientific research to adopt careers as "crusaders." They may limit themselves to one area or may widen their critique to more general scientific

and technological issues. Nobel physicist Hans Bethe has played such a role in relation to disarmament and ABM, as has Linus Pauling on several issues. Physicists John Gofman and Arthur Tamplin became crusaders on nuclear radiation hazards.²²

Perhaps the best example is biologist Barry Commoner, one of the scientific community's best known crusaders on ecology. His critique has continually widened, encompassing the role of science itself. He claims: "Increasing environmental pollution is evidence that our technology is, in important ways, incompetent. Behind this incompetence is ~~an~~ intrinsic weakness in science."²³ This weakness is an atomized nature which makes science incapable of dealing with wholistic problems. Commoner therefore conducts public crusades so that an informed public can help mitigate problems of today--many of which "have been generated by the progress of modern science and technology."²⁴ Commoner's views, and probably the views of many in this category, are summed up in his, Science and Survival (1963), which also provides some interesting examples and specific warnings. A scanning of chapter headings is suggestive: "Is Science Getting Out of Hand?" "Sorcerer's Apprentice;" "Greater Than the Sum of Its Parts;" "Society VERSUS Science;" "The Ultimate Blunder;" "The Scientist and the Citizen;" "To Survive on the Earth." One interesting proposal he puts forward is the establishment of science advocates analagous to legal public defenders to provide services to citizens facing technological changes.

Modalities for exercise of "social control" over science, as favored by Baconian Oppositionists, constitute a question they have generally not broached at great length. However, this has been of concern for a few social scientists and humanists whose formulations

seem consonant with this oppositionist position. One can find such ideas expressed, for instance, by Harvey Wheeler in "The Constitutionalization of Science,"²⁵ Nicholas Golovin in "A 'Fourth Branch' of Government,"²⁶ and W.H. Ferry in "Must We Rewrite the Constitution to Control Technology?"²⁷ Each seeks to elaborate a political mechanism for controlling and planning technology within the U.S. constitutional structure. The most interesting of these ideas is the proposed creation of a "fourth branch" of the U.S. government, constitutionally empowered to decide on crucial scientific and technological issues and having representation from the scientific community.

In summing up the position of this group, key words are "concern" and "responsibility" since these capture a general attitude of alarm which distinguishes oppositionists from optimistic activists. But in the end the difference tends to be one of emphasis. Hence a thinker such as Platt can be placed through time consecutively in either category.²⁸ What is common to both may be summed up in this quotation from "The Integrity of Science" a report of the AAAS "Committee of Science in the Promotion of Human Welfare" headed by Barry Commoner: "All citizens bear serious responsibilities toward maintenance of the integrity of science. The basic social function of science is the development of objective knowledge about the natural world and of means for directing this knowledge toward the satisfaction of human needs."²⁹

3) Baconian Determinism

This still uncommon view in the West can be found expressed in an optimistic essay by scientist-philosopher Stephen Toulmin. For Toulmin, science is on its way to becoming fully integrated into the

purposive institutions of society:

Pure scientific research is, and can deliberately be chosen to be, one of those new, tertiary activities by which employment and prosperity can be maintained in an industrialized society, even after both primary and secondary industries have become too efficient to occupy the available labour force. A scientific research laboratory can serve, just as well as a manufacturing enterprise, as the focus around which the life and prosperity of a community can be organized.... One can well imagine the 'laboratory town' becoming as characteristic a feature of late twentieth-century social life as the 'mill town' was in the mid-nineteenth century.³⁰

This description is not extreme in its determinism, nor is Toulmin's personal stance consistent with his Philosophy of Science (1953) which dismisses "causality" and causal laws in the development of science because events do not obey logic. To find this view directly expressed we must go back in time. Baconian Determinism dates back to thinkers like Condorcet, Saint-Simon and Bellamy who forecast the coming of a new epoch where scientist-experts would lead the way, applying a rationally determined social policy. Education and the breakdown of traditional loyalties would create a population ready to accept a rationally ordered society. Science would be a measure of good and bad, and its fruits would be reaped rationally and effectively. As a rational or enlightened world approached, or so the philosophers of the Enlightenment and their successors thought, science was to take its rightful place in society and history.

Some orthodox Marxists can be classed as contemporary heirs to this tradition. Arguing that there is a determinism in history which can be studied and understood, they conclude that proper science involves applying laws of history to the current social landscape. While in earlier stages before and during the proletarian revolution scientists play only a secondary social role, once the "final" stage

has been reached their part becomes crucial. After class conflict has been eliminated politics becomes administration and with the advent of highly technological industrial society, scientists comprise a crucial aspect in this administration. Bukharin, Soviet delegate to the International Congress of the History of Science and Technology in London in 1931 stated:

The building of science in the U.S.S.R. is proceeding as the conscious construction of the scientific 'super-structures'; the plan of scientific works is determined in the first instance by the technical and economic plan, the perspectives of technical and economic development. But this means that thereby we are arriving NOT ONLY AT A SYNTHESIS OF SCIENCE, BUT AT A SOCIAL SYNTHESIS OF SCIENCE AND PRACTICE. The relative disconnection between theory and practice characteristic of capitalism is being eliminated. The fetishism of science is being abolished. Science is reaching the summit of its social self-cognition.³¹

Such views as these have influenced some Western scientists, most notably the Bernal circle in Britain, but have had seemingly little impact on U.S. scientists. (The actual extent to which scientists believe in a foreordained culmination of history into a world ruled by science rather than politics cannot be measured. Such a faith can occasionally be elicited in private conversation with U.S. scientists who would never publish their views.)

Views based on the premises of Baconian Determinism are to be found today as in the past often expressed not by scientists but by others who would place on the shoulders of scientists often unwanted social, even historic, tasks. Such can be found, for example, among some exponents of "scientific management" in industry or among spokesmen for the aerospace industry, "we can therefore we must."³² They are expressed in far wider philosophical terms by Technophillic Determinists treated below.

In conclusion, we note that Baconian Determinism has not caught on among contemporary American scientists--which comes as no surprise given the generally poor public reception that any form of historicism has encountered on this side of the Atlantic. Furthermore, "faith" can by definition not be grounded in fact. Little is lost then by subsuming Baconian Determinism as a rare form of Baconian Activism.

4) Radical Oppositionism

This view of American science has been associated with the "New Left," especially with its adherents among younger American scientists. This group takes a posture of radical rather than loyal opposition. The Radical Oppositionists, while ultimately favoring a highly integrated science, reject the present relationship of society with science and are prepared to make few compromises. The basic structure of society, as they see it, determines that those in power, including some scientists, will continue to use science and technology as one of the means available to exploit and destroy. Only by subverting conventional scientific pursuits and turning one's attention and commitment to transforming the system radically can an acceptable relationship ultimately emerge. Such a view predominated among the student organizers at MIT. SDS leader Eric Mann told the March 4th audience:

What I'm trying to argue is that dropping out does not mean physically dropping out, because there's no place to drop out to. It means seeing yourself not as a scientist but as a radical scientist and a radical organizer.

Let me posit an alternate model. A group of people, some of whom are teachers, some of whom are scientists, some of whom are lab technicians ... all going into those jobs understanding that every single institution that they're going into hurts other people.

They work together by helping each other financially. They work together by being part of a political movement in a city.³³

Such sentiments have taken root among a small but active group of young scientists who have formed "Scientists and Engineers for Social and Political Action" (SESPA) and who publish the magazine Science for the People.³⁴

An intriguing statement of the radical standpoint has been articulated by New Left theoretician and activist Carl Oglesby who argues that "the crisis is political" and that the main effect of scientists using their expertise to solve social problems is not to resolve the problems but instead to allow scientists to "get into the Palace." "I admit, and in fact insist, that humans who do science need to transcend the horizons of its world frame and its method if they want to help get us out of our troubles. They must, however, first put off their lab coats and not try to come on the scene as if their arrival were the next thing before salvation."³⁵

John McDermott shares this view. McDermott, too, would ultimately like to see "a reorganization of technology to serve human needs" but he sees the established scientists and technologists not as concerned individuals but rather as the conscious or unconscious allies of those in power who have a vested interest in maintaining the existing structure with its inherent problems and inequalities. For him too, the crisis is political. Until the needed radical political and social changes come about, using scientists and technologists to resolve social problems is a contradiction in terms, since their organizational pattern and underlying assumptions serve to abet the system as it is and the powerful within it.³⁶

While radical, often revolutionary, in their political persuasion,

this group's view of science is nonetheless Baconian. Their strong antipathy for Baconian Activists and their view of Baconian Oppositionists as misguided if well meaning reformers stems from their structural analysis of society, not from their ultimate view of the proper role of science.

The most profound philosophical, as opposed to political, differences lie between all of the above groups on the one hand and the Cartesians on the other--to which we now turn. Unfortunately, this distinction between Cartesians and Baconians is very frequently obscured by scientific spokesmen who simultaneously take recourse to arguments from both positions to make their case.

5) Cartesian Activism

The Cartesian position is that of traditional Western science, of Descartes and Newton. Science is to be valued for its own sake, as a worthy cultural pursuit, rather than for the sake of what it produces. Science should not take its cue for directions from socially expressed needs, rather it should be inner directed, seeking its own truth according to its own standards. Any uses to which its truths may be put are a separate matter entirely remote from the concerns of the "Republic of Science." This phrase was popularized by a leading Cartesian Activist, Michael Polanyi, British scientist and philosopher. He and other scientists founded the "Society for Freedom in Science" to oppose the Baconian views articulated by Bernal and his colleagues. Polanyi sees the dynamism of science as inherent in its autonomous "laissez-faire" structure which must be preserved.³⁷ This autonomous enterprise, in the words of NSF director Leland Haworth, "has a very great intellectual and cultural value ... the same kind of cultural value that music or paintings or things of that sort have."³⁸

For noted American physicist I.I. Rabi, science precedes the arts in value because:

The arts ... are not the kind of thing that will inspire men to push on to the new heights.... The works of Shakespeare ... are really wonderful glorified gossip. I mean, after Shakespeare and others, how many more persons can say the same things...? Where do we find the really meaningful thing, the thing that will show the glory of God and the originality of nature, the profundity-- and where do we find the imagination that delves into the mysteries of life, into the existence of matter? That imagination, I would say, will be found only in science.³⁹

Harvey Brooke sees science as "one of the characteristic expressions of the highest aspirations of modern man." He too finds it superior to art or music, because science is always in principle subject to independent verification and so has a "public character still lacking in other forms of knowledge."⁴⁰

Sentiments of the well-known American mathematician, Norbert Wiener, may serve as an example of Cartesian Activist thinking. Wiener announced in the late 1940's his intention not to cooperate in any form of scientific endeavor which could be used for military purposes.⁴¹ He later explained his belief that scientists must keep themselves free from political pressure:

Historical predictions and prejudgements which belong primarily to one particular age ... may be proved false.... Thus the internal life of science must be preserved without a too direct dependence on the policies of the moment, or the official fashions of thought. This means that the scholar must retain for his own efficacy something--not too much--of the ivory tower attitude which it is the spirit of the times to decry.⁴²

Cartesian scientists often tend to speak of science in almost religious terms, as something sacred which gives enlightened meaning to life. One is reminded of Max Weber, who described "Science as a Vocation" which offers a special kind of personal freedom and provides

an understanding of reality in a world where other values seem illusory and undependable.⁴³ Perhaps the staunchest and most widely known exponent of Cartesian Activism today is physicist Victor Weisskopf who has declared:

These are the privileges of being scientists; we are participating in a most exhilarating enterprise right at the center of our culture.... We are all working for a common and well-defined aim; to get more insight into the workings of nature. It is a constructive endeavour, where we build upon the achievements of the past; we improve but never destroy the ideas of our predecessors.... The growing insight into nature is not only a source of satisfaction for us, it also gives our lives a deeper meaning.⁴⁴

The enterprise of science must be maintained and continued, but this requires huge government support. Still Weisskopf, in an editorial in Science, maintains:

The careful analysis of the problems and the necessary measures for solving them will require more, not less, of the spirit engendered in pure research, in the unbiased search of causes and effects. The scientists who are involved in fundamental research have always proved to be the best reservoir of manpower for tasks requiring objectivity, innovative ideas, and imaginative approaches.... [Therefore look to science] for its intrinsic values as well as for its role as a source of brainpower for the tasks we will face in the future.⁴⁵

This Cartesian Activism is widely held among leading university scientists in the U.S. today. It is philosophically in disagreement with the Baconian Activists and yet there is little explicit argument between exponents of each position. Apparently both groups look at American science and see different things. The Cartesian Activists see an autonomous enterprise of science, and they are interested in preserving it and the financial support they feel is required to keep it progressing. The Baconian Activists see scientists predominantly involved in carrying out tasks society has imposed, and wish to rationalize and co-ordinate scientific activities along desired lines.

For example, President Johnson's science advisor, Donald Hornig, was asked his objectives for distribution of federal research grants: "The most basic goal he could point to was this: to ensure that every person with scientific talent gets the resources he needs to do what he wants to do."⁴⁶ One wonders if Hornig never entertained the idea that soon if not now the piper's patron would begin to call the tune. Alan Waterman, when director of the NSF, expressed a position similar to Hornig's, stating that "society ... has a double obligation: to support and encourage basic science in all fields ... and, as new forces are discovered ... to apply them usefully."⁴⁷

Both Hornig and Waterman, like many leading scientists, would like to have it both ways; a Cartesian science supported in Baconian fashion. They reject the notion that one should justify any scientific work per se by its application, voicing instead the Cartesian principle that science is self-justifying--but then adding that science as a whole is socially useful in its consequences. They have generally ignored the implications of making such a concession.

Ultimately, it seems, American science cannot have it both ways. Either science is to be essentially inner-directed, and thus its applications must be accidental, and its work meagerly supported for "cultural" reasons; or it must be primarily outer-directed, serving externally defined political and social goals with visible strings attached to its massive support. (This argument is carried to extremes by Scientific Primitivists.)

Many scientists whose principled position is Cartesian adopt a more conciliatory (Activist) public position than they might wish. Though often aware of this fact, they find themselves in a position fraught with contradiction. While they want to maintain scientific

autonomy, they are at the same time fighting to preserve a rather healthy slice of public funds to carry on some very expensive pure research--for instance in high energy physics. Not wishing to offend their patron any more than absolutely necessary, Cartesians allow themselves to be convinced they are on the whole playing their own tune, and thus tolerate those (to them isolated) instances where scientific activity is directed by non-scientific bodies which operate on the basis of non-scientific considerations. If they are wrong in such assessment, they certainly risked gravely compromising their principles. While Max Weber captured the potential inner freedom of scientific work in "Science as a Vocation," the thrust of his general analysis of society constituted effectively a prediction of a heavily controlled and bureaucratized world, and implied rather strongly that this area of inner freedom was likely to be threatened by the rationalizing, organizing and integrating forces of modern society. For Baconians, this is potentially all to the good, but for Cartesians, it should much be feared; yet articulated opposition is still rare among the latter.

6) Cartesian Oppositionism

Those who hold this position share concerns for a possibly "captive" science, but they seldom if ever publicly express their views. Sharing the view of all Cartesians that science should be pure and autonomous, they feel such a situation threatened by the state of science and technology in the U.S. It appears to this writer that such a position is quite common, particularly among university scientists; many of them simply want to do science in their laboratories completely divorced from societal concerns which they believe to be irrelevant to science. We noted in Chapter One the willingness of American scientists passively to condone loyalty oaths, security

clearances, etc., in return for scientific research support. This contradictory attitude appears to us symptomatic of their silence.

One indication that Cartesian rather than Baconian views are dominant within the "silent majority" of scientists is a survey conducted in the early 1960's by Fortune magazine. A sample of scientists was asked whether or not research findings likely to be applied to produce "more evil than good" should be disclosed. Approximately 80 percent of the total sample said: "Never Withhold."⁴⁸ In a recent survey of American scientists, Marlan Elisset posed contradictory assertions:

- 1) The pursuit of science is best organized when as much freedom as possible is granted to all scientists.
- 2) The federal government should directly supervise the activities of science so its achievements are directed to socially beneficial ends.⁴⁹

The responses he received were as follows:

Question	Number	Strongly Agree	Agree	Can't Say	Dis-Agree	Strongly Disagree
1)	835	40%	37%	7%	14%	1%
2)	845	1	3	3	38	54

Elisset offered an example of the type of response he frequently encountered, from a respondent described as "an internationally famous chemist ... at the University of Texas:"

A pure scientist must not deny himself a discovery by worrying about the social consequences. He can't possibly know the practical derivatives of his work and, therefore, cannot be held responsible for the eventual use to which his discoveries may be put. Technology, however, is different. While not separable from science, it nevertheless affects society directly and must be managed by society.⁵⁰

This distinction between pure and applied science or technology is often drawn, but Elisset discounts its importance, pointing to the

almost inevitable response of scientists when asked to classify their own research: "Why pure, of course." This ambivalence pervades the Cartesian attitude. Science should be autonomous, but some societal interference is inevitable and even required, and so is tolerated in those lesser spheres called applied science and technology.

Beyond this, the shortage of Cartesian Oppositionists can be attributed in part to the fact that many scientists of that minority who come to understand the profound effects of technology, in the end adopt a Baconian orientation in spite of themselves. Only through the utilization of a restructured science and technology, they conclude, will some of the problems arising from the present situation be rectified.

Finally, some mention should be made of another group of individuals whose work perhaps best fits under this heading. These might be called muckraking opponents of the "military industrial complex." Best known among these are probably Harold L. Neiburg, Ralph Lapp, and Nigel Calder.⁵¹ They are seldom themselves practising scientists, their work is not about the uses of science per se, nor do they usually state a position outlining the principles of what constitutes a proper role for science; however their attack on the "military industrial complex" appears to rest partly upon a position of Cartesian Oppositionism. To use the title of Neiburg's major work, it is "In the Name of Science" that expensive weaponry is often justified--and they view this as illegitimate. Science, Neiburg says, can justify only science, and any appeal to science to justify military or industrial policy is an abuse of science.⁵²

To conclude discussion of this position, we note that exhaustive search of the literature failed in discovering any reasoned, explicitly

articulated statement of Cartesian Oppositionism. Evidence that such a position is held is only inferential. The silence is evidently due to the practical and political folly of publicly maintaining Cartesian Oppositionism. Because we are treating the published literature, this position must be excluded from further consideration.

7) Cartesian Determinism

There is only a small and relatively unimportant distinction to be made between a Cartesian voluntarist and a determinist whose orientation to the present situation is positive. Normally Cartesian orientation tends to be voluntarist, seeing science as a "delicate plant," which must be safeguarded from wilting under societal interference. One partial exception is Derek Price who appears to regard science virtually as a globally autonomous force with which any one society could not effectively interfere, even if it so desired. For Price, world science tends to grow according to its own inexorable laws of development. Consequently, interference in a given society is a local factor probably of little affect on this world-wide historical development.⁵³ The views of Price might reasonably be labelled as Cartesian Determinism. While this distinction is a real one, it is seldom made and in the end is one of emphasis. Price himself admits that Nazi Germany was a dangerous exception to the natural flourishing of science; he would no doubt react as a voluntarist oppositionist if American science were threatened in a similar way.

Because we could find only one ex-scientist-turned-historian who could, with reservations, be placed in this grouping, we have no viable category here. Little is lost by regarding Cartesian Determinism (category 7) subsumed under Cartesian Activism (category 5).

8) Scientific Primitivism

The Cartesian who takes such an extreme position as Scientific Primitivism is in an apparently difficult intellectual bind. He cannot find the autonomous and free science he desires. Science has become captive to societal forces, and this is an almost universal fact which scientists, however they may wish it otherwise, are powerless to alter. Such a person is compelled to reject the practice of science under present conditions, and if he is to advocate or carry on any kind of action, it is directed toward the radical transformation of the whole society. Under a new and radically different social system it could be possible to restore science to its true Cartesian standards.

Examples of such thinking are to be found among the "Technophobic Primitivists" to be treated in Part Two of this chapter. However, one thinker who deserves attention here is Eugene S. Schwartz. His introductory remarks in a major book portray this dilemma in highly personal terms:

Science, I had once thought, was the ultimate answer to understanding and freedom. Science did away with metaphysics and philosophy. It established standards for understanding a fact as a fact; it afforded a universal language in mathematics for communication. Its methods and conclusions were open for revision, and its truths were assured by the error-correcting capability of an international community of scholars.... As I probed deeper into the meaning of science I saw that the euphoric vision of science and technology is a myth--now become dangerous and threatening.... Science is a twentieth-century religion and, to paraphrase Marx, it has become a new opiate that in the short run may help to delude people to think all is well....

The thesis of the book is that the crises that threaten human survival are inherent in science and technology and are not amenable to rectification by more science and technology.⁵⁴

Schwartz's assessment of the prevailing state of American science

is sombre, but his conclusions are Cartesian, not Baconian. For Schwartz the answer to bad technology is not more science; if anything, it is less science. This is the most extreme Cartesian denunciation of the relationship of contemporary American society to science:

Although a lone scientist can conceivably still make a scientific discovery through his own resources ... the overwhelming trend in both science and technology is toward ... organization and institutionalization....

The organization of science to carry out 'politically defined aims' is a contradiction. Organization requires authority, and authority requires hierarchy, and hierarchy demands obedience. The administrative authority in scientific organizations cannot be divorced from scientific authority, for the power to make decisions invests the decision-maker with intellectual authority as well. Freedom to seek the truth must by necessity be circumscribed by the demand to perform an assigned task....

The Manhattan Project has become the prototype of the scientific organization formed to carry out political ends; compartmentalization of work, divorce of scientist from ends, renunciation of personal responsibility, obedience to authority--and secrecy. The close association between science and war and the support of science by government for national purposes has become a major component of Big Science in all technological societies....

Increasingly, science is based not on human needs or curiosity but on political aims. Scientific institutions begin to assume roles that are intrinsically inimical to science.... The acceptance of secrecy and the abandonment of Democracy to the decisions of experts undermines the integrity of science.⁵⁵

Although Schwartz is more concerned with the impact of this state of affairs on the common man than on the scientist, his position on the desirable role for science is clearly that science should return to its past condition--isolated, small, not irrelevant but free of interference. A significant and vocal minority of young scientists either lean toward or cling to this view, a view we note that places the scientist in a difficult and sometimes agonizing position vis-a-vis his own profession.

In concluding our survey of these eight positions, we might comment that strong and eloquent assertions such as those excerpted from Schwartz are sufficiently dramatic to sway lay readers' opinions and views. This should be at least equally true in Part Two below. The problem with such statements is that of substantiation. Schwartz's eloquence, for instance, does not seem to convince scientists who hold contrary positions. Indeed, we have seen little evidence of any agreement even on what it would take to be convincing enough to raise the level of debate on this question.

What we have termed Baconian Activism (1) remains among the major positions of scientists with respect to the impact of society upon science—and the ways in which scientists should respond to social needs. The analytic category of Baconian Oppositionism (2) is also of considerable, and growing, importance. On the other hand, those whose optimistic thinking lead them to a position of Baconian Determinism (3) are (at least in the U.S.) prudent enough to keep their views private. Since this is the case, they must be dismissed from further consideration. This is not the case with those vocal Baconians we call Radical Oppositionists (4); they are too many, too vocal, too articulate, and too well organized to be ignored.

Cartesian Activism (5) proved to be significant, although upon examination we found some logical difficulties with this stance. Cartesian Oppositionism (6), when looked at closely, turned out to be rarely (and then only privately) articulated, as well as internally inconsistent. Despite evidence suggesting that quite a few U.S. scientists may share this attitude, we must dismiss the category. A further argument for doing so is given below. Its significance is its potential as an intellectual trap. The analytic category of Cartesian

Determinism (7) proved virtually empty, and so it too may be dismissed. This is not so with respect to our final Cartesian category of Scientific Primitivism (8), for there is a logical argument in addition to evidence that this position is strongly held.

This argument runs as follows. A Cartesian, to be clearly and consistently such, must hold that science does not have a deterministic effect upon his society. This is the most basic underpinning of his argument for scientific autonomy. If one perceives science to have largely lost autonomy, this must have some cause other than science itself; for scientists, phenomena are not merely chance but are caused. As Albert Einstein averred, God does not "play dice with the universe." Logically, then, a Cartesian perspective coupled with perception of extensive social control over science would lead one to fault the basic social structure and functioning which must have brought about such control. A failure or unwillingness to see the necessity for such internal consistency reveals the intellectual barrenness of Cartesian Oppositionism (6).

If one is an unhappy Cartesian, then, who perceives an increasing degree of social control over science, he is logically bound to be radical in the sense of seeing scientific autonomy lost by the root cause of a faulty society (Scientific Primitivism). If Scientific autonomy is to be regained, basic social change would be required. This can only be accomplished through political action, broadly conceived.

Let us briefly sum up our analysis and findings to this point. We attempted to create a broadly inclusive analytical matrix and then to fill it with actual positions as articulated (or not) by American scientists. Our intent has been to distinguish major points of

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disagreement from those less important or vacuous. The basis on which we eliminated categories was not logical inconsistency per se—since all were in principle logically consistent. Popularity was not itself a decisive factor in eliminating categories, but inability to find any reasoned, articulate support for a category was taken to indicate a more fundamental failing. The primary exclusionary premise was essentially one of salience: does this position provide us with a meaningfully useful vantage point from which to compare and contrast other positions in light of what we understand, from the preliminary survey in Chapter One, to be the case today?

Through examination, the positions of Baconian Determinism (3), Cartesian Oppositionism (6) and Cartesian Determinism (7) were not found to be independently useful. The five remaining categories do appear useful in that they direct attention to three dichotomies which cannot be analytically eliminated and so form a basis for focus. These are: 1) the Baconian vs. Cartesian view of science; 2) supportive vs. oppositionist assessment of the prevailing situation; 3) radical vs. loyal opposition. Our new configuration appears as follows:

Figure 2-2

	<u>Supportive</u>	<u>Oppositionist</u>	
		LOYAL	RADICAL
B A C O N I A N	(1)	(2)	(4)
C A R T E S I A N	(5)		(8)

We also found that the essential theoretical distinction initially drawn between Baconian and Cartesian views of the effect of society on science has proven infrequently to divide thinkers in any explicit way. This is not due to the fact that this distinction is unsound, quite the contrary. We have seen that when the question of how are we to understand the present situation and what are we to do about it arises, Cartesian and (less often) Baconian principles tend strongly to be ignored or obscured. The two status-quo oriented groups, 1 and 5, look at what is happening differently and both accept it, rationalize it, and thus agree on the appropriateness of "science as usual." The two radical opposition groups, 4 and 8, while ultimately differing in their conceptions of the role science and technology should play, suspend this question far into the future and share condemnation of the present system. Both conclude that political and social change, rather than efforts of scientists and technologists as experts, are

required for societal transformation. Finally the "loyal" oppositionists are eventually forced to adopt some form of Baconian analysis and call upon scientists to direct their efforts, either directly or indirectly, toward meeting social needs.

To conclude this section, the following observations seem appropriate. First, not all theoretically available positions have been taken. Second, those taken have been insufficiently differentiated from each other by the participants in the "dialogue" under consideration. Therefore, no real dialogue can be said to have occurred. Those whose perceptions and viewpoints lead them to differing understandings of reality have largely failed to take cognizance of their divergent perspectives. Even where such differences are acknowledged, there has been scarcely any effort to reconcile them. No conscious attempt to relate differences to differing perceptions or even to confront each other on the common ground of political philosophy has been made. Third, this literature may be seen as distilled into rather straightforward disagreement between supportive, oppositionist and radical positions.

To have begun our analysis with concentration upon these three positions would have been to perpetuate errors which we attribute to others. Our analysis has demonstrated some of the current deficiencies in the literature which may aid those who would wish to advance controversy to the level of debate. Still, for the present--and for purposes of this work--the conclusion must be accepted that the literature on this question consists essentially of the three categories.

PART TWO

We now turn to the other side of the coin, the effect of science on society. Because of the vastness of this literature, we shall have to be much more highly selective than in Part One. The views of scientists are treated but writers of varied training, background and ideological predispositions have expressed themselves on this issue. Concerns here are those of social philosophy, and they raise "ultimate" questions on the widest historical and sociological landscape.

The Cartesian/Baconian distinction is not central to this discussion. A strict Cartesian would argue that science is an autonomous and independent enterprise with no systematic societal effect. He would accept that technology does affect contemporary society, but would consider this irrelevant to the scientific enterprise. Only Baconians, or those who accept a Baconian analysis whatever their philosophical position, concern themselves with this question. Our own Baconian stance is based on the historical picture drawn in the first chapter. Whether one would prefer a socially relevant or an independent science, one cannot convincingly deny that in the U.S. technology affects society directly and profoundly. Today technology consists primarily of the application of science not haphazardly and many decades removed, but in a planned and often practically immediate fashion. When discussing effects of science on society one cannot avoid discussing effects of science and technology on society. Furthermore, due to public support of scientific research and the consequent highly organized structure of science, the effect of the process of planning technology deeply involves scientists in myriad ways--whether they realize this or not. What this implies for

society as a whole and how scientists, citizens and decision-makers should react are questions eliciting profoundly different views.

We here proceed as above, listing the various possible positions which could be taken. This results in another eight-box matrix. The focal division is made between those thinkers we call "technophiles" who welcome, and "technophobes" who oppose, a technocratic society (for now simply defined as a society structurally and culturally conducive towards major roles for science and technology). These two groups are subdivided into those ("optimists") who see their vision as prevailing now or in the future, and those ("pessimists") who do not. A further subdivision is made between "determinists" who see the developing situation as a global phenomenon and as inevitable--barring an unexpected revolutionary upheaval--and "voluntarists" who see present structures of decision and action as possessed of a potential to assess and realize optimum societal roles for science and technology. The matrix thus formed appears below:

Figure 2-3

Voluntarist (Micro)		Determinist (Macro)	
Approve (Optimistic)	Oppose (Pessimistic)	Approve (Optimistic)	Oppose (Pessimistic)
I Technophillic Activism	II Technophillic Oppositionism	III Technophillic Determinism	IV Technological Alternativism
V Technophobic Activism	VI Technophobic Oppositionism	VII Technological Primitivism	VIII Technophobic Determinism

In brief, the minimal contents of these eight theoretical positions are as follows.

I) Technology is changing society giving us the tools and skills to create a different and better social order. We should grasp new opportunities to create structures able to maximize this potential as fully and effectively as possible. This has well begun and we must ensure it continues. We call this Technophillic Activism.

II) Holders of this position agree with the above group's vision, but without complacency. The technocratic society, seems not to be sufficiently materializing. This is typically attributed to

irrational attitudes of the general population, the naivete of politicians, the shortsightedness of interest groups, etc. In general they deplore what they see as a lack of understanding and appreciation of the real possibilities of technology. This has led to frequent misapplication of science and technology, causing environmental pollution and other abominations. The condition must and can be rectified. This is Technophillic Oppositionism.

III) These writers are even more excited by the perceived developing technocratic society but it can scarcely be harnessed by man through intentional decision and action. Rather it tends to be seen as an independent historical force shaping society which, regardless of man's will, is likely to continue to do so. This is termed Technophillic Determinism.

IV) This group could be viewed as extreme Technophillic Oppositionists. They see the present system moving almost inexorably toward ecological catastrophe, but also see any salvation lying, if at all, in science and technology applied in radically new and different ways. They tend to be active in highly unconventional forms of research and experimentation, as well as propagandizing, to lay the groundwork toward developing new technologies based on a totally different outlook in environmental planning, agriculture, education, etc. We refer to this movement as Technological Alternativism.

V) Exponents of this position want to keep science and technology "in place," but feel this is being relatively successfully accomplished and must continue. They are not Luddites; the technophobia they articulate is moderate. Not overly fearful of technocracy, they are confident that the benefits of science and technology can be reaped without threatening the existing social structure. They are also

confident that the prevailing political and educational mechanisms, as long as these are vigorously defended, can adapt sufficiently to protect traditional social values and institutions. This is Technophobic Activism.

VI) Those in this position share the "science and technology should be kept in place" view, but display no complacency vis-a-vis the status quo. With much concern they perceive society developing in ways which complement the needs and potential of technology. New structures, redirected public education, changed attitudes of political leaders, constant vigilance—these and more are required if science and technology are not to subvert human values. We call this Technophobic Oppositionism.

VII) The rare individuals in this category see science and technology almost invariably doomed as dominant forces in contemporary society and welcome this eventuality. They share the general technophobic critique of the effect of science and technology, but they see potent historical forces leading the world away from a technocratic era. We choose to call this Technological Primitivism.

VIII) Finally there are those whose dread of technological domination leads to rejection of the prevailing social system. They view the accelerating onslaught of science-based technology as increasingly incorporated within the fabric of modern society. While not dogmatic, they are generally fatalistic. Quasi-revolutionary strategies (if any at all) often seem the unlikely but only possible response. A few pray for God's intervention. They reject the moderate means advocated by Technophobic Oppositionists as either irrelevant or sometimes even complementary to technological subordination of civilization. They are of the Technophobic Determinism persuasion.

We can now describe these eight positions through a brief summary of the work of some of their chief exponents, concentrating on the positions of those thinkers we take to be focal.

I) Technophillic Activism

We find the distinction between activists and oppositionists among technophiles (as among Baconians) often turns out to be one of emphasis. Just as John Platt might be characterized as either a Baconian Activist or (recently) Oppositionist, so Kenneth Boulding might be characterized as a Technophillic Activist or (recently) Oppositionist. We describe first the major points upon which voluntarism leads both groups to agree, and later draw contrasts based on their attitudes toward the status quo.

Technophillic Activists are heirs to a legacy of views such as were propounded by Technocracy Inc. in the 1920's and 1930's, though they are not nearly as naively optimistic. Well-known political scientist Zbigniew Brzezinski is a leading spokesman who has stated the indisputable facts as he sees them:

... the approaching transformation will come more rapidly and will have deeper consequences for the way and perhaps for the meaning of human life than anything experienced by the generations that preceded us.

America is already beginning to experience these changes and in the course of so doing it is becoming a 'technotronic' society: a society that is shaped culturally, psychologically, socially and economically by the impact of technology and electronics, particularly computers and communications.⁵⁶

While Brzezinski is aware of some dangers lying ahead, especially in the struggle between technologically developed and technologically backward nations, he is quite hopeful these can be overcome. He looks forward to a developing "universal intellectual elite" and a

"super culture strongly influenced by American life."⁵⁷ This would allow safeguarding liberal freedoms against authoritarian possibilities by devising a "meritocratic democracy"—a political system objectively preferable to and more democratic than that now prevailing.

Economist-philosopher Kenneth Boulding has made similar arguments, exploring at some depth in several books the development of contemporary and future civilizations. He too is optimistic, advocating an attitude of "critical acceptance" toward the "great transition:"

The crucial problem is the development of electronics, automation, cybernation, and the whole complex of control systems.... It seems probable that all existing political and economic institutions will suffer some modifications as a result of this new technology.... That the ultimate results of this development will be benign can hardly be doubted except by ... extreme pessimists.⁵⁸

In the new "post-civilization" men must act to develop "a pragmatic social strategy that is based on the ideology of science" to ensure a "benign" society. He looks toward new visionaries, statesmen and philosophers to form an "invisible college" which will provide direction through the transition period—with himself acting as an "unashamed propagandist." (Recently, he seems to have become a little less hopeful, though his basic analytical position remain unchanged.)

The concept of post-civilization, of the arrival of a basically different and potentially better era, gained credence among U.S. intellectuals with sociologist Daniel Bell's seminal 1965 essay "The Post-industrial Society." For Bell, the crucial change is the "new centrality of theoretical knowledge." This means rational social structure and culture in which research and scientific knowledge and expertise play the dominant role. With intellectual institutions

gaining ascendancy in society. Ball sees "the rise of a new class who, on the political level, serve as advisors, experts, or technocrats." He would not, however, predict the outcome of the anticipated struggle for power between this new group and the conventional "brokerage" politicians, for any crucial decisions will remain political and this is the "problematic question that confronts us."

Writing as a social scientist, Ball does not offer personal aspirations but lengthy treatment of a "system of social accounts" as a most significant development for the post-industrial society betrays his leanings. This is a system in which: one, alternative societal goals are elaborated through means provided by modern technology; two, choices are made democratically by the population at large; and, three, these are implemented by utilizing the post-industrial society's intellectual tools. Social accounting would include measurements of socio-economic "ills," costs and benefits of different policies. Such a system would be both more technocratic and more democratic, providing American society with means "to appraise its achievements, its needs, and its shortcomings by being able to specify broad national goals and national priorities."⁵⁹

Another sociologist, Amitai Etzioni, has provided comprehensive theoretical analysis of a possible "Active Society."⁶⁰ Like Ball, he singles out as the linchpin a new role for institutions of knowledge. Etzioni's "Active Society" is able to end social drift, to articulate and move toward societal goals, a possibility created by scientific knowledge and the technically knowledgeable. Thus The Active Society is at once both an extremely detailed critical analysis of current U.S. structures and functioning, and an imaginative description of the

utopian society toward which he feels men of knowledge should strive through use of science and technology.

Emmanuel Mesthene, Director of the Harvard Program on Technology and Society, (funded, and recently closed down, by IBM) is another Technophillic Activist. He is concerned about reconciling the required technocratic expertise in government and private institutions, with maintenance of democratic consultation and mass participation. We label him an activist because his writing appears to lean primarily toward reconciling the latter to the imperatives of the former. Technocracy in one form or another is here to stay and we should be concerned about shaping institutions to adapt and make good use of it. At present, we are failing:

... technology lies fallow because existing social structures are inadequate to exploit the opportunities it offers....

Pollution of the environment, potential damage to the ecology of the planet, occupational and social dislocations, threats to the privacy and political significance of the individual, social and psychological malaise--are negative externalities.... They are with us in large measure because it has not been anybody's explicit business to foresee and anticipate them.⁶¹

Institutional change is generally needed, but:

Above all, perhaps, higher education especially will need to attend more deliberately and systematically than it has in recent decades to developing the reflective, synthetic, speculative, and even the contemplative capacities of men, for understanding may be at a relatively greater premium henceforth than particular knowledge. When we can no longer lean on the world's stabilities, we must be able to rely on new abilities to cope with change and be comfortable with it.⁶²

Among the most clearly activist among technophiles is Melvyn Brantsberg, editor of the magazine Technology and Culture. He has little patience with those who see a contradiction between humanist

values and technology. What is human about man is his use of tools, that is, technology. Through industrialization, efficiency, and the creation of leisure, technology has released and enhanced man's capacity to be "human." Humanists, rather than criticizing technological society, should be thinking of ways to take more humane advantage of its possibilities. This would require close working relationships with the scientists who are creating new possibilities and the technologists implementing them. Technology is neither bad nor good, but if man can do good then technology can be a powerful means to that end. We must continually refine our methods of technology assessment, utilizing the abilities of intellectuals of varied backgrounds.⁶³

This group, while still very influential, has experienced some defections from its ranks and is less numerous in the 1970's than in the earlier post-war period.

II) Technophillic Oppositionism

Oppositionists of a pro-technology persuasion might, like Baconian Oppositionists, be labelled "established dissenters." While sharing many assumptions and aspirations with technocratic activists, they are highly dissatisfied by what they see. Their dissent is not radical. They accept the prevailing social order generally and, however unwelcome, their criticism is often accepted in establishment circles. While there are many such writers, we will examine only a few.

John Kenneth Galbraith's The New Industrial State is a key oppositionist statement. Galbraith sees what he calls the "industrial system" as now able, through use of planned technology, to determine social goals and cultural content for society--forcing out non-technical

considerations. It is "the imperatives of technology and organization which determine the shape of economic society.... We are becoming the slaves in thought as in action of the machine we have created to serve us."⁶⁴ Galbraith sees a "technostructure" so pervasive that "countervailing" mechanisms against concentrations of power, especially the marketplace (which he had relied upon in earlier works), have themselves been incorporated into the technostructure. Marketplace rationality has largely been replaced by corporate technological decision making leading to producer sovereignty; and we cannot count on either labour unions or the state to re-create economic rationality.

However, neither uprooting the technostructure nor rejection of the present social system is needed. Instead Galbraith wants a realignment of elites; a misguided technostructure must be made rational by the actions of an informed intellectual elite. "The future of what is called modern society depends on how willingly and effectively the intellectual community in general, and the educational and scientific estate in particular, assumes responsibilities for political action and leadership."⁶⁵ The book concludes with subdued optimism. "The industrial system ... is intellectually demanding. It brings into existence, to serve its intellectual and scientific needs, the community that, hopefully will reject its monopoly of social purpose."⁶⁶ More recently, this economist's optimism has become even more subdued.⁶⁷

Alvin Toffler, whose Future Shock was a long time best seller, clearly fits here. More than anyone else, perhaps, he has popularized the issue of technology and society, and has generated public interest, awareness and concern. Generally, Toffler shares the technophillic perspective that there are various powerful societal effects emanating from science and technology in the U.S. and we must make the best of

this situation. He is in the dissenters' camp because he implies that unless popular consciousness alters considerably things will get worse.

"Future Shock" is the state of distress experienced by contemporary man whose adaptive systems simply cannot keep pace with the "accelerative thrust" of technology and the state of "overchoice" it creates. Toffler outlines various types of changes, in communications, transportation, consumption, the rate of knowledge, etc., many of which are disturbing. However, he has little sympathy for the "strange coalition of right-wingers and New Leftists" who would "plunge backward into irrationality, anti-scientific attitudes, a kind of sick nostalgia and an exaltation of newness."⁶⁸ He calls for a new "social futurism" entailing "humanised planning." To allow for "anticipatory democracy," Toffler wants ad hoc "social future assemblies" which represent geographical areas and social units, to make broad decisions.⁶⁹ A society with technology moderated and controlled will provide "future shock absorbers" to alleviate man's distress and help him adapt to possibilities provided by the new era. The most vital concern is, as all voluntarist technophiles agree, to harness technology for consciously determined beneficial social ends.

A well known exponent of the Guaranteed Annual Income (GAI), Robert Theobald, has similar views. Writing on the "social impact of cybernation," Theobald notes important results of widespread computer use: "unlimited destructive power, unlimited productive power, the elimination of the human mind from repetitive tasks, and the organizing drive of the computer within a cybernated system." Unfortunately, he adds: "We have not yet been willing to recognize that we live today in a truly lazy society--a society where we allow technological trends

to make our decisions for us because we have no mechanisms to allow us to control them." Man must creatively accept responsibility for his own survival. We must develop new computer-aided policies for a new socio-economic balance to meet fundamental societal needs.⁷⁰

Theobald urges guaranteed annual income and the consequent cultural change with respect to work and leisure as a necessary first step.

In The Next Generation, Donald Michael describes the world to come, echoing many other technophiles' acceptance of a technological imperative at the root of these changes. In The Unprepared Society, Michael sounds oppositionist warnings, depicting structural and cultural failure to meet these changes and utilize them for human purposes. While society now is dangerously deficient, through education we must and can overcome our "institutional, methodological, and personal unpreparedness to make the kind of long-term technological planning so desperately needed."⁷¹

Michael considers himself a "futurist," and so does British scientist Denis Gabor who, in a phrase well-known in the U.S., speaks of the need to "invent the future." This phrase perhaps best describes the social task urged by voluntarist technophiles. As Gabor puts it: "We cannot stop inventing because we are riding a tiger, we must now start thinking of social invention to anaesthetize the tiger, so that we can get off its back."⁷² The futurists' "dean" today is probably French philosopher Bertrand de Jouvenal whose The Art of Conjecture⁷³ has become a basic textbook in futurist logic, and whose profession he calls the study of possible futures. This self-designation combined with his critical stance and concern over the lack of serious future planning justifies placing him among the Technophillic Oppositionists.

A more clear cut oppositionist is Michael Harrington. In The Accidental Century, he portrays modern society as having lost control of its development to the unrestrained forces of technology. The answer is not another plea to intellectuals to take responsibility. He advocates a democratic socialism in tune with the new era:

I am suggesting that the only way the accidental revolution can become socially conscious of itself is through a profound economic and social deepening of democracy.... If the accidental revolution is to be made conscious and democratic, that will happen in the course of an involved, complex process. It is no longer a question of 'seizing' power.... It is rather a problem of transforming power, of changing it, of making new institutions.⁷⁴

The answer is thus political, and Harrington looks to the model provided by West European social democrats' concepts of democratic planning. As with Theobald's concentration on the GAI, Harrington selects specific proposals. Publicly supported adult education and democratically designed public-housing are his key areas and he concludes:

Perhaps it seems strange in the final pages of a book which has raised issues of God and man to be talking about the way in which public housing is built, but that is precisely the problem: to infuse the minute and calibrated technology, and the growing abundance of statistical and planning data, with the Western spirit, to marry engineering and philosophy.⁷⁵

This is Harrington's response to a problem he poses dramatically:

"Either Western man will have to choose a new society or a new society will choose, and abolish, him."⁷⁶ Through this combination of perceiving a global crisis yet offering "reformist" solutions, Harrington clearly exemplifies a defining characteristic of Oppositionists. His socialist language is foreign to the other American observers but its content is quite similar.

III) Technophillic Determinism

This is a group of thinkers who have acquired some following and

thereby merit consideration. They might be conceived as contemporary heirs to the social vision of Condorcet, Bellamy, and the other early prophets of a technological utopia. For them the advent of this utopia is not the product of man's conscious choice but the result of powerful historical forces.

Theologian and biologist Teilhard de Chardin provides perhaps the most cosmological evolutionary perspective of the coming era. Evolution is seen as an organic rise in Christian consciousness and Teilhard foresees scientific consciousness and political organization advancing to a point of coalescence. Evolution will enter "a new phase ... thus extending on a planetary scale the fundamental vital processes which cause matter, under certain conditions, to organize itself in elements which are ever more complex physically, and psychologically ever more contraded. Thus ... we see woven around us ... the network and consciousness of a noosphere."⁷⁷ The result will be a fully developed human community, directed by science and the organizational tools science has provided toward a community of love.

American theologian Harvey Cox has analogous, though far less extreme, views. For Cox technological development has resulted in the creation of "the secular city" which (unlike most theologians) he welcomes, as it has freed man from the constraints of nature--opening the possibility of greater individual moral freedom: "Technology ... should be a means to man's fulfillment." Cox sees man's central concern as not to control technology but to make use of the freedom it offers, leaving technology to remain an autonomous force.⁷⁸

Marshall McLuhan is a secular Technocratic Determinist. Technology replaces God, and he is one of its prophets: "All social

changes are the effect of new technologies ... on the order of our sensory lives."⁷⁹ The computer and related technologies create a totally new environment, and, as for Teilhard, this new environment is the Nirvana of the future. "Our technologies ... now become the matrix of macroorganic connubial bliss."⁸⁰ McLuhan argues that ultimately rationality, or understanding as Western man knows it, is irrelevant to the new sensate culture science and technology are creating. The new medium must be understood in terms of itself; the content of the new electronically transmitted culture is in its transmission and must be experienced from within. This conviction makes McLuhan not only pro-technocratic but clearly determinist.

Buckminster Fuller shares much of McLuhan's vision. While McLuhan sees the electronic "revolution" creating a global village, Fuller uses the term "one-town world" to describe the future. As to its direction, Fuller coined the term "Space-Ship Earth" to highlight the notion of man at the helm of a massive machine manoeuvring and evolving through history and the natural environment. But this apparent voluntarism is on the surface of a basic determinism ensured by man's nature:

Man ... is a self balancing, 28-jointed adapter-base biped, an electro-chemical reduction plant, integral with the segregated storages of special energy extracts in storage batteries, for subsequent actuation of thousands of hydraulic and pneumatic pumps, with motors attached; 62,000 miles of capillaries, millions of warning-signal, rail road, and conveyor systems; crushers and cranes ... needing no service for 70 years if well managed; the whole, extraordinary complex mechanism guided with exquisite precision from a turret in which are located telescopic and microscopic self-registering and recording range finders, a spectroscope, etc.⁸¹

For Fuller, stress on man's spiritual, non-mechanical qualities represents an outdated mentality once useful, now of little meaning.

soon to be discarded--as is politics:

Suppose, asks Fuller, a comprehensive anticipatory design science were implemented on a global scale. The waves of that revolution would submerge politics and ideologies forever.... Although politicians and their ideologies were needed to catalyze its early growth, industry reaches a threshold (and on a world scale has already reached it) where these forces become redundant--huge megaliths obstructing the throughways of the future.⁸²

Simon Ramo serves both as spokesman and inspiration for many Technophillic Activists, but his own more extreme position qualifies him for a Determinist label. His following consists largely of engineers and technocrats who both sing the praises of contemporary technology and, at the same time, live off it.⁸³ In 1953, Ramo and Dean Wooldridge, both Cal Tech scientists, were members of the U.S. Air Force's commission on missile development. Ramo and Wooldridge also owned a small research company which was contracted by the commission for research and development work. In five years of operation Ramo-Wooldridge had become the first of the great aerospace conglomerates--TRW. TRW's high profits and extremely close connection to the Air Force brought adverse publicity and congressional questioning but it continued to grow nonetheless, with annual sales topping \$500 million in 1963. By the mid 1960's the Air Force-TRW special relationship was terminated and other aerospace giants had appeared, but the example of TRW remains as perhaps the best (or worst) example of symbiosis between high technology, high profit and the American state.⁸⁴

The benefits of science and technology, Ramo argues, can be reaped by judicious use of "systems engineering." With the aid of this new method to combine skills of engineers, natural and social scientists, the U.S. can create a "synthetic superbrain" to "break

the bottlenecks in solving many problems of our society where technology is a factor."⁸⁵ While Ramo's claims are not terribly ambitious, they are based on a rather deterministic confidence in the new technology: "Once most people are wedded to creative logic and objectivity to get solutions to society's problems, the world is going to be a lot better. Then maybe we can say ... that science and technology are then being used to the fullest on behalf of mankind."⁸⁶ How quickly this progress is to spread depends largely on systems engineering teams. The systems-engineer:

... must include in 'his' head the total intelligence, background, experience, wisdom, and creative ability in all aspects of the problem of applying science, and particularly, who must integrate his total intelligence....

In this sense, then, a good systems-engineering team ... combines individuals who have specialized variously in mathematics, physics, chemistry, other branches of physical science, psychology, sociology, business, finance, government, and so on.⁸⁷

While Fuller ignores the non-technological in man's nature, Ramo ignores the non-scientific in man's external progress. Both seemingly find no place for the poet and philosopher, the artist and visionary among the elite destined to create the future. Krantsberg, otherwise very close to Ramo, does envision a potential role for "humanists."

On a totally different philosophical plane the work of B.F. Skinner and some of his followers in behavioral psychology can be compared to the position expressed by Ramo. Skinner is an eminent psychologist and philosopher of science who, in contrast to Ramo, is divorced from the hardware of technology. Nevertheless his general ideas lead further in the direction of technological determinism than Ramo's. While Ramo emphasizes the mechanical technology of modern society, Skinner concentrates on the implications of the behavioral technology developed by modern psychological research, telling us: "A technology

of behavior is available which would successfully reduce the aversive consequences of behavior, proximate or deferred, and maximize the achievements of which the human organism is capable."⁸⁸ Further: "What is required is more intentional control, not less, and this is an important engineering problem."⁸⁹ To achieve this means to adopt a new world view, one "demanded" by science:

Science has probably never demanded a more sweeping change in a traditional way of thinking.... In the traditional picture a person perceives the world around him, selects features to be perceived, discriminates among them, judges them good or bad, changes them to make them better ... and may be held responsible for his action.... In the scientific picture a person is a member of a species shaped by evolutionary contingencies of survival, displaying behavioral processes which bring him under the control of the environment in which he lives, and largely under the control of a social environment which he and millions of others like him have constructed and maintained during the evolution of a culture. The direction of the controlling relation is reversed: a person does not act upon the world, the world acts upon him.⁹⁰

IV) Technological Alternativism

This category covers a wide range of opinion and several levels of analysis. It is marked by almost as much action as discussion. Its members are united by fairly simple and basic notions: first, technology is the driving force of contemporary society; second, the presently dominant technology is carrying us toward catastrophe-- particularly to ecological disasters; and third, survival will require revolutionary new values, technologies and organizational structures to replace present "establishment" scientific attitudes, activities and hierarchical forms.

The best source for these ideas is to be found in the rapidly expanding literature on ecological requirements for survival of modern society. Scientists, architects, agronomists, engineers,

social scientists, etc., are publishing books and articles offering new and "far out" ideas for ecologically sound ways of living in tune with nature. Periodicals such as Mother Earth News, The Whole Earth Catalogue, and Modern Utopias, and books such as Earth Tool Kit, The Environmental Handbook, How to be a Survivor, and Ark II.⁹¹ among many others abound with proposals, suggestions, and demands on these topics. This work consists of more than a fad. The authors are almost invariably driven by a strong sense of impending technologically based catastrophe. They attempt to provide bases for genuine technological alternatives. Here are two excerpts from introductions to books listed above.

The hope that is contained in all this despair is that an understanding of the need for new man and new life styles is spreading rapidly. More and more people are realizing that the mere treatment of the symptoms of our disease will no longer suffice; fundamental changes will be required if we are to have a chance.⁹²

To many, it will appear that social actions which challenge growth, power and progress are negative, but these efforts must also be seen as an affirmation of life. If the proper groundwork is done by environmental activists, public support for controlling technology ... will develop. It will in part come from the proselytizing of environmentalists, but the largest segment will be generated by individuals rejecting a death-oriented society. The task, then, is not to create a constituency for a life-oriented movement; it is only to give it direction.⁹³

In sum, this position combines the extreme conservationist notions of the organic gardeners and waste recyclers, and the call of various contemporary utopians for a rejection and replacement of contemporary technology, with a very deep concern for the future of civilization. (One might also include in this category some of the more radical futurist ideas which have been emerging.)⁹⁴

V) Technophobic Activism

This position, which combines an anti-technocratic outlook with

a fair amount of confidence that science and technology are being kept "in place" is rapidly losing popularity, and with good reason. There is plausible cause to believe, we have suggested, that the evidence does not lend itself to easy confidence today on this score. Two important thinkers do appear to take such a position.

Michel Crozier is a highly respected industrial sociologist whose approach is less theoretical than empirical. In his famous study of two French industrial settings, The Bureaucratic Phenomenon, Crozier treats the impact of technocrats on industrial enterprises and concludes: "When progress accelerates, the power of the expert is diminished and managerial power becomes more and more a political and judicial power rather than a technical one. Managers' success depends on their human qualities as leaders and not on their scientific know-how."⁹⁵ Crozier suggests technocratic power is intrinsically self-limiting; therefore, fears of technocracy may be stilled.

Technocracy is still a constant cause of fear.... Many intelligent observers feel that because of the complexities of our technical age (technocrats) are coming to hold more and more power in society as a whole, and that these technocrats will form the new ruling class....

Such a belief is not validated by a careful analysis of modern decision-making. It derives from a misunderstanding of the situation created by technical and scientific progress. The invasion of all domains by rationality, of course, gives power to the expert who is an agent of this progress. But the expert's success is constantly self-defeating.... As soon as a field is well covered, as soon as the first intuitions and innovations can be translated into rules and programs, the expert's power disappears.⁹⁶

Victor Ferkiss' work is of a different order. In Technological Man, this political scientist considers the impact of science and technology on society at some length, reviewing a few of the major

works on the subject. His conclusions are somewhat unusual in that they consist of reflective questioning:

... there is no guarantee that any of the fundamental realities of the social, economic, or cultural systems will change. The new powers in man's possession may be in the hands of those motivated by greed, fear, superstition or the lust for power in the old sense. A heartless, cold, and rational technological man is sometimes portrayed as the bogeyman of the future. Many hope that he may be induced to take on more the humanistic virtues and use his physical powers in accordance with traditional moral codes. But what if he emerges as the thrall of the worse elements of the past? What if technology rather than becoming 'autonomous' remains much as it is today, the tool of the salesman and the militarist? What if bourgeois civilization and its socialist counterparts remain dominant, and technology becomes merely another weapon in the arsenal of the drive for profit and power?⁹⁷

In other words, the question can only be resolved through time. Perhaps the major effect of accelerated science and technology is to highlight more starkly critical but traditional issues. Ferkiss is not complacent. He sees emerging problems, and offers solutions which lie in the adoption of a new outlook on life. Science and technology are for him neither saviour nor Satan; both of these lie not within technology but within man himself. On the whole, Ferkiss' contribution to Technophobic Activism is to affirm man's dual nature, and to encourage skepticism--casting a critical eye upon the entire discussion.

VI) Technophobic Oppositionism

Technophobic Oppositionists would also like to keep science and technology "in place," but are disturbed at seeing a failure to do so. They are opposed to technocratic domination in principle and fear it in practice. They are voluntarists in that they believe action in the face of impending technocratic domination can be successful. Such a position has gained support in the last few years, especially among younger educated people.

Psychologist Erich Fromm is an older Oppositionist. In The Revolution of Hope, Fromm applies the general framework developed in earlier works to a critique of technological society. In such a society, he says, quality is replaced by quantity and value by efficiency, creating a technological imperative: what is technologically feasible becomes socially necessary--"is" automatically becomes "ought." Therefore he concludes, contrary to Krantzberg, that humanist imperatives are rigidly excluded. For Fromm this is a deplorable situation for it turns man into a robot--programmed to consume for the sake of consumption--and devalues genuine human emotion. His answer is socialist humanism, and as such might seem reminiscent of Harrington. Fromm's revolutionary state, however, is not social democracy devoted to rational public planning, but rather a highly decentralized socialism with neighbourhood groups providing alternatives to a dehumanized system and asserting primacy of the human person and primary group over technological rationality.⁹⁸

Anarchist Lewis Herber (Murray Bookchin) has much in common with Fromm's analysis and vision. In a series of well-known essays on ecology and anarchism in contemporary America, Bookchin advances the thesis that ecological appreciation leads naturally to encouragement of alternative technologies. Ecological principles require a life style in harmony with natural surroundings. He argues that today any social system striving for a harmonious and stable ecosystem could only be structured along highly decentralized, democratic lines. He predicts that today's technology, once freed from socioeconomic class control and callous exploitation, would serve to make manifest a decentralized society serving human needs while enhancing the natural environment. His rejection of the contemporary social order is total

and his strategic program is revolutionary, but his view of technology is not deterministic. Bookchin calls for creation of a revolutionary socialist movement with conservationist and humanistic goals:

Having acquired a vitalizing respect for the natural environment and its resources, the free decentralized community will give a new interpretation to the word 'need'. Marx's 'realm of necessity,' instead of expanding indefinitely, will tend to contract; needs will be humanized and scaled by a higher valuation of life and creativity. Quality and artistry will supplant the current emphasis on quantity and standardization; durability will replace the current emphasis on expendability; an economy of cherished things, sanctified by a sense of tradition and by a sense of wonder for the personality and artistry of dead generations, will replace the mindless seasonal restyling of commodities.... Man will recover a sense of the things that are for man, as against the things that have been imposed upon man.⁹⁹

Paul Goodman, a radical of uncertain ideological stance, is a steadfast Technophobic Oppositionist, though his language is restrained, and his ideal seemingly modest:

To meet the historical crisis of science at present, for science and technology to become prudent, ecological, and decentralized requires a ... kind of religious transformation. Yet there is nothing untraditional in what I have proposed: prudence, ecology, and decentralization are indeed the high tradition of science and technology. Thus the closest analogy I can think of is the Protestant Reformation, a change of moral allegiance, liberation from the Whore of Babylon, return to the pure faith.¹⁰⁰

Goodman responds positively to the major question of whether technology can be humane. The simple requirement, perhaps already being fulfilled, is a moral restoration vis-a-vis science and technology to check their excesses and make their character "prudent, ecological, and decentralized."

Lewis Mumford, the most prolific writer on this subject, has a breadth of scholarship which defies categorization. His critiques of "technics," span historical epochs and scholarly disciplines, and put him at the forefront of the most cosmic critics. However, he is not

determinist. The trends he deplores are ascribed to mistaken attitudes and poorly chosen values. These, he appears to believe, can ultimately be replaced. Humanistic "technics" once were dominant and may come to be so again. No excerpt would begin to do justice to the comprehensiveness of Mumford's description and analysis of the origins and development of modern science and technology, and the thinking implicitly conjoining "the machine" with political power and social control.¹⁰¹ For an alternative to the present technics-dominated society, Mumford has no blueprint to impose upon the "forces of life" he sees gathering. The decentralized "basic communism" he advocates¹⁰² has much in common with the more elaborate program outlined by Bookchin, though Mumford's pacifist convictions would certainly lead him to disfavor revolutionary aspects of Bookchin's strategy.

Another Technophobic Oppositionist is the noted French student of the 5th Republic, Jean Meynaud, who has conducted an exhaustive study of the role of "technicians" (technically trained bureaucrats) in contemporary France. He rejects Crozier's confidence in the declining importance of technocracy. Crozier, he says, ignores the esoteric content of technical knowledge, thus exaggerating its accessibility to those without formal scientific training.¹⁰³ This does not lead to extreme pessimism, but Meynaud does urge caution:

While admitting that technicians often constitute a valuable and irreplaceable counterweight to particular interests, I believe that it is desirable that their actions be placed under the supervision of a higher authority.... It seems to me that there is no other way to ensure its implementation than by putting technicians under the control of elected authorities.¹⁰⁴

Development of technocracy is possible, perhaps imminent, but is by no means inevitable:

... we might wonder whether the determination to obtain maximal efficiency, combined with greater prosperity in larger sectors of society, is not likely to throw into gear a movement which will lead to progressive monopoly of power by 'competence'....

Many aspects of contemporary social life ... forbid us to neglect these suppositions. The writing on the wall is already too clear. I doubt whether the observation of social facts ... can avoid giving rise initially to a pessimistic reaction. But such an attitude is a healthy one if it brings about the desire to join battle against those factors of enslavement and degradation which threaten our existence.¹⁰⁵

American political scientist Hans Morgenthau has expressed comparable, though somewhat more pessimistic views. Morgenthau too fears for democracy, viewing with alarm the arrival of a politics of expertise:

Scientific arguments have become indispensable weapons in the struggle for power within the executive branch, and the scientific elites have become the providers of the new weapons. Starting out as the disinterested surveyors of esoteric knowledge, the scientific elites end up by rationalizing and justifying the political interests by dint of their possession of esoteric knowledge.... This combination of esoteric knowledge and political power alters the function and character of scientific knowledge.... In the eyes of both the political authorities and the public at large, the scientific elite appears as the guardian of the Arcana Imperii, secret remedies for public ills.¹⁰⁶

Morgenthau offers no specific solutions, and says widespread belief in "solutions" is a manifestation of the hybris which an age of "scientism" has fostered. Man is free to oppose this, to struggle for a return toward more traditional forms of political power and decision-making.

VII) Technophobic Primitivism

In this category we place the ideas of a few individuals whose fear of technocracy is combined with a vision of an emerging anti-scientific culture and society. Two writers in particular have

received widespread attention in recent years.

While science and technology per se are not his primary concern, Charles Reich's descriptive critique of the American "corporate state" and the "level of consciousness" arising to transcend that state reveal insight into effects of science and technology upon prevailing culture. Reich has a controversial and novel vision of inevitable nonviolent revolutionary change:

There is a revolution coming. It will not be like revolutions of the past. It will originate with the individual and with culture, and it will change the political structure only as its final act. It will not require violence to succeed, and it cannot be successfully resisted by violence.... It promises a higher reason, a more human community, and a new and liberated individual.¹⁰⁷

The new consciousness ... tells us how to make technology and science work for, and not against, the interests of man. The new way of life proposes a concept of work in which quality, dedication, and excellence are preserved, but work is nonalienated, is the free choice of each person, is integrated into a full and satisfying life, and expresses and affirms each individual being.¹⁰⁸

Reich has been accused, to some extent justifiably, of extreme naivete and over-optimism.

Theodore Roszak's views, similar to Reich's, have a less rosy perspective. Roszak is perhaps more important to us because his analysis of U.S. society is centered on his understanding of the role of science and technology. The subtitle to his book The Making of a Counter-Culture is "Technocracy and its Youthful Opposition." "Technocracy" is a social form whose complexity places the individual in such a state of bewilderment that he leaves decisions based on unknown scientific criteria to technical experts. The mass is also seen to be actively manipulated into conformity.

While not ignoring structural consequences of today's technology,

Roszak is more concerned about its psychological effect. In Where the Wasteland Ends, he attempts to develop philosophical bases for a new psychology: "But if our psychology is not itself to be debased by scientific objectification, then it must follow where liberated consciousness leads it; into the province of the dream, the myth, the visionary rapture, the sacramental sense of reality, the transcendent symbol."¹⁰⁹ Roszak is not sanguine about an early dominance of social consciousness, though he too looks to the youth culture as its prime agent.

The religious renewal we see happening about us--especially among disaffiliated young people, but by no means only among them--seems to me neither trivial nor irresponsible, neither uncivil nor indecent. On the contrary, I accept it as a profoundly serious sign of the times, a necessary phase of our cultural evolution, and--potentially--a life-enhancing influence of incalculable value....¹¹⁰

VIII) Technophobic Determinism

These thinkers raise most difficult questions and fearful possibilities. Among them are writers of both left and right, but they share a portrayal of technocratic society as both inevitable and intolerable. They are contemporary descendants of a strain of anti-modernism which dates back to the earliest years of the industrial revolution.

One thread of this view is a romantic yearning for the wholesomeness of some earlier civilization. Early twentieth century poets such as Yeats and T.S. Eliot expressed such feelings, as did anti-utopians such as Aldous Huxley, Karel Capek and E. Zamyatin. Religious philosophers such as Tillich, Mounier and Heidegger have also voiced similar forebodings about the direction of modern civilization.¹¹¹ Critics of massified industrial society, Ortega Y. Gasset, for example,¹¹² can also be included. Nevertheless, with the developing

popular faith in science and technology early in this century, these voices were largely ignored. After World War II a few critiques of scientism began to appear and the analysis gradually deepened. Examples are Frederick Junger's The Failure of Technology (1949), Roderick Seidenberg's Post-Historic Man (1950), William F. Whyte's The Organization Man (1956), and The Human Condition by Hannah Arendt (1958).¹¹³ In a designedly prophetic work, Seidenberg proclaimed that "the triumph of science ... hastened the dissolution of human and cultural values in favor of the more consciously and purposively determined ends and aims of our predominantly technological civilization."¹¹⁴ The result was inevitable:

As science and technology advanced, man alone appeared intractable in a tractable universe. Such a situation becomes in time intolerable as the world is subject to increasing change; and the intelligent operation of human affairs thus required man to look upon himself objectively as part and parcel of his own system; he too, had to become amenable to engineering calculation like his steel beams, his dynamos, and his turbines, to the end that men and machines might be correlated with the same nicety and precision achieved in their design.¹¹⁵

For Arendt, the value to be preserved is the functioning of the political arena. A reasonable amount of "political space" must be kept open for the common man to fulfill himself through participation in the making of decisions which determine his future. She is extremely pessimistic about this in light of emerging tendencies, particularly the rise to power of technical expertise. Out of this will come a technologically based condition distinguished by "the deadliest, most sterile passivity the world has ever known,"¹¹⁶ with a resulting huge loss in human experience:

If it should turn out to be true that knowledge

(in the modern sense of know-how) and thought have parted company for good, then we would indeed become the helpless slaves, not so much of our machines as of our know-how, thoughtless creatures at the mercy of every gadget which is technically possible, no matter how murderous it is.¹¹⁷

The 1964 English translation of Jacques Ellul's 1954 study The Technological Society captured the imagination of many observers and in a sense distilled the insights and fears of earlier writers by carrying determinist arguments to logical extremes. While Ellul's book offers a chilling and detailed analysis of technology's effects, for instance in communications and propaganda, and in industrial organization, its most important theoretical contribution is postulation of a "convergence" effect of different techniques on man:

Our interest here is the convergence on man of a plurality, not of techniques, but of systems or complexes of techniques. The result is an operational totalitarianism; no longer is any part of man free and independent of these techniques. This convergence might be likened to the convergence of theater projectors, each of which can fulfill its individual function only in conjunction with the others. The effect cannot be predicted on the basis of the individual projectors, only on the basis of the object illuminated. Such is the case with human techniques. A plurality of them converge toward the human being, and each individual technician can assert in good faith that his technique leaves intact the integrity of its object. But the technician's opinion is of no importance, for the problem concerns not his technique, but the convergence of all techniques.¹¹⁸

Finally there is the work of Herbert Marcuse.¹¹⁹ It is remarkable how this revolutionary Marxist's views dovetail with those of the Catholic conservative Ellul. The general thesis is bleak:

One of the most vexing aspects of advanced industrial civilization [is] the rational character of its irrationality.... The people recognize themselves in their commodities; they find their soul in their automobile, hi-fi set, split-level home, kitchen equipment.... The prevailing forms of social control

are technological ... all contradiction seems irrational and all counteraction impossible....

When the individuals identify themselves with the existence which is imposed upon them [this constitutes] a more progressive stage of alienation.... The subject which is alienated is swallowed up by its alienated existence. There is only one dimension, and it is everywhere and in all forms.¹²⁰

Science, usually hero in Marxist lexicon, becomes villain:

Today the question must be asked whether, in the 'affluent societies,' science has not ceased to be a vehicle of liberation, whether it does not ... perpetuate and intensify the struggle for existence instead of alleviating it. The traditional distinction between science and technology becomes questionable. When the most abstract achievements of mathematics and theoretical physics satisfy so adequately the needs of IBM and of the Atomic Energy Commission, it is time to ask whether such applicability is not inherent in the concepts of science itself. I suggest that the question cannot be pushed aside by separating pure science from its applications and putting the blame on the latter only: the specific 'purity' of science facilitated the union of construction and destruction, humanity and inhumanity in the progressive mastery of nature. In any case it is impossible ... to distinguish, within the whole of scientific research, between life-preserving and life-impairing fields, methods, and concepts--they seem to be internally linked.¹²¹

If science cannot provide the necessary critical mode by which man can examine reality, "from the outside" and potentially transform it, where can this be found? Probably nowhere (or just possibly in the "counter-culture" as Marcuse speculates rather uncharacteristically in his Essay on Liberation).¹²² In One Dimensional Man Marcuse warned at the outset that he would "vacillate throughout between two contradictory hypotheses: (1) that advanced industrial society is capable of containing qualitative change for the foreseeable future; (2) that forces and tendencies exist which may break this containment and explode the society.... The first tendency is dominant."¹²³

Such then, in brief compass, are the positions ascertainable from

the postulated divisions with respect to the impact of science upon society. Unlike Part One, no position was dismissed as it was being described. This is because the debate is more heated and public--and therefore any such conclusions must be more contentious. Simplifying the categorization is less easy than was the case with the Cartesian-Baconian distinction, for the technophile-technophobe distinction carries more emotional involvement. With these pitfalls noted, we may proceed.

The Technophillic Activists (I) and the Technophillic Oppositionists (II) differ mainly in emphases, but these are based upon real differences in perceptions of the extent to which a desirable technocracy is being achieved. These two groups are able to argue concerning their differences, and such discourse is leading to some defection from activism into oppositionism. We cannot rationally exclude either.

For the Technophillic Determinists (III), such discourse appears not to be possible. Thinkers such as McLuhan and de Chardin see voluntarism as an illusory ripple on the surface of a basic determinism rooted either in historical evolution or in man's nature. Whatever empirically demonstrable degree of human free will there may be, then, is viewed as simple self-deceit. A group with which one cannot raise, let alone argue, the facts, in turn, must be considered unlikely to contribute significantly to our understanding. Despite this basic stance, Technophillic Determinists such as Rayb, Cox, Skinner and Fuller do exhort their followers to take the positive steps necessary to the fulfillment of their visions, and their followers generally can be classified as Technophillic Activists. Consequently that which is of value in their writings may be subsumed under Technophillic

Activism, while the remainder can be said to lie outside the bounds of useful discussion.

The Technological Alternativists (IV) hold a curiously anomalous position of hopeful despair—or of extremely pessimistic utopianism. Despite frequent pronouncements of impending technologically-based catastrophe, in seeking alternative technologies commensurate with ecologically sound life styles, this group is positively demonstrating that such can be found and adopted. Their actions appear to an outside observer more persuasive than their arguments and radical rhetoric—and the actions display both conservationist and futurist threads which they are weaving with increasing sophistication. The emerging fabric of this growing movement seems increasingly to be losing deterministic hues and gaining voluntaristic tones. Given increasing recognition of this direction as "respectable," one needs only slight anticipation of events to include Technological Alternativism as a wing of Technophillic Oppositionism.

The above observations suggest that among technophiles determinism is of small (and decreasing) relevance in the debate concerning the effect of science upon society.

The Technophobic Activists (V) and Oppositionists (VI) differ mainly in emphases, again based on clear differences in perceptions. However, we find Technophobic Activism deficient in that precious little evidence or persuasive argument can be found to support this position. There is little wonder that it has become an almost empty category, for it fails the crucial test of salience and so may be considered of little importance today.

Technological Primitivism (VII), has had its day—during which it gained a few more advocates than it now has lost. It may yet rise

again, but presently appears to have been naive in the extreme. It could stand firmly only as a field of its own--and then only if shorn of its naivety, as was Ghandi's "Satyagraha," or non-violence movement in South Africa before he carried it successfully back to India. Such a development is hard to conceive in contemporary America.

The Technophobic Determinists (VIII) cannot be so easily dismissed--however extreme one may consider their position. This view has been systematically developed and applied to contemporary society in ways sufficiently compelling to attract many adherents. Still, one can clearly see through examination of the literature that after the first burst of advocacy for this position in the 1960's, support has significantly declined in the U.S. One might speculate that scaling down anti-ballistic missile plans, halting development of the supersonic transport plane, loss of popular enthusiasm for space flight, an end to U.S. technological "horror stories" from Viet Nam, the energy crisis, ecology movement confrontations, and the cooling of America's love affair with the automobile--all of these together may have helped to bring this position into question. (The instrumental role played by the tape recorder in the downfall of what many saw as the U.S.'s most technocratic presidency showed the ironic capacity of technology to backfire.) Most of those still maintaining this position have moderated their views, sometimes to the point of ambivalence.

In summary, then, Technophobic (and to a far lesser degree Technophillic) Activism has given way to Oppositionism as science and technology come under increasing attack in the U.S. Technophobic Determinism presently seems to be losing ground to the facts and appears to be slowly moving toward an extreme form of Technophobic Oppositionism.

Technological Alternativism has been gradually transforming into an extreme form of Technophillic Oppositionism. Finally, Technophillic Determinism and Technological Primitivism appear to lack sufficient salience for the student of political theory relating to science and U.S. society today. We therefore left with the following matrix:

Figure 2-4

The categorical neatness displayed above is somewhat deceptive. The focal division between the technophobes and the technophiles is clearly basic. Both oppositionist groups wish to keep science and technology "in place," but hold different perceptions as to where that "place" now stands and different assessments concerning the extent and location of the desired "place." Unfortunately these

differences have not been articulated with sufficient precision in commonly acceptable language to stimulate much useful debate. This fact is not noted as a simple criticism; it reflects one of the thorniest, most intractable problems relating to the whole complex area of human thought under consideration.

With the partial exception of the waning group of Technophobic Determinists, we now see the initially posited voluntarist/determinist polarity is not nearly as salient as might have been expected. Perhaps this is due partly to the overlap between Technophobic Oppositionists and Technophobic Determinists in shared visions of a "humane" society and in common fears (different in degree) of technological domination which could negate their visions. Such fears constitute a partial, but major, convergence of concern which seems largely responsible for depleting the ranks of Technophobic Activism.

These groups differ as to what, if anything, can be done, but this is a question about which the writers themselves generally admit to some ambivalence. The Technophobic Determinist Marcuse, is, as we have seen, one of the most forth-rightly open in admitting this, but even Ellul finds a small smattering of hope in the end. The Technophobic Determinists would necessarily stand in an isolated class by themselves if it were not for the fact that they are largely undogmatic. John Wilkinson of the Center for the Study of Democratic Institutions in California was instrumental in disseminating the Technophobic Determinist perspective in the U.S. with his translation of Ellul's Technological Society, yet that Center became probably the nation's major proponent for "constitutionalization" of science and technology to bring their effects under social control. This advocacy cuts both ways; as we have seen, it also had a place in the discussion of the

effect of society upon science.

One of the leading exponents of Technophobic Determinism and constitutionalist, W.H. Ferry, of the same Center, has well expressed the pessimism tinged with determination which characterizes this group and prevents our dismissing them as beyond the periphery of reasoned dialogue:

I conclude that technology has to be brought under public regulation and direction.... Among other things this means less centralization, not more--the participation of citizens in decisions that closely touch them. How this can be done in a system that is more and more comprehensive in its approach is only one of the significant and unresolved problems in political theory. Yet we must try; and even if the effort in the long run should fail--even if technology and its proponents showed themselves to be beyond human control--the effort would still have been worthwhile.¹²⁴

Because of these considerations, it appears reasonable to group the technophobes as constituting the radical opposition generally. This is unlike in Part One where radical was distinguished from "loyal" opposition by its determinist perspective.

Conclusion

In Part One we found the focal theoretical distinction between Cartesian and Baconian views of science in fact served very little as a means to differentiate various assessments of the prevailing situation or strategies posed to deal with it. This is a significant finding, because it helps to explain why there are so few agreed-upon common perceptual and conceptual bases from which reasonable disagreement may depart. We now have a partial explanation as to why for so long there has been a "literature in search of a field."

The fundamental argument today concerning how society affects science distills into three positions. The "supportive" Baconians or Cartesians, seek to preserve the present role and functioning of

American science. The radicals, either Baconian or Cartesian stand diametrically opposed. They are far less concerned with the ultimately correct role for science to play in society (on which they differ) than they are with their shared perception that U.S. society is controlled by a small elite standing against the long term interests of the general citizenry. The radicals agree it is not science per se which must be changed, nor do they see scientists per se able to act alone to bring about the necessary transformation. Society as a whole must be transformed, including the roles of science and scientists.

Between these two extremes the "loyal opposition," are almost invariably Baconians and look toward a socially responsible science as primary means for resolving contemporary societal crises which relate to science and technology. For many of these dissenters, scientists have a key role to play.

A similar, but logically more diffuse, generalization follows from our examination in Part Two of those writing on the impact of science and technology on society. Again, with greater qualification, we found only three assuredly significant positions. These are:

- 1) The technocrats, including Technophillic Activists and a few Technophillic Determinists; 2) The radicals, consisting of Technophobic Oppositionists and Determinists; and 3) The reformers, whose ranks comprise the Technophillic Oppositionists and what is left of the Technophobe Activists.

On both of the central issues we are left with three generalized positions, which, having been arrived at through analysis of the literature, are not trite postulations. Put into one sentence each, they are:

- 1) The relationship of science and society is proper and should

be maintained.

- 2) The relationship is improper and should be reformed.
- 3) The relationship is improper but the cause does not lie with science or technology, so the only possibility for change, if any, lies in a radical transformation of the society.

These findings allow combined treatment of the two issues we kept separate above. On a broad theoretical level such combination is inappropriate since each raises different philosophical problems. However, we have now reached a point of analysis where the basic concern is practical assessment of the prevailing situation and appropriate responses to it. Here, we can see a rough correspondence between the two groups: that is, the reformers on one question, to the extent they have any views at all on the other question, almost always take reformist positions. This is true of the defenders and radicals as well.

We have noted that arguments from one position on the effect of society upon science often reached into the discussion of the effect of science and technology on society--as is the case linking Baconian Determinism and Technophillic Determinism for example. While the link is not usually as clear (since the thinkers' range of concerns on these two different issues is widely divergent), it is nonetheless there. To illustrate this one need merely go through the mental exercise of asking: which position on the effect of science and technology on society would Baconian Oppositionists such as Commoner or Radical Oppositionists such as Oglesby be likely to take? Conversely, which position on the effect society should have on science would a Technophillic Oppositionist, such as Toffler or a

Technophillic Determinist such as Marcuse be likely to take? The answers are those one would have expected.

A problem with the activist position on each side may exemplify how rough the correspondence can be. Cartesian Activists such as Polanyi, Rabi, Brooks and Weisskopf would clearly never countenance a Technophillic Activist position. But, as noted, they could not be expected to take any position on this question since their interest is in science which they rigidly separate from technology. Wiener is an exception who does see a connection between mathematics and militarism. There are many wafflers, such as Horney and Waterman, who would have a Cartesian science based upon support bolstered by Baconian arguments. Baconian Activists, such as Platt and Weinberg, could be expected to take Technophillic Activist positions (and have); conversely, Technophillic Activists such as Brzezinski, Boulding, Bell, Etzioni, Mesthene and Krantzberg could be certain to take some form of a Baconian Activist position on the effect of society on science. With such cautionary considerations in mind, we shall henceforth subsume treatment of the effect of society on science under the more global discussion, the effect of science and technology upon society. This allows more general treatment of the relationships between science (and technology) and society.

In this chapter we have used such vague phrases as "the effect of society on science," etc., because only through such all-embracing terminology could we encompass the main aspects of the relevant discussion. Having done so, we shall try to move toward greater precision and concreteness. This is especially important in the discussion below concerning the discrepancies between what scientists actually do, on the one hand, and how they normally think of their behavior

and its consequences (and causes), on the other.

The next question to be addressed is deceptively simple: how can we resolve the differences among the three positions derived above--or can we? In one sense we cannot, since it might seem that the basic difference lies in the different interpretations of just what a "proper" relationship should be, and that this is an unverifiable "value" question. This is true to a certain extent; but only to a certain extent, for we have seen that crucial differences among thinkers very often relate to their perceptions of the prevailing relationship of science and technology to society. The "phobes" and the "philes" have highly divergent descriptions of the present situation. Obviously value differences do exist among the different observers, but the only way these value differences can be isolated and assessed is through some testing of the degree to which each position conforms to ascertainable empirical reality. Unfortunately, we have found such a method by which to verify, or even assess, empirical observations as to their relative significance to be lacking. Instead, we have generally found three different levels on which the analyses rest. Furthermore, the three different camps on this issue have each tended to develop rather different methods to verify their positions, the compatibility or incompatibility of which seems nowhere to have been systematically examined. It is almost as if differing exponents were writing on different issues.

Let us consider first the supportive position. Included among its exponents are Baconian and Cartesian Activists, Technophillic Activists and proselytistic Technophillic Determinists. They generally do not recognize their similarities. These include not only a view that the prevailing relationship of science and technology with

society is a proper one; they also tend to conceptualize "solutions" in terms of individual scientists and individual tools--or rather technological units. These may be spontaneous as Boulding's invisible college, evolutionary as Teilhard's noosphere, or organized as Ramo's systems engineering teams, but all see key roles for scientists, technologists and/or engineers. These roles are generally seen to involve application of "big science" as the essential field of activity. Weinberg might be speaking for most in this menagerie by asserting:

To me, then, the job and the purpose of science and technology remain overwhelming: to create a more liveable world, to restore man to a state of balance with his environment, to resolve the remaining elementary and primitive suffering of man--hunger, disease, poverty, and war. These are not small tasks, nor are they new ones; that in science and technology we have the possibility of dealing with them is an article of faith of all who have committed themselves to the scientific way of life.... It is up to us, members of the scientific-technological establishment, to persuade our younger impatient scientific nihilists that in the scientific-technological tradition lies our best chance of ultimate survival.¹²⁵

The supportive thinkers sometimes describe the problems facing society in grand terms but the solutions are scientific and technical. If any major social transformation is to occur it is to be gradual and evolutionary, the accumulation of separate technological "fixes" developed by new organizations or restructured old ones--gradually altering, but not directly challenging, the present relationship of science and society. Radical Oppositionist McDermott critically labels this "Laissez Innovate."¹²⁶

The reformers, consisting of moderate Baconian and Technophillic Oppositionists, and increasingly those we have called Technological Alternativists, hold a similar unrecognized commonality of position in favor of reconstituted Baconian science as necessary but far from

inevitable. They share a perception which leads them to view the conditions they castigate as lying with the crucial points of decision making in U.S. society. The type of knowledge, the integration or disintegration of that knowledge and the decisions or non-decisions which are determining the future are found to be presently lacking adequate intellectual context. This finding is key to their proposed reforms. New or alternative forms of education, brought about by intellectuals or socially responsible scientists, comprise the primary fulcrum for needed changes. Another central thread stated most explicitly by the "constitutionalists" is reform of the political structures for decision making to help ensure socially responsible uses of science--as well as to provide systematic input for scientists into socially crucial decision making.

The reformers' orientation toward positive changes goes beyond individual institutional innovations to envisage important alterations in the relationship between science and society. These would involve restructuring of the decision making apparatus and, particularly, of the decision makers' education (and the public in general as well).

For the radicals, consisting of Technophobic Oppositionists and Determinists, and Radical Oppositionists, any hope for change must occur in the basic structure of society. Science and technology are usually understood to constitute a major form the domination they oppose has taken, but a change in science itself or even in its relation to society will not as such confront the real source of this domination. Hope for such change is inherently futile. The convergence of technologies, coupled with technologically conditioned societal values, means that any attempted control of technology can

merely compound the problem. The problem is political, and scientists are unconscious allies of the status quo. Real change must be revolutionary and could occur only as a result of a thorough socio-political transformation. The solution lies, if anywhere, in a revolutionary liberation from a technological culture in its deepest sense and this is a social and political project.

Having thus associated the three major positions with three levels of analysis and perspective, we caution that these correlations are only of a general nature. Among the thinkers surveyed there are some exceptions. One that comes to mind is Brzezinski, who though classified as within the camp of supporters, appears to see changes in the offing in more revolutionary terms than the "fixes" envisaged and advocated by others in that camp. Brzezinski though nowhere spells out the practical forms his predicted changes will take and the kinds of direct effects their implementation will have. Were he to make such an effort, he might be compelled to take a general view closer to the reformists. We are suggesting that generally the advocates of the three positions provide us only with analytical support for their own positions, not with analytical refutations of the other positions.

In summary, we have tried to make some sense and order out of the millions of words which have gone into what has been called the science and society debate. On each of the two separate but related questions--the effect of society on science and that of science on society--we began by identifying what appeared to be the eight possible logically consistent positions and to illustrate these by the ideas of representative thinkers. We discovered that of the sixteen positions only nine could justifiably be termed meaningful. Going from the

more philosophical level to the more immediate one of applying these positions to the strategic question of how to understand the situation today in the United States and how to act within it, we were able to combine both the remaining five positions on the first question and the remaining four positions on the second question into three general views which we labeled supportive, reformist and radical. The three positions were understood to encompass views on both questions since the views of those termed supportive, reformist, or radical on one, were found to be either complementary or unrelated but not contradictory to those so termed on the other. Furthermore, we found that not only did the thinkers in each of these three broad categories generally share a position on the relation of science to society but they also tended to share a perspective and level of analysis.

Is progress possible toward developing a method which would potentially incorporate and relate these different levels of analysis so the various perspectives might be set off against each other with less disassociation than heretofore? Obviously no simple analytical model will satisfy all intellectual needs of each perspective since reality itself is too complex. Rather a conceptual language is needed which is sufficiently simple and clear and at the same time sufficiently wide to provide an intellectual bridge between them. Preliminary identification of some major components of such a conceptual language is our goal in the following chapters.

Notes

1. John Desmond Bernal, The Social Function of Science (New York: Macmillan, 1939).
2. Quoted in James L. Penick, Jr., The Politics of American Science: 1939 to the Present (Chicago: Rand McNally, 1965), p. 164.
3. Quoted in Michael Reagan, Science and the Federal Patron (New York: Oxford, 1969), p. 40.
4. Alvin Weinberg, "Can Technology Replace Social Engineering?", American Behavioral Scientist, Vol. 10, No. 9 (May, 1967), p. 8.
5. Alvin Weinberg, "In Defense of Science," Science, Vol. 167 (January 9, 1970), p. 143. For an excellent discussion of the historical differences between the views of Bacon and Descartes on science, see Gerald Haberer, Politics and the Community of Science (New York: Van Nostrand-Reinhold, 1969), Chapter One.
6. Quoted in Hiller Kreighbaum, Science and the Mass Media (New York: New York University Press, 1967), p. 5.
7. Alvin Weinberg, Reflections on Big Science (Cambridge: MIT Press, 1967), p. 123.
8. See John Platt, "What Must I Do," Science, Vol. 166 (November, 1969), pp. 1115-21; "Research and Development for Social Problems," Bulletin of the Atomic Scientists, Vol. 20 (June, 1964), pp. 27-29; Center Magazine, Vol. IV, No. 2 (March, 1971), pp. 63-66.
9. Platt, "Science and Survival," ibid., p. 64.
10. Ibid., p. 66.
11. John Platt, "Science and the University Crisis," Robert L. Carovillano and James W. Skehan, eds., Science and the Future of Man (Cambridge: MIT Press, 1970), p. 88.
12. Arthur B. Brownell, ed., Science and Technology in the World of the Future (New York: Wiley, 1970), p. xix.
13. Such, for instance, was the justification of Edward Teller for opposing the Nuclear Test Ban Treaty. Teller described his role as a "lobbyist for unborn ideas," and said the ban would "prohibit future science, future progress, and was directed against knowledge, our knowledge." Quoted by Cecil H. Uyehara, "Scientific Advice and the Nuclear Test Ban Treaty," Sanford Lakoff, ed., Knowledge and Power

(New York: Free Press, 1966), p. 158. Another example is found in the words of Robert Oppenheimer who had originally opposed the hydrogen bomb development; he later admitted before Congress that "when you see something that is technically sweet, you go ahead and do it, and you argue about what to do about it only after you have had your technical success." Quoted in ibid., p. 120. Others such as Dr. Hans Bethe shared this excitement over the possibility of thermonuclear explosion. See Lakoff, "The Trial of Dr. Oppenheimer," ibid., pp. 80-81.

14. Lloyd Berkner, "The Compelling Horizon," Bulletin of the Atomic Scientists, Vol. XIX, No. 5 (May, 1963), pp. 8-9.

15. New York Times (January 25, 1969), p. 18. More recently there has been some disillusionment with the failure of the space program to live up to its much heralded promise of scientific and technological spin-offs. See Jack McWethy, "U.S. Technology is Slipping--Question Is: Who is to Blame," U.S. News and World Report (August 5, 1974), pp. 28-29.

16. From an interview quoted in Robert Colborn, ed., The Way of the Scientist (New York: Simon and Schuster, 1966), p. 257.

17. Don K. Price, "The Established Dissenters," Gerald I. Holton, ed., Science and Culture (Boston: Houghton Mifflin, 1965), pp. 109-14. For a description of the founding and early days of the FAS, see Alice Kimball Smith, A Peril and a Hope (Chicago: University of Chicago Press, 1965).

18. See William R. Nelson, "Pugwash: The Scientific Conscience and International Politics," William R. Nelson, ed., The Politics of American Science (New York: Oxford, 1968), pp. 405-19. Two older groups to be mentioned under this heading are the Council for a Liveable World (CLW), and The Society for Social Responsibility in Science (SSRS). See S. Blume, Toward a Political Sociology of Science (New York: Free Press, 1974), pp. 155-60.

19. George Wald, "A Generation in Search of a Future," J. Allen, ed., March 4: Scientists, Students and Society (Cambridge: MIT Press, 1970), pp. 110-11.

20. Ibid., pp. xxii-iii.

21. Quoted in Charles Schwartz, "Professional Organization," Martin Brown, ed., The Social Responsibility of the Scientist (New York: The Free Press, 1971), p. 32.

22. See for instance John Gofman and Arthur Tamplin, "Nuclear Radiation," ibid., pp. 133-48.

23. Barry Commoner, "The Ecological Crisis," ibid., p. 175.

24. Ibid., p. 182.

25. Harvey Wheeler, "The Constitutionalization of Science" (Unpublished

manuscript, Santa Barbara, Calif., 1965). See also Wheeler's The Restoration of Politics (Occasional Paper, Santa Barbara, Calif.: Center for the Study of Democratic Institutions, 1965).

26. Nicholas E. Golovin, "A Fourth Branch of Government" (Unpublished manuscript, Washington, D.C.: George Washington University Program on Technology and Society, 1968).

27. W.H. Ferry, "Must We Rewrite the Constitution to Control Technology?", Saturday Review (March 2, 1968), pp. 50-54.

28. On the whole, Platt's earlier writings justify the "Activist" categorization, while his more recent work shows movement in the "Oppositionist" direction. He appears not to be alone in experiencing this kind of change.

29. Reprinted in Holton, op. cit., p. 317.

30. Stephen Toulmin, "The Complexity of Scientific Choice II: Culture, Overheads, or Tertiary Industry?", Minerva, Vol. 4, No. 2 (Winter, 1966), pp. 155-69.

31. N. Bukharin, et al., Science at the Crossroads (London: Frank Cass and Co., 1971), p. 31. (Capitalization included.)

32. See Bernard Nossitter's important article "Arms Firms See Postwar Spurt" on the aerospace industry and the thinking of many of its leaders, Washington Post (December 8, 1968), pp. A1, A18 and (December 9, 1968), pp. A1, A6.

33. Eric Mann, "Academic Freedom and the Military," Allen, ed., op. cit., pp. 75-77.

34. This group, with chapters in most university cities and towns, has taken its position to meetings of the AAAS and other scientific bodies. Here is a statement of one of its chapters (Boston) published in the May, 1973 issue of Science for the People (p. 43):

Science, in a capitalist society is used against the people. The basic control over scientific work and its further development is in the hands of a few people at the head of large private institutions and government bureaucracies. Under these people science is consistently utilized for the perpetuation and extension of economic and political power based upon the monopolization of natural and human resources in the hands of a few.

Science provides the technology which is utilized to exploit resources on the basis of maximum profitability. This results in inefficiency and waste and in ecological disruptions which assault our health and life.

Science provides the weapons of war and the tools of the police state, so that an obsolete economic system

can defend itself against growing movements for fundamental social change through force, violence and intimidation.

Science for the People means the explicit recognition of the political nature of science in this society.

Science for the People means access for all people to useful human knowledge.

Science for the People means the alliance of those who presently have access to scientific knowledge with movements for fundamental social change.

35. Carl Oglesby, "The Crisis is Political," Center Magazine, Vol. IV, No. 2 (March, 1971), p. 68. See also his "The Young Rebels," William R. Ewald, Jr., ed., Environment and Change (Bloomington: Indiana University Press, 1969).

36. John McDermott, "Technology: The Opiate of the Intellectuals," New York Review of Books (July 31, 1969).

37. See Michael Polanyi, Personal Knowledge (Chicago: University of Chicago Press, 1958), especially Chapter Six; "The Republic of Science," Minerva, Vol. I, No. 2 (1962); and Science, Faith and Society (Chicago: University of Chicago Press, 1964). Marlan Blisset argues convincingly, that Polanyi's vision of the Scientific Community is remarkably similar to Adam Smith's analysis of the self-regulating market. Politics in Science (Boston: Little Brown, 1974), pp. 11-24. See also Harvey Brooks, The Government of Science (Cambridge: MIT Press, 1968), p. 59.

38. Quoted in Reagan, op. cit., p. 36.

39. Quoted in Spencer Klaw, The New Brahmins (New York: Collier Books, 1962), p. 296.

40. Quoted in Reagan, op. cit., p. 37.

41. Bernard Barber, Science and the Social Order (New York: Collier Books, 1962), p. 296.

42. Norbert Wiener, "Science and Society," Science (November 9, 1962), p. 651.

43. See J.S. Dupree and Sanford Lakoff, Science and the Nation (Englewood Cliffs, J.J.: Prentice-Hall, 1962), p. 88.

44. Victor Weisskopf, "The Privilege of Being a Physicist," Physics Today, Vol. 22 (August, 1969), p. 43.

45. Victor Weisskopf, "The Need for Basic Research," Science, Vol. 167 (February 13, 1970), p. 935.

46. Colborn, ed., op. cit., p. 16.

47. Alan Waterman, "Integration of Science and Society," American Behavioral Scientist, Vol. VI, No. 4 (December, 1961), p. 3.
48. See Barber, op. cit., p. 275.
49. Elisset, op. cit., p. 55.
50. Ibid., p. 59.
51. Harold L. Neiburg, ed., In the Name of Science (Chicago: Quadrangle, 1966); Ralph Lapp, The Weapons Culture (Baltimore: Penguin, 1969); and Nigel Calder, Unless Peace Comes (New York: Viking, 1968).
52. This issue is treated in Chapter Five.
53. Derek Price, Little Science, Big Science (New York: Columbia University Press, 1962). See also Brooks, op. cit., pp. 60-61.
54. Eugene S. Schwartz, Overskill (Chicago: Quadrangle, 1971), pp. viii-ix.
55. Ibid., pp. 267-69.
56. Zbigniew Brzezinski, "America in the Technotronic Age," Encounter, Vol. 29 (January, 1968), pp. 25-26.
57. Ibid.
58. Quoted in Paul Durbin, "Technology and Values: A Philosopher's Perspective," Technology and Culture, Vol. 13, No. 4 (October, 1972), p. 560.
59. Daniel Bell, "Notes on the Post-Industrial Society (II)," The Public Interest, Vol. 7 (Spring, 1967), pp. 117-18. See also Daniel Bell, "Social Choice and Social Values," Bertram Gross, ed., A Great Society? (New York: Basic Books, 1968).
60. Amitai Etzioni, The Active Society (New York: Free Press, 1968).
61. Quoted in McDermott, op. cit., p. 25.
62. Emanuel Mesthene, "How Technology Will Shape the Future," Ewald, ed., op. cit., p. 145.
63. Melvin Krantzberg, "Technology and Human Values," Virginia Quarterly Review, Vol. 40 (Autumn, 1964), pp. 578-92.
64. John Kenneth Galbraith, The New Industrial State (Boston: Houghton Mifflin, 1967), p. 19. Although his language is suggestive of determinism, Galbraith cannot be so classified. For him an "imperative" is nothing more than a converging of vectors of societal forces, or (more simply put), a powerful trend. An economist whose opposition to the current direction of society is more pronounced, and whose views are becoming increasingly known, is Edward J. Mishan.

See, for instance, his Technology and Growth (New York: Praeger, 1969).

65. Galbraith, op. cit., p. 388.

66. Ibid., p. 399.

67. See, for example, John Kenneth Galbraith, "Power and the Useful Economist," American Economic Review (March, 1973), pp. 1-11.

68. Alvin Toffler, Future Shock (New York: Random House, 1970), p. 400.

69. Ibid., p. 424.

70. Robert Theobald, "Cybernetics and the Problems of Social Reorganization," Charles R. Dechert, ed., The Social Impact of Cybernetics (Notre Dame: University of Notre Dame Press, 1966), p. 68.

71. Donald Michael, The Unprepared Society (New York: Basic Books, 1968), p. 3. Political scientist Lynton K. Caldwell has similar ideas, seeing the task of education as "managing the scientific super-culture." He advocates in particular mid-career educational programs provided by government-university cooperation for individuals in important management roles. The cost per man-year he estimated at \$20,000 to \$25,000, but: "Cost what it may, the task is one that we cannot refuse to accept if we are serious about the prospects for survival." "Managing the Scientific Super-Culture," Public Administration Review, Vol. 27 (June, 1967), p. 113.

72. Denis Gabor, Inventing the Future (New York: Knopf, 1964), pp. 209-10.

73. Bertrand de Jouvenel, The Art of Conjecture (New York: Basic Books, 1967).

74. Michael Harrington, The Accidental Century (New York: Macmillan, 1965), p. 296.

75. Ibid., pp. 300-01.

76. Ibid., p. 275.

77. P. Teilhard de Chardin, The Future of Man (New York: Harper and Row, 1964), p. 196; also D. Hopps, "Teilhard de Chardin and the Vision of the Scientific State: A Critical Evaluation," (Unpublished Ph.D. Dissertation, University of Washington, 1969).

78. Quoted in Donald W. Schriver, Jr., "Man and His Machines," Technology and Culture, Vol. 13, No. 4 (October, 1972), p. 533.

79. Marshall McLuhan with Quentin Fiore, War and Peace in the Global Village (New York: McGraw-Hill, 1968), p. 3.

80. Ibid., p. 190.

81. Quoted in Lewis Mumford, The Myth of the Machine: The Pentagon of Power (New York: Harcourt Brace and Jovanovitch, 1970), p. 56.
82. William Kuhns, The Post Industrial Prophets (New York: Weybright and Talley, 1971), p. 230.
83. This group of people, the systems designers, have been called "The New Utopians" by Robert Boguslaw in a perceptive book of that title. The classical utopians, he says, at least tried to plan perfect societies by conceiving of perfectable human beings; the new utopians would, ignoring human nature, utilize people, machines, systems, and statistics to plan the perfect society. (Englewood Cliffs, N.J.: Prentice-Hall, 1965), pp. 14.
84. See Nelburg, ed., op. cit., pp. 200-17.
85. Simon Ramo, "New Dimensions in Systems Engineering," Brownell, ed., op. cit., pp. 127-46.
86. Quoted in Theodore Roszak, Where the Wasteland Ends (Garden City, N.Y.: Doubleday, 1972), pp. 36-37.
87. Ibid.
88. B.F. Skinner, Beyond Freedom and Dignity (New York: Bantam, 1971), p. 119.
89. Ibid., p. 169. In Boguslaw's terms Skinner qualifies as new utopian and, with his early work Walden Two (1948), as a classical utopian as well.
90. Skinner, Beyond Freedom and Dignity, op. cit., pp. 200-01.
91. See Sam Love, ed., Earth Tool Kit (New York: Pocket Books, 1971); Garrett de Bell, The Environmental Handbook (New York: Ballantine, 1970); Paul Ehrlich and Richard Harriman, How to be a Survivor (New York: Ballantine, 1971); and Dennis C. Pirages and Paul Ehrlich, Ark II (San Francisco: W.H. Freeman, 1974).
92. Ehrlich and Harriman, op. cit., pp. 134-37.
93. Love, ed., op. cit., p. 10.
94. Examples are: Robert Jungk and Johann Galtung, eds., Mankind 2000 (London: Allyn and Unwin, 1969); Gerhard Hirschfeld, The People: Growth and Survival (Chicago: Aldine, 1973).
95. Michel Crozier, The Bureaucratic Phenomenon (Chicago: University of Chicago Press, 1964), p. 300.
96. Ibid., pp. 164-65.
97. Victor Ferkiss, Technological Man: The Myth and the Reality (New York: Braziller, 1969), p. 25.

98. Erich Fromm, The Revolution of Hope (New York: Harper and Row, 1968), see especially pp. 93-140.
99. Murray Bookchin, Post-Scarcity Anarchism (Berkeley: Ramparts Press, 1971), p. 135.
100. Paul Goodman, "Can Technology be Humane?," Brown, ed., op. cit., p. 263.
101. Perhaps the two most important of Lewis Mumford's many works on this subject are Technics and Civilization (New York: Harcourt Brace and World, 1963) and the two volumes of The Myth of the Machine (New York: Harcourt Brace and Jovanovitch, 1967, 1970).
102. Mumford, Technics and Civilization, op. cit., pp. 403-06.
103. Jean Meynaud, Technocracy (New York: The Free Press, 1968), p. 66.
104. Ibid., p. 299.
105. Ibid., pp. 301-02.
106. Hans Morgenthau, Truth and Power (New York: Praeger, 1970); Scientific Man versus Power Politics (Chicago, University of Chicago Press, 1946).
107. Charles Reich, The Greening of America (New York: Random House, 1970), p. 4.
108. Ibid., p. 19.
109. Roszak, Where the Wasteland Ends, op. cit., p. 379.
110. Ibid., p. xvi. See Theodore Roszak, The Making of a Counter-Culture (Garden City, N.Y.: Anchor, 1969).
111. An analysis of long term developments leading toward modern approaches to technology is given in Manfred Stanley, ed., Social Development: Critical Perspectives (New York: Basic Books, 1972), Chapter Ten.
112. Ortega Y Gasset, The Revolt of the Masses (New York: Norton, 1967).
113. F. Junger, The Failure of Technology (Hinsdale, Ill.: Humanist Press, 1949); R. Seidenberg, Post Historic Man (Chapel Hill: University of North Carolina Press, 1950); W.F. Whyte, The Organization Man (New York: Simon and Shuster, 1956); H. Arendt, The Human Condition (Chicago: University of Chicago Press, 1958).
114. Seidenberg, op. cit., pp. 60-61.
115. Ibid., p. 107.
116. Quoted in Roger Williams, Politics and Technology (London: Macmillan, 1971), p. 32.

117. Arendt, op. cit., p. 3.
118. Jacques Ellul, The Technological Society (New York: Vintage, 1967), p. 391.
119. A European scholar, Jurgen Habermas, holds views on science as well as general social philosophy which are similar to Marcuse's. Because of Marcuse's importance in the U.S., we will only consider him. Habermas' analysis can be found in Toward a Rational Society (Boston: Beacon, 1970).
120. Herbert Marcuse, One Dimensional Man (Boston: Beacon, 1964), pp. 9-11.
121. Herbert Marcuse, "Remarks on a Redefinition of Culture," Holton, ed., op. cit., pp. 231-32.
122. "If now, in the rebellion of the young intelligentsia, the right and the truth of the imagination become the demands of political action, if surrealist forms of protest and refusal spread throughout the movement, this apparently insignificant development may indicate a fundamental change in the situation. The political protest, assuming a total character, reaches into a dimension which, as aesthetic dimension, has been essentially apolitical. And the political protest activates in this dimension precisely the foundational, organic elements: the human sensibility which rebels against the dictates of repressive reason, and, in doing so, invokes the sensuous power of the imagination.... But instead of being shaped and permeated by the rationality of domination, the sensibility would be guided by the imagination, mediating between the rational faculties and the sensuous needs.... The imagination, unifying sensibility and reason becomes productive as it becomes practical: a guiding force in the reconstruction of reality--reconstruction with the help of a gaya scienza, a science and technology released from their service to destruction and exploitation, and thus free for the liberating exigencies of the imagination. The rational transformation of the world could then lead to a reality formed by the aesthetic sensibility of man. Such a world could (in a literal sense:) embody, incorporate, the human faculties and desires to such an extent that they appear as part of the objective determinism of nature--coincidence of causality through nature and causality through freedom." Herbert Marcuse, An Essay on Liberation (Boston: Beacon, 1969), pp. 30-31.
123. Marcuse, One Dimensional Man, op. cit., p. xv.
124. Wilbur Ferry, "The Technophilliacs," Center Magazine, Vol. 1, No. 5 (July, 1968), p. 48.
125. Weinberg, "In Defense of Science," op. cit., p. 145.
126. McDermott, op. cit.

CHAPTER THREE

Concepts of Scientific Community

This chapter explores an equally relevant area of discussion which may be described as "internal" to the activities of science. We are not concerned here with the relationship of science to social reality external to it, but with the meaning of science itself as understood by leading contemporary observers. Various definitions given, and the analytical frameworks in which these have been utilized, are explored and related to specific examples of current experience. Finally, the question of whether the "scientists of science" have provided useful conceptual tools for our purposes is considered. The answer will be no, not directly, since their studies suffer from a weakness complementary to that of the literature analyzed in Chapter Two. Indirectly, however, this literature is very useful, for in what is missing from the scientists' self-conception lies a clue concerning means for overcoming some obstacles encountered in assessing the relationship of science to contemporary society.

In the first chapter we described significant recent changes in the relationship of science and society associated with the concept of a "second scientific revolution." In the period of the first scientific revolution the Cartesian picture of a remote science was still the most accurate; Bacon and his successors were more visionaries than social analysts. The Baconian conception of science in service to man applies more to our time than to theirs.

In Chapter Two we examined arguments of both scientists and non-scientists who have written on the relation of society to science and found that the philosophical difference between the Cartesian and Baconian positions is either not acknowledged or treated superficially--and is seldom followed to any logical conclusions. Too often the writers simply avoid this central theoretical issue. A result has been that the practices of American scientists are still viewed in largely Cartesian terms, while spokesmen often appeal to Baconian reasoning to justify (and increasingly to organize) science's growing position of interdependence with other parts of society.

This situation of duality, we have suggested, is inherently unstable and, although the contradictions remained much beneath the surface during the 1950's and early 1960's, their emergence since then has been instrumental in bringing science under increasing challenge.¹ This suggests that the problem lies very deep, in the self-conception of scientists and ultimately in definitions of science itself as understood both by scientists and by outside observers. If the definitions of science are Cartesian, while much of its practice has become Baconian, then there is a disjuncture between science and its putative content, a "cultural lag," which inhibits analysis.

To begin, we shall illustrate this disjuncture. Here are nine quite commonplace examples of "scientific" activity in the United States today emerging from our general descriptions in the previous chapters. Do they fit the general definition of science as understood by scientists themselves?

1. Mr. X, with a Ph.D. in chemistry, works in Corporation A, developing new techniques for the processing of food. His work is

original but never publicized for fear of revealing corporate secrets.

2. Dr. Y is a scientist working for the Defense Department on totally classified research. No one except his government superior knows its content or its applications.

3. Professor Z is one of a group of 40 scientists on the verge of a major breakthrough in high-energy physics. Seeking to gain the necessary financial support for this very expensive project, he becomes involved in university, state and national politics.

4. Dr. N is vice-president of a small but prosperous research and development firm which has contracts with local and state governments as well as several federal agencies.

5. Dr. D, a well respected biologist, spends most of his time consulting with various agencies of government as well as private institutions regarding the applications of his special field of knowledge.

6. Scientist P is an administrator in a large government agency. The tasks of his agency are set out by statute and his work is to organize a staff of hundreds of scientists and laymen for these purposes.

7. Scientist B, a well known Nobel Prize winning physicist, is opposed to testing nuclear weapons. Much of his time is spent making speeches on its dangers, organizing letter-writing campaigns, and in related activities.

8. Scientist C is a member of a systems analysis task force appointed by a large city mayor to investigate local transportation problems and propose improvements.

9. Scientist T is Assistant Secretary of Defense, and so shares in political decision making responsibility.

These cases give us a picture of activity carried out by scientists far removed from the common picture of scientific research. The fact that such activity is taking place is not of concern here--it is documented historically in Chapter One and its importance has been noted by various observers discussed in Chapter Two. Of concern here is how definitions of science relate to these activities.

Heretofore, we have largely refrained from defining terms, having been able to proceed utilizing mainly standard usages of the terms we employ. This has been the case with our usage of the words science, scientists, etc. However, we have reached a point where greater precision is required. When we try to spell out what actually is meant by science we find the going difficult. Obviously one could define science as the study of physics, chemistry, biology, and scientists as the students and graduates of this or that discipline, but this would have little value since it does not provide any criteria for selecting which activities qualify for inclusion. Perhaps we may find some assistance through a brief excursion into the philosophy of science.

Traditionally science is differentiated from other intellectual activity by its internal method. Science, it is said, consists of the verification of theories through replication, quantification, etc., progressively proceeding from one theory to another, and providing additional knowledge in the process. The traditional philosophy of science has been reformulated by Karl Popper who places emphasis upon falsification, for he takes verification to be an unattainable goal.

In this view, science consists of theories which are constantly

subject to falsification through experimental investigation, and to replacement if the falsification succeeds. "A scientist ... constructs hypotheses, or systems of theories, and tests them against experience by observation and experiment."² Science is thus still understood as a creative enterprise whose practitioners engage in critical work toward continual progress through replacement of outmoded theories.

Lakatos, basically following Popper, has added the caveat that falsification leads to the replacement of one theory by another only when an alternate theory is available.³ While Popper and others in his school modify somewhat the traditional philosophy of science, they nevertheless maintain the main thrust, distinguishing science from other intellectual activities by its critical character as manifested in the continual building and testing of theories.

While there have been numerous works on the traditional view of science, the most explicit and controversial opposing view has been formulated by Thomas Kuhn. Kuhn disputes the Popperian description of critical science, arguing that only in unusual or crisis situations are theories or rather "paradigms" in fact discarded and replaced. This "revolutionary" science stands in contrast with the usual or "normal science." Under normal conditions each "scientific community" operates within a consensually accepted paradigm so that scientists' defining activity consists of solving "puzzles." Because these puzzles themselves are derived from the paradigm scientists are not concerned with theory formulation or even falsification, but merely with elaborating certain aspects of the paradigm:

Scientists do repeatedly subject to systematic test ... statements of an individual's best guesses about the proper way to connect his own research problem with

the corpus of accepted scientific knowledge. He may, for example, conjecture ... that a newly discovered spectral pattern is to be understood as an effect of nuclear spin.... The next steps in his research are intended to ... test the conjecture.... If it passes enough or stringent enough tests, the scientist has ... resolved the puzzle he had set.... Tests of this sort are a standard component of what I have ... labelled 'normal science' or 'normal research', an enterprise which accounts for the overwhelming majority of the work done in basic science. In no usual sense, however, are such tests directed to current theory. On the contrary ... the scientist must premise current theory as the rules of his game. His object is to solve a puzzle ... and current theory is required to define that puzzle and to guarantee that, given sufficient brilliance, it can be solved.... If it fails the test, only his own ability not the corpus of current science is impugned.⁴

Kuhn states that "in its normal state ... a scientific community is an enormously efficient instrument for solving the problems or puzzles that its paradigms define,"⁵ precisely because scientists are not distracted by having to defend the basic principles of the paradigm:

One 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 assumed to have solutions. To a great extent these are the only problems that the community will admit as scientific or encourage its members to undertake.... One of the reasons why normal science seems to progress so rapidly is that its practitioners concentrate on problems that only their lack of ingenuity should keep them from solving.⁶

The replacement of one paradigm or general theory by another does not, then, result from normal science. Each paradigm, according to Kuhn, carries with it its own lexicon of symbols and hence testing one against another requires a form of translation that cannot be expected to take place without some agony. A new paradigm can only be adopted when more scientists begin to adopt its lexicon and so to define the

puzzles they are attempting to resolve within it.⁷ Clearly, argues Kuhn, the trauma involved in the adoption of one such language to replace another cannot be characteristic of normal science. Even so, unlike other communities attempting to adjust to a crisis of belief, when necessity arises, the scientific community can rapidly adapt to a new paradigm and redefine its membership--in the process disposing of "old believers."

It is not our purpose to give full elaboration of the views of Kuhn and Popper. Clearly Kuhn is correct in identifying the existence of those activities he terms normal science. In his response, Popper does not deny their existence; he merely attempts to deny that they define what science is.⁸ On the other hand Kuhn has been properly criticized for overstating the difference between normal and revolutionary science, as he is unable to demonstrate distinctive criteria for evaluating whether prevailing scientific activity in a given period falls within the normal or revolutionary mode,⁹ or set out the conditions necessary to bring about the overthrow of a ruling paradigm.¹⁰

We are not concerned with the pros and cons of this highly interesting debate per se, but rather with what light it may shed on the definition of science. The Kuhn thesis presents the traditional formulation with certain questions. First, is it possible to distinguish science as a social entity; and second, does the practice of science occur in one or many communities of science?

For our purposes, the first question can be easily resolved. While Kuhn insists upon a reformulation of understanding of scientific method, he nevertheless accepts the fact that science is different from other intellectual pursuits--insisting that only with a clear

understanding of normal science can we really determine what is or is not scientific. Kuhn and Popper agree that the ability to make continual progress through improving theoretical understanding distinguishes science from other intellectual pursuits, but Kuhn adds that this is possible only because normal science is spared the traumas of revolutionary science. Conversely, other fields of knowledge are distinguished from science by their lack of an acknowledged paradigm, without "which the puzzle solving activity fundamental to science cannot take place."¹¹ These distinctions seem unobjectionable.

Let us add, moreover, that Kuhn's work should put an end to any remaining popularity of the belief that science is inherently a "revolutionary" activity whose method guarantees that conventional wisdom is constantly subject to the most thorough and far-reaching scrutiny. For Kuhn, science not only does not conform to this image, but cannot and indeed must not take on this revolutionary form except in rare instances if it is to be successfully advanced. This point should be kept in mind in consideration of the work of the sociologists of science. Furthermore, it adds weight to our expressed misgivings vis-a-vis the compromise of Baconian reality with Cartesian principles noted earlier. From Kuhn's analysis, we might suggest that the paradigms adopted in different scientific disciplines may strongly reflect research decisions based on the kind of societal (governmental, industrial ...) support available. If so, we have little reason to expect that scientific methods used in such research will provide desirable critical scrutiny, premising as they generally must the ruling paradigm.

The second question of whether the practice of science takes the form of one or many communities would, at first glance, seem to pose

and either/or proposition. If a science is defined by the existence of a paradigm, then disciplines may be seen as constituting separate communities, and seemingly one would have to speak of "sciences" rather than of science. However, while there may be many communities of science, different paradigms do not necessarily require different norms. Lines between scientific disciplines may be significant under normal science, but as Kuhn points out the entry of scientific knowledge from one field into another is quite characteristic of revolutionary science--when adoption of a new paradigm sometimes results in establishment of a new discipline or sub-discipline. This is instructive, for as new sub-disciplines--say in physics--develop, we do not see the discipline disappear; such may also be said of science as a whole. Furthermore, distinctions between normal and revolutionary science are not easily made in practice, because these are not absolute but relative differences. At a given moment in any discipline one may find instances of both normal and revolutionary science--and be forced to wait until hindsight shows which was the more important. Whatever the final verdict, if such is possible, one can more broadly say that no individual discipline or scientific community can be totally isolated from the greater scientific issues raised by revolutionary science. We should therefore avoid positing an either/or dichotomy and affirm that one may speak without contradiction of both one scientific community and of many--and that each concept has equal sociological validity.

Kuhn's work encompasses both the philosophy of science and the sociology of science, and draws attention to the latter by an analysis based more on what scientists do than on what scientists (or philosophers) say they do. He does not, however, make use of a social

science conceptual framework to study why scientists behave as they do, and so we now turn to the work of some scholars who might be called sociologists of science. Their particular contribution has been in their conceptualization of scientific values as not simply (supposedly) guiding the research activities of individual scientists, but more importantly shaping the ways in which scientists relate to one another--how they act to form a community. This school has limitations, but it also has provided valuable insights into the internal workings of science as a social enterprise or system.

Five of the essays in Robert Merton's Social Theory and Social Structure treat different aspects of scientific enterprise and together constitute the classic contemporary work in the sociology of science. In one of them, Merton characterizes the ethos of science as comprising four "sets of institutional imperatives--universalism, communism, disinterestedness, and organized skepticism."¹² Universalism requires that scientific work be accepted or rejected everywhere according to the same criteria. Communism (or communalism) posits that all scientific knowledge belongs to all of science, that there is no "private property" in science. Disinterestedness requires that scientists have no personal stake in the outcome of their research or in judging the work of other scientists. This discourages fraud. Finally, organized skepticism means judgment is always suspended until the facts are at hand, and all judgments are subject to reevaluation and rejection.

Merton's work was taken up several years afterwards by Bernard Barber who uses the term "values" to describe what he takes to be the crucial element of any scientific enterprise. The scientist holds to the following six values: rationality, emotional neutrality,

universalism, individual freedom, communality and disinterestedness.¹³ Barber has added rationality and emotional neutrality to Merton's imperatives and substituted individual freedom for organized skepticism, but this is not major since Barber is pointing to what he sees as the form of organized skepticism, that is the freedom of each individual scientist to do his work, whatever pre-established concepts it may challenge. Emotional neutrality and rationality add dimensions of context. Emotional neutrality appears anterior to the other norms. Rationality refers to that aspect of scientific method basic to which are standards allowing for proof to be demonstrated and replicated. The sum total of Barber's contribution is thus to widen the insight provided by Merton without fundamentally altering it.

Warren Hagstrom's work which appears three years after Barber's, is less concerned with refining theoretical distinctions among norms prevailing in scientific work than with seeing them at work through the actual interrelationship among scientists and the direct influence of scientists upon each other's work.¹⁴ Hagstrom places great stress on Barber's norm of individual freedom which he finds to be central. Otherwise he shares essentially Merton's and Barber's conception. His interests lie in the question of why scientists conform to these communal norms. His interviews with many scientists led him to conclude that recognition on the part of other scientists is the primary incentive which scientists accept in exchange or reward for their acting in accordance with the norms of science. Thus, for example, he explains that popularization of scientific work is unacceptable to the scientific community because it would replace recognition by other scientists as the basic reward.

Norman Storer builds on the conclusions of Hagstrom, in effect

to reformulate the idea of scientific community and therefrom a definition of science.¹⁵ For Storer, the internal workings of the scientific community, its norms and structure, and the interrelationships among its members can be best understood within the analytical framework of social system theory. A social system is defined as a "stable set of patterns of interaction organized about the exchange of a quantitatively unique commodity and guided by a shared set of norms that facilitates the continuing circulation of that commodity."¹⁶ A viable social system must have members, a set of shared norms and values enabling the members to maintain stable interrelationships, and a distinctive incentive or reward, "both psychologically meaningful and institutionally appropriate, motivating the members to continue their participation in this social system."¹⁷

The norms are Barber's six imperatives of science while the distinctive reward is what he calls "response to creativity." While Storer obviously agrees that the reward of social recognition is basic to scientific activity as Merton first suggested and Hagstrom investigated, he "is not convinced that the question of why professional recognition is able to serve as the motivating reward in science has yet been adequately explained."¹⁸ The problem is to relate the prevailing norms and values of science directly to the reward system through recourse to general sociological social system theory. Each is necessary if an exchange system of response to creativity is to continue over time, so none is accidental. This relationship ties scientific practice to the existence and social patterns of the scientific community. Scientists at work support the scientific norms not because these are inherent in each individual scientific experiment but because scientists "are in some sense aware that these

norms are necessary if the exchange system of science is to operate properly."¹⁹

For Storer a social system is thus a stable pattern of inter-relationships of human beings which has developed around a particular commodity desired by all participants and acquired in exchange with others. In this way, for example: "The economic system may thus be identified as the complex of statuses or social positions whose interrelations are concerned with the exchange of money for goods or services, with money circulating in one direction and goods or services in the opposite direction."²⁰ Other examples offered by Storer are the political, the family, and the religious systems.

Storer argues that the urge to be creative is a normal human urge central to scientific activity and requires response to creativity. Storer thus sees the six norms of science actually embodying the principles of a social system. They function to maintain the ongoing scientific social system in which all participants have a stake; each scientist realizes he must help maintain this system as it provides the needed response to his own personal creativity. The three most important norms for the maintenance of the system are disinterestedness, organized skepticism, and communality. Communality guarantees that exchange can take place in the first place. Organized skepticism provides for scientists' supplying suitable responses to the creativity of peers. Disinterestedness adjures scientists against significant interest in any other commodity such as power, money, or popular acclaim.

A violation of these principles on a continuing and significant basis would threaten the system itself. He concludes: "As the preceding discussion demonstrates, science does have these

characteristics: scientists are engaged in producing new empirical knowledge and want to obtain each other's evaluations of their contributions: the norms of science guide their behavior in these areas so that all scientists can participate in this type of exchange."²¹

Because their first audience is restricted to a minority of the population composed of "experts" and their products can be communicated verbally, that is, in written form, science, Storer argues, is the only creative profession to have produced a separately viable and identifiable social system. Viewed as a social system, the scientific community becomes understandable and identifiable. All scientists are at least implicitly aware of the norms and how these are applied, for example, to ascertain the prestige to be accorded to individual scientists, to different works of science, to different universities where scientists are trained, and even to the different branches of science. This allows for a great deal of consensus as to the content of scientific education so that disputes concerning credentials are infrequent.

Storer's work is an advance in the sociology of science. He has added to Merton, Barber and especially Hagstrom's demonstration that in reality science (like truth and virtue) is not its own reward, but instead this reward consists of recognition by one's colleagues. He does this by placing science in the context of a functioning social system and showing how the norms of science are functional to the maintenance of that system. His choice of key terms however is somewhat misleading, notably the stress on creativity and the creative urge. A less grandiose term would seem more appropriate to describe the puzzle-solving activities that characterize normal science.

A recent addition though from a different vantage point, is in

the work of British theoretical physicist J.M. Ziman.²² Ziman takes a similar analytic position but from within rather than outside the scientific community. He asks at the outset: what is science; is it the mastery of man's environment; is it the study of the material world; is its defining characteristic that science arrives at truth by logical inferences, by the experimental method? All of these are often suggested and each has some validity but none is acceptable:

The answer proposed in this essay is suggested by its title: Science is Public Knowledge.... Science is not merely published knowledge or information. Anyone may make an observation, or conceive a hypothesis, and, if he has the financial means, get it printed and distributed for other persons to read. Scientific knowledge is more than this. Its facts and theories must survive a period of critical study and testing by other competent and disinterested individuals, and must have been found so persuasive that they are almost universally accepted. The objective of Science is not just to acquire information nor to utter all non-contradictory notions; its goal is a consensus of rational opinion over the widest possible field.

In a sense, this is so obvious and well-known that it scarcely needs saying. Most educated and informed people agree that Science is true, and therefore impossible to gainsay. But I assert my definition much more positively; this is the basic principle upon which Science is founded. It is not subsidiary consequence of the Scientific Method; it is the scientific method itself.²³

Ziman thus shares the perspective of the sociologists. What is essentially defining about science is its function as a social institution. The emphasis on public knowledge is similar to Merton's notion of communality. "Public" means open to one's peers, not to "the public." Ziman adds humility to the norms of science--explaining that public knowledge must be "persuasive" and humility adds to credibility. The linchpin of scientific activity, says Ziman, is the act of the referee. Scientific papers submitted for publication are sent out to two referees. Neither the author nor either of the

referees knows the name of any other of the three involved. Thus universalism is guaranteed. In this impersonal, "disinterested" way we see the exercise of the public aspect of scientific activity.²⁴ It is the most symbolic and also the most concrete expression of organized skepticism, and the public nature of this knowledge ensures its reliability in comparison to other knowledge.

What is rewarded through publication is the originality of knowledge communicated by individual scientists. The scientific method as it has developed is the most likely guarantee that individual scientists will obey the norms of science and that recognition will be based on originality—hence reliability is built in. Personal or private discovery, the mythical quality which has been so long heralded, is not really science; it becomes science only on being made public. In his understanding of what is "public" about scientific knowledge, Ziman uses the term in a way congruent with the sociologists' understanding of science as a social system—though he describes scientific activity in somewhat more glowing terms. It goes without saying that Ziman's unqualified use of terms such as "rational consensus," "discovery," and "originality" are to be taken with several grains of Kuhnian salt.

We should note generally that even as a description of academic science the analytical portrayal found in the work of Ziman and to a lesser extent that of Merton, Barber, and Storer is somewhat rosy. Apparently none give sufficient weight to the continual violations of scientific norms which occur within the scientific community. Numerous observers have noted the existence of a hierarchy within the scientific community which means that the scientific pronouncements of those at the top, or research of others supporting these pronouncements,

will be treated more favorably than the work of unknown scientists or of those who are seen as dissenters from approved principles. The viciousness of attacks against the theories of dissident astronomer Emmanuel Velikovsky is often cited as an example.²⁵

In addition, Kuhn's analysis, while not directly contradicting the conclusions of the sociologists of science, does place them into a somewhat different light. First of all, it is clear, the sociologists could and perhaps should have spoken of many communities of science as well as of one, though here there is no reason to accept that any of these sub-communities can be expected to operate noticeably differently from the others. It is not incorrect to describe each and also the total community of science as a social system. Secondly, as we noted, the emphasis of Storer for example on the "creative" aspect of scientific activity and that of Ziman on "critical study and testing" is shown to be somewhat overdone. Normal science is carried on at a much more mundane and routine level than they imply, with the result that enforcement of the norms is not as clear-cut as they suggest. Merton himself was heavily influenced by the traditional philosophy of science, concentrating his own historical research upon instances of more "revolutionary" science which often resulted in controversies over claims to priority of discovery.²⁶ The sociologists' stress on organized skepticism as a key norm of science, while not altogether wrong, is certainly overstated. While skepticism has its place in the solution of puzzles, only under revolutionary situations can this norm sometimes be seen to prevail over the major corpus of theoretical knowledge, over the paradigm.

In addition, none of the sociologists, it seems, gives sufficient attention to the particular kind of training and education that

scientists undergo and its effect. Kuhn emphasizes that most undergraduate and especially post-graduate training of scientists today is best understood as a process of initiation into the norms of the community and acceptance of the dominant paradigm. Textbooks play a major role in this and their nature illustrates how this process operates: "Textbooks refer only to that part of the work of past scientists that can be easily viewed as contributions to the statement and solution of the texts' paradigm problems.... [They] are implicitly represented as having worked upon the same set of fixed problems and in accordance with the same set of fixed canons that the most recent revolution in scientific theory and method has made seem scientific."²⁷

Insofar as we limit our discussion to the internal workings of the scientific community, the general insights of the sociologists remain, with the above reservations, valid and useful. Beyond these limits, however, we must raise serious questions which this perspective does not pose and therefore does not answer. Our understanding of normal science leads us strongly to suspect the norms to be far less encompassing and strictly operative than the sociologists imply. For our purposes, this is unfortunate. There is clearly a link between the rigidity with which the norms are enforced and adhered to, and the impact that external forces may have on the functioning of any scientific community.

The definitional problems concerning the scientific community seem clearly to lie deeper than the sociologists' insights can resolve. This may be partly due to the acceptance by Storer and most others of the internal definition of science as de facto empirically relevant to contemporary society. It has been maintained in many circles that systems theory in sociology and the structural

functionalism on which it is built is inherently conservative, accepting what is (or was) as functional and therefore as what must be. This methodology tempts sociologists to fail to appreciate and to analyze critical possibilities for recent or prospective change.²⁸ It may very well be that the same charge holds up against the systems theory analysis of science. Storer admits, for example, that his model is not perfectly accurate. He acknowledges that, given the large number of trained scientists, many must be and are engaged in day-to-day activities in industry and government such as those described above--the goals of which regularly contravene the norms of science. Nevertheless, Storer holds that these persons for the most part acknowledge this situation with the result that they are seen and see themselves at the fringes of the scientific community, or, to use the language of system's theory, they are "located on a wide boundary on one side of which lies the system," and "the system is so defined because of the frequency of transactions within it, and on the other side (lies) the environment--where such transactions are infrequent."²⁹

We are convinced that such an explanation should not be uncritically accepted. Is there not a certain point when a system begins to break down, when its norms become outmoded? In such a case, traditional norms may suffice neither to describe nor motivate the actions of participants; such norms may rather be preserved largely as a myth by vested interests served by such a myth. It has been argued that preservation of belief in the sanctity of "free market-place" principles during actual conditions of monopoly and/or oligopoly has been encouraged to serve such a function.³⁰ If this is the case for the norms of science, social systems theory could

serve to obscure this. One cannot expect definitively and assuredly to determine whether a system's breakdown has occurred or is occurring, but this line of questioning can at least be pursued a bit further.

The way in which science is organized and carried on, as one contemporary student of science concluded, "is highly dependent upon the social, economic, and political organization of society, and extremely sensitive to changes in this environment."³¹ Any discussion of the meaning of science must incorporate the Baconian aspect of contemporary scientists' roles such as classified research and other activities of scientists listed above. A recent work by Roger Krohn divides scientists into three groups, the "intellectuals," the "professionals," and the "organized scientists." Krohn challenges the strong tendency to identify the first group, consisting of academic scientists, with science as a whole since the two other groups, comprising industrial and government researchers on the one hand, and administrators in scientific research institutions on the other, may be just as important and play roles just as vital. The error, he concludes, is parallel to accepting a primitive tribe's self conception to explain its workings.³²

Krohn concludes by calling for, though not himself formulating, a wider conception of scientific activity which includes the work of the professionals and organized scientists. This new conception would serve as a "significant point of departure for studies in the sociology of science."³³ We agree; and this brings us back to our nine examples of active scientists above. Is it evident that these are merely exceptions which confirm the rule? There are two empirical problems with any such claim. The first is sheer numbers; there are just too many exceptions, too many trained scientists operating

outside an academic or "free" scientific setting. Secondly, the assumption that these scientists today see themselves as deviant--believing in norms they break every day--has nowhere been demonstrated. There may be sufficient evidence to suggest that such "deviance" was unimportant in the past, but there is also reason to believe that scientifically trained people have lately come to occupy a significant number of positions outside the reaches of norms supposedly operative in the social system of science.³⁴ It is not unreasonable to suspect that, for such people, their norms have changed, or at least been strained, by changing conditions. Krohn has voiced one such suspicion:

Thus far, the adaptations to the growth of our national science program have occurred largely inside universities and in university-government relations as, for example, the invention of the various forms of research grants and contracts. If continued growth strains these adaptations, we may see a decline in the predominance of universities in basic research, and consequently a decline of academic influence and ideological supremacy. At the 1963 meetings of physicists at the Learned Societies of Canada ... the suggestion was forcefully made by representatives of government and industrial laboratories that the training they give to young physicists is not inferior to that given them in universities. The quality of training and the supply of physicists, they argued, would be increased if selected government and industrial laboratories were entitled to grant the Ph.D. qualification in Physics. These straws in the wind may predict coming struggles.³⁵

It might be argued that the socialization of scientists which takes place in the educational process is thorough enough to lead to a lifetime commitment to the norms of science. To us this seems doubtful: the sociologists of science have argued quite effectively that the motivation of the individual scientist is less a matter of personal ethical commitment than an acceptance of the working norms of the social system of science in which one is a participant. Students are apprentice participants at best,³⁶ and their socialization,

it follows, ultimately depends on their full participation. Currently, with the rather high unemployment rate among scientists and the decrease in financial support for graduate education,³⁷ one might suggest that even apprentice scientists quite early in their careers learn to be skeptical about the possibility of their own full participation in the scientific community.

Clearly there is some tension between the actual practice of scientists in the U.S. today and the supposed norms that guide scientific activity. We cannot state to what extent these norms themselves have been replaced. For our own purposes it will suffice to list the violations of the norms of scientific activity reflected by various practices of contemporary scientists listed above:

1. Secret research, either governmental or corporate: a clear violation of universalism and communalism.
2. Research priorities being determined by government and corporate funding agencies often on non-scientific grounds: a violation of disinterestedness and organized skepticism.
3. The actual employment of scientists as administrators of other scientists in government agencies, technological firms, even university faculties: a threat to communalism, disinterestedness and the individual freedom required for organized skepticism.
4. The extremely high pay for research and development (R and D), consulting and similar work: a threat to the disinterestedness and emotional neutrality of scientific activity.
5. Scientists who are employed by government or corporations being rewarded on the basis of their direct contributions to the goals of the institution: a violation of universalism, communalism and disinterestedness.
6. The involvement of scientists in research activity which is so

highly organized and so large in size and scope as to result in the ignorance on the part of some participants concerning the very direction of their work: a violation of communalism and of rationality (of the scientific type--as distinguished from "bureaucratic rationalization" in Weber's terms).

7. The submission on the part of scientists to loyalty oaths, security checks and the like for screening purposes with regard to employment and research grants: a violation of universalism, communalism, organized skepticism, emotional neutrality and individual freedom.

8. Political roles played by scientists³⁸ which lead to their being identified and identifying themselves with particular political choices which affect scientific research: a violation of communalism and disinterestedness--and a threat to emotional neutrality on their part and on the part of other scientists with regard to them.

For example, the following excerpt from the report in Science on the elite National Academy of Science meetings in Spring, 1971 gives a sense of the manner in which "sociopolitical" issues crop up within the very core of the scientific community.

[Dr. George] Lewontin pressed his motion that the National Research Council not accept work which prohibits the council from disclosing results to members of the Academy. When a motion for adjournment was passed, effectively killing Lewontin's proposal, Lewontin submitted his letter of resignation, in which he said it was an 'intolerable contradiction' for scientists to be asked to bear responsibility for work they are not allowed to know about. He charged also that secret work is allowed at the Academy in the interest of a small group of Academy and National Research Council functionaries who have a personal interest in playing an important role in government....

Lewontin also played a role in the discussion of the council's action in omitting from the final list of nominees for membership the name of Lamont C. Cole.... The top 46 in the voting are named to membership in

order of preference.... It is also customary for the council to complete the roster of 50 new members by naming those on the general list in order of votes received from the membership. This year, apparently, the council skipped Cole, whose name topped the remaining nominees on the list, but picked the succeeding names in order for the unfilled spots.

The council's action was questioned from the floor.... Lewontin, for example, inquired which members of the council were ecologists qualified to judge Cole's work in his field. There was no response. It developed that the council's objections centered on Cole's public pronouncements, particularly a statement which appeared in a popular magazine saying that we were in danger of lowering the supply of oxygen in the atmosphere to dangerous levels.

At a press briefing that followed the business session, NAS President Handler was questioned about the incident, and one reporter asked if the council were making scientific work or public activities the criteria for membership. In his reply Handler indicated it was the council's view that it behooves a scientist to be even more sure of his facts when speaking before the public than before a scientific body.³⁹

Apparent regular violations of all the norms of science raise serious questions as to the viability of any definition of science based on these norms. The sociologists of science properly stress that what characterizes science is not a method applicable to each individual piece of scientific work (no such method is to be reliably found),⁴⁰ but is rather the process in which scientific work is judged and rewarded through recognition by other scientists. The continuity of such a system, they argue, depends on the willingness of scientists to adhere to the norms required for its operation--and the success of their demand for conformity from other scientists. Most do assume continuity. However unintended, their work has served to lend added credence to the unproven assumption that while scientists do such things as consulting, advising politicians, engaging in classified research, and administering, these may be considered peripheral.

In his definition of science as a "creative" profession, similar in this respect to art, literature, and music, Storer quite explicitly accepts the traditional Cartesian description of science. Such a definition allows Storer to define the reward as something which can be provided entirely within the internal structure of the scientific community--a reward analogous to the acclaim provided by art or music critics. If, however, the changes in the roles of U.S. scientists are as important as they appear to be, then the Cartesian view of science as a creative profession is becoming less and less apt. If this were generally recognized, the reward structure at the base of the social system of science might be quite different--recognition and reward being accorded less on the basis of one's contribution to "science" than on one's contribution to society in a manner analogous to service-oriented professions such as physicians or engineers.

The arrival of such a state of events would not ensure a breakdown in the operating norms of the social system of science. For one thing, the principle that others in the profession are to be the primary assessors of professional competence can surely be found among physicians and engineers. This alone could mean that some of the more central features of scientific practice would remain more or less intact. Nevertheless, with a reward structure which is fundamentally different, both the degrees and forms of compliance to the various norms would also be altered. This does appear to be occurring; communalism and disinterestedness are the first norms to be widely discarded, while universalism, rationality, and emotional neutrality are regularly violated. Organized skepticism, (within the bounds of normal science) remains as the most basic quality which distinguishes the enterprise of science. But this alone is insufficient, for it

is a norm shared with several professions oriented toward providing a direct service--especially engineering.

Since the work of the sociologists, while providing an important and useful analytical framework, does not adequately provide us with an instrument with which to define and describe science in contemporary society, we must be wary of this intellectual trap. Accepting the Cartesian self-conception of science and building around it, sociologists of science have tended to exclude from their conceptual framework the very real Baconian aspects of the scientific enterprise. This contributes to potential misunderstanding--taking science as scientists would perhaps wish us to take it, rather than as it is.⁴¹ For the moment we have a working definition of science but also a basis on which to judge its inaccuracy. How then are we to describe the scientific community?

Unfortunately we cannot easily arrive at a suitable definition of this term. Storer's description of the social system of science gives us one side of the picture of the community of science. We have already noted the difficulties raised by Kuhn's analysis. It appears equally legitimate to speak of a community of physics as it is to speak of a community of science. In either case, we may begin to define the scientific community as an implicitly organized institution containing roles, norms and a rudimentary but ongoing structure which ensures that the activities of its members conform to the norms. Such an organizational structure includes the existence of formal bodies such as the American Physical Society or the American Association for the Advancement of Science, scientific publications such as The American Physicist or Science, leaders growing out of recognizable hierarchy of prestige (such as the Nobel Prize in Physics

or selection to the National Academy of Science), or elite university affiliation,⁴² and an established procedure built around university accreditation for training, certification, and advancement.⁴³ Membership in the community is noted through such means as listing in American Men and Women of Science.

While the question of one or many communities of science is not of direct concern to us (we may perhaps speak of a "federal" community of science), the legitimacy of this definition is open to the same questions which confronted traditional definitions of science in general. We pointed to "exceptional" activities of science which violated the norms of the social system of science. We may point to various institutional factors which equally violate any Cartesian conception of the scientific community.

First is the fact that scientific organizations tend to be separated along national lines, that is, most organizations group scientists (or physicists) within one political unit--a principle in no way related to scientific norms. Furthermore, these institutions are frequently in part politically constituted. Secondly, scientific organizations have often been set up for specifically professional/political purposes such as is the case with the FAS, UCS and others described in Chapter Two. We can say the same of certain scientific publications such as the Bulletin of the Atomic Scientists. In addition, there is in evidence an increasing tendency, especially in Europe, toward the unionization of scientists.⁴⁴ The existence and importance of such factors must put into question any definition of the scientific community as contiguous with its internal social system. The fact that one is editor of the Bulletin of the Atomic Scientists, or science advisor to the U.S. President, or director of the National

Science Foundation or the Federation of American Scientists, must add to one's prestige within the scientific community even though one's attainment of such positions was no doubt based on qualities above and beyond (and perhaps contradicting) those defined as valuable under the operating norms of the social system.

From the outside, science is an esoteric body of knowledge, arcane and essentially unfathomable, upon which judgments, decisions, expenditures and commitments are nevertheless regularly made. Public and policy maker alike are dependent upon scientists designated by their peers as expert to aid in such matters. If so, this area of judgment may extend to the actual determination of scientific boundaries by scientists themselves--a crucial activity in the external relations of science.

Internally, the scientific community sees itself as caring little if at all about boundaries of scientific knowledge. This is equally the case if one is speaking of scientific communities. Scientists insist the boundaries are wherever the prevailing state of scientific knowledge has reached at the moment, so one should understand these to be shifting and temporary. Any attempt to concretize lines of demarcation would potentially damage science by inhibiting scientific research at the frontiers where the most important accomplishments are the most inclusive. Furthermore, such an attempt would violate the key scientific norm of organized skepticism. This perspective would be adequate were science today playing an entirely Cartesian rather than an increasingly Baconian role. However, when they testify before Congress, advise governmental agencies, serve industry, make public declarations on controversial issues or engage in any other

"peripheral" activities, scientists are often forced to acknowledge this problem.

Science per se provides no guidelines for demarcating boundaries in such a manner that an outsider could distinguish on his own what is legitimate scientific knowledge from what is not. No analysis of the internal workings of science can provide a basis for making such a distinction. Yet without any such basis, questions concerning the relations of science and society must remain unanswered and enigmatic. Progress toward resolution of virtually any of the problems posed in Chapter Two seemingly would require a working external definition of science, that is, an understanding of the meaning of science from a societal perspective, to help us begin to establish a framework in which the various assessments can be placed. This is the task set out for us in the next chapter. On our way toward doing so in concluding this chapter, we shall critically examine one important study to show some of the conceptual pitfalls lying in wait for the intrepid student of science in American society.

A very interesting empirical study of American science was undertaken by Dean Schooler, Jr. His project was to identify conditions relating to the influence of scientists and scientific knowledge on the making of public policy in the U.S. executive branch of government. In arriving at his definition of science, Schooler ignored the work of the sociologists:

'Scientists' will be defined broadly as individuals who are perceived to develop knowledge using scientific methods. Generally, these methods imply such norms as empiricism control, quantification, replication, and verification. This definition should be sufficiently inclusive to cover physicists, 'scientific strategists', systems analysts, economists, behavioral scientists, statisticians, engineers, and related scientific skill groups.

Science is a form of knowledge. Personal knowledge, moral truth, religious beliefs, philosophies, and common sense are other forms of knowledge or ways of knowing. But scientific knowledge is empirical, specific, replicable, verifiable, and sometimes quantifiable.⁴⁵

Scientists were thus defined as individuals engaged in a particular type of intellectual activity and science is the sum total of those activities. The collective enterprise of science and the interrelations of scientists were neither mentioned nor considered.

Schooler looked at twenty different policy "types"--from economic policy to space policy--in which there is some scientific involvement, and divided these into nine policy "arenas" based on a framework developed by Theodore Lowi. His findings are summarized in the graph below:⁴⁶

FIGURE 3-1

Policy Arenas								
Self-Regulative	Social Redistributive	Governmental Redistributive	Extra-National	Distributive	Regulative	Economic Management	Communal Security	Entrepreneurial
								Science
						Fiscal and Monetary	Weather Weapons Deterrence and Defense Health	Space
					Pollution Trade and Balance of Payments Conservation Antitrust Transportation Safety			
			Foreign Aid Disarmament and Arms Control	Agriculture Transportation				
Extraction	Social	Organization	Foreign Political					

SCIENTIST'S LEVEL OF INFLUENCE

1-01

2000000000

1-01

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2000000000

2000000000

Schooler limited himself to analyzing the direct influence of individual scientists on the executive branch, and thus, as we shall argue, probably underestimated the total influence of scientists on federal public policy-making. After individually assessing each arena of scientific influence, Schooler attempted to establish some correlation between factors prevailing in these and the different levels of scientific influence.

Schooler summed up his work in describing several conditions, the presence or absence of which affect the influence of scientists in public policy. Each, save one, can be applied more or less equally to any relatively organized group involved in questions of public policy. These relate to such matters as the presence or absence of: a zero-sum policy situation; a perceived need to change the "rules of the game;" unity among the scientists involved; an ability to provide tangible rather than symbolic results; and scientists' own vested interests. The one point of major interest to an understanding of the specific role of science and scientists, in Schooler's words, is this:

Scientists' influence within the executive branch increases when available or potential means to public policy objectives are material or "technological" rather than behavioral....

Scientific professions, such as medicine, straddle a similar dilemma. Doctors prescribe technological means to health (pills, injections), and they are reasonably successful. But when their solutions depend on changed behavior ... they meet with less success.... Scientists' forte lies in technological means.⁴⁷

After Sputnik, "scientists were given wide latitude, assuming that their activities would restore lost prestige, assure American control of space, and overcome the threat of Soviet missiles to Americans' military security. Science could provide a technological

solution to the loss of prestige, insecurity, and the desire to establish escape into outer space.⁴⁸ Similarly, Schooler found that weapons achieve "communal security." Even though they are costly in income taxed, they are an easy technological path to security. Weapons policy, like health, conservation, pollution, and other policies, loses public support when it requires individuals to commit their time or alter their life-styles. The anti-ballistic missile system (ABM/Safeguard) overcomes these popular objections; civil defense, requiring popular participation, does not. Technological paths to security are psychologically more reassuring than diplomatic and personal paths. Air power is more antiseptic and less "dirty" than ground warfare, and it costs fewer American lives.⁴⁹

Although Schooler's approach is insightful and his empirical observations interesting, the direct results of his study are disappointing for our purposes. Scientists simply appear to have more influence in areas where influence is more easily available to them. Such can be said of all organized groups. The influence of scientists (and presumably that of technologists and engineers) specifically relates only to the availability of technological solutions. Let us consider this.

What does it mean to say that scientific influence is greater in areas where technological rather than behavioral means to achieve public policy objectives are available? In part, this assertion is a matter of common sense. Normal patterns of human and social inertia assure that, all other things equal, the influence of anyone is likely to be greater when the kinds of goals he advocates can be achieved with less change in accustomed patterns of behavior or habitual thinking. Using Alvin Weinberg's celebrated phrase, Schooler's

conclusion is that scientific influence is greater where a technological fix is possible than where behavioral engineering is necessary. The close interrelatedness of today's science and technology means this basis for influence is not specious. Following Schooler, we might conclude simply that as more and more spheres of social life become increasingly technologized, scientific influence grows.⁵⁰

An unavoidably serious initial difficulty which is bound to plague any such conclusion of this kind, is the problem of causality. (This shall be equally true of our own analysis where, using the concept of scientific authority, we assume a preponderance of causation moving in one direction without being able to prove this is the case.) Arthur Stinchcombe helps to show what would be required. He writes: "A causal law is a statement or proposition in a theory which says that there exist environments (the better described the environments, the more complete the law) in which a change in the value of one variable is associated with a change in the value of another variable and can produce this change without any change in other variables in the environment." We think some correlation between (a) the availability of technological fixes and (b) scientific influence does seem to exist, but would be foolish to attempt to apply any causal law to this observation. It could be argued that (a) is one of many facilitating conditions which would tend to enhance (b)--even that (a) may be a prior necessity for the exercise of (b)--but that which is necessary may not be sufficient, and in human affairs seldom is. Stinchcombe uses a metaphor to represent causality as an arrow with only one shaft. This is "quite different from the double-shafted arrow of logical implication [to be used] when discussing the testing of theories rather than their formulation."⁵¹ Robert MacIver has

written well of the peculiar difficulties which beset any effort to investigate the causes of social phenomena:

Any social change we seek to explain is meshed in a tangled web of its inclusive history. It is dependent on conditions arising within every order of reality--physical, biological, psychological, and social. The factors we invoke embarrassingly combine the universal elements of physical causation and the human elements of teleological causation. The phenomena themselves are often hard to demarcate. What shall we include in a social movement...? They have no clear boundaries marking where one ends and another begins. They have ever-changing configurations. All the time each conjuncture is undergoing subtle modifications. There is unbalance and mobility, the constant emergence or injection of new elements. What we call the movement or the trend is an aspect of an endlessly variant flux. We cannot isolate it for experimental study. We cannot find two identical instances of it, or even two instances with no relevant difference between them. Nor can we find two instances that differ only by the presence or absence of a single clearly designated factor.⁵²

Schooler's assertion that scientific influence is greater where technological fixing is available should not be dismissed, however; apart from other considerations, it raises an important theoretical problem at the core of his and similar work. The problem consists of the selection and definition of key concepts in a manner which results in the automatic and unstated exclusion of vital elements that cannot be encompassed within the framework adopted. We noted this problem in our conclusion to the preceding chapter; we note it here again.

In attempting to assess what he terms the influence of scientists, Schooler pays no attention to the complex social character of the scientific enterprise, choosing instead to define it in simple terms relating to individual expertise, an individual's acknowledged possession of one kind of clearly identifiable knowledge. Defining it in this way, Schooler does not even consider the possible existence of differing views and conceptions of scientific knowledge which must

complicate and potentially alter the relationship of scientist to policy maker and of science to the public. Stuart S. Blume, in a perceptive study, underscores this point in more general form.

In attempting to grasp the political functions (and, more fundamentally, the political attitudes) of intellectuals, sociologists have explored individuals' relationships to the social system.... But there is an alternative perspective, which in my opinion offers a more profitable vantage point for observation of the political roles of scientists as scientists. Unlike intellectuals of left and right, who may be united by passage through a similar educational system (but perhaps by no more than nationality), scientists - whether acting the roles of expert, critic, or observer - are united by much more. They are linked, after all, by the consensual nature of their knowledge and the communal nature of their profession, to an extent not equaled by those engaged in other intellectual pursuits. The perspective of the sociologist of science suggests another viewpoint: the political roles of scientists might be examined in terms of the statuses of their actors within the scientific community. That the role of the scientist as expert is a valid concern of the sociologist of science is given added weight by the fact that the (political) value of his advice is frequently dependent upon his relationship to the scientific community.⁵³

If we choose to look at scientific knowledge as more than a particular body of knowledge associated with a number of easily identifiable individuals but rather as a system of norms and values embedded within the workings of the social system of science and to some extent in the American culture, then science must be understood not merely as an attribute of scientist A in his relationship with non-scientist (policy maker) B, but as an element in the institutional context in which their relationship is situated. To assert that scientific influence varies positively with the availability of technological fixing when science is understood in this wide sense, turns out to raise rather than resolve basic questions. These questions constitute a transition from the methods discussed here to the theme of the next chapter.

To conclude, the work of the scientist of science, from Kuhn to Ziman, has contributed important, though indirect assistance. They have unmistakably depicted the essential social nature of the scientific enterprise. Yet this literature too has been found lacking for our purposes, for it fails to incorporate the growing number of activities of science external to the self-conception of the scientific community and in violation of its basic norms resulting from the increasingly Baconian position of American science. Consequently, social definition of science which incorporates its contemporary Baconian side is required.

This is undoubtably a tall theoretical order, and our hope in the next chapter is but to make a small advance in that direction, and in Chapter Five to illustrate how a conceptual framework developed in such a way might possibly provide a useful explanatory tool. The first step is to find an appropriate terminology of linkage or externality between science and society, or science and public policy. Schooler, as we noted, chose to speak of scientific influence. We shall suggest that his definition of influence is inadequate for such purposes and ultimately that a more suitable concept than influence is required and available.

Notes

1. See for example, Edward Shils, "Anti Science," Minerva, No. 9 (October, 1971), pp. 441-53.
2. Karl Popper, The Logic of Scientific Discovery (London: Hutchison, 1959), p. 27.
3. Imre Lakatos, "Falsification and the Methodology of Scientific Research Programs," Lakatos and Musgrave, eds., Criticism and the Growth of Knowledge (London: Cambridge University Press, 1970).
4. Thomas Kuhn, "Logic of Discovery or Psychology of Research," ibid., pp. 4-5.
5. Thomas Kuhn, The Structure of Scientific Revolutions (Chicago: University of Chicago Press, 1962), p. 166.
6. Ibid., p. 37.
7. Thomas Kuhn, "Reflections on My Critics," Lakatos and Musgrave, eds., op. cit., pp. 266-77. See also Kuhn's postscript added with the 1970 printing of his book, pp. 174ff.
8. In Lakatos and Musgrave, eds., op. cit.
9. See Jerome Stephens, "The Kuhnian Paradigm and Political Inquiry: An Appraisal," American Journal of Political Science, Vol. XVII, No. 3 (August, 1973), pp. 471-72.
10. See Sheldon Wolin, "Paradigms and Political Theories," in B.C. Parekh and P. King, eds., Politics and Experience (Cambridge: Cambridge University Press, 1968), p. 136.
11. Kuhn compares astronomy and astrology in the medieval and Renaissance period to illustrate this point. See "Logic of Discovery or Psychology of Research," Lakatos and Musgrave, eds., op. cit., pp. 8-11.
12. Robert K. Merton, Social Theory and Social Structure (New York: The Free Press, 1968), p. 607.
13. Bernard Barber, Science and the Social Order (New York: The Free Press, 1962), pp. 122-35.
14. Warren O. Hagstrom, The Scientific Community (New York: Basic Books, 1965).
15. Norman W. Storer, The Social System of Science (New York: Holt,

Rinehart and Winston, 1966).

16. Ibid., p. 76.

17. Ibid., p. 89.

18. Ibid., p. 20. (Emphasis included.)

19. Ibid., p. 76.

20. Ibid., p. 41.

21. Ibid., p. 89.

22. John M. Ziman, Public Knowledge (Cambridge: Cambridge University Press, 1966).

23. Ibid., pp. 8-9. (Emphasis included.)

24. Ziman fails to note the other side of the referee system. The anonymity of the referee contravenes the principle of communality or, in Ziman's terms, the very public nature of science. Furthermore, as one critic put it, when considering the power of the referee to delay publication and perhaps pursue the work himself, even the disinterestedness of the referee is open to question: "Why should the wish to publish a scientific paper expose one to an assassin more completely protected than members of the infamous society, the Mafia?" Douglas Wright of Melbourne University quoted Bernard Dixon, What is Science For? (London: Collins, 1973), p. 49.

25. See Marlan Elisset, Politics in Science (Boston: Little, Brown, 1974), pp. 85-88.

26. Merton's papers on science have been recently published in one volume: R.K. Merton, The Sociology of Science (Chicago: University of Chicago Press, 1973); see especially pp. 281-415.

27. Thomas Kuhn, The Structure of Scientific Revolutions, op. cit., p. 138.

28. There has been a great deal of work treating this problem. See for example, C. Wright Mills, The Sociological Imagination (New York: Oxford, 1959); Maurice Stein and Arthur Vidich, Sociology on Trial (Englewood Cliffs, N.J.: Prentice-Hall, 1963); and Alvin Gouldner, The Coming Crisis in Western Sociology (New York: Basic Books, 1970).

29. See Karl Deutsch, Politics and Government (Boston: Houghton-Mifflin 1970), p. 117.

30. A similar argument could thus be made against the common analysis of the economic sub-system and the prevailing norms. See Robert L. Heilbroner, The Future as History (New York: Harper Torch Books, 1968). See also John Kenneth Galbraith, The New Industrial State (Boston: Houghton-Mifflin, 1967).

31. Stuart S. Blume, Toward a Political Sociology of Science (New York: Free Press, 1974), p. 279.

32. Roger G. Krohn, The Social Shaping of Science (Westport, Conn.: Greenwood Publishing, 1971).

33. Ibid., p. 209.

34. See above, Chapter One.

35. Krohn, op. cit., p. 211.

36. As scientist-philosopher Thomas Kuhn pointed out: "Typically, the undergraduate and graduate student of chemistry, physics, astronomy, geology, or biology acquires the substance of his field from books written especially for students. Until he is ready, or very nearly ready, to commence work on his own dissertation, he is neither asked to attempt trial research projects nor exposed to the immediate products of research done by others--to, that is, the professional communications that scientists write for each other. There are no collections of 'Readings' in the natural sciences. Nor is the science student encouraged to read the historical classics of his field--works in which he might discover other ways of regarding the problems discussed in his text....

"In contrast, the various texts that the student does encounter display different subject matters, rather than, as in many of the social sciences, exemplifying different approaches to a single problem field. Except in their occasional introductions, science textbooks do not describe the sorts of problems that the professional may be asked to solve and the variety of techniques available for their solution." Quoted in Hagstrom, op. cit., p. 10.

37. See for instance, "NIH Training Grants Going, Going, Gone," Science, Vol. 129 (January 16, 1973), pp. 356-58. "Engineering Unemployment," Science for the People, Vol. 2 (March, 1973), pp. 42-44 and D. Shapley, "Route 128, Jobless in a Dilemma," Science, Vol. 172, No. 1116 (1971).

38. Scientists were organized in the last four presidential elections, the best known example being the "Scientists and Engineers for Johnson-Humphrey" in 1974. See Daniel S. Greenberg, The Politics of Pure Science (New York: New American Library, 1968).

39. John Walsh, "National Academy of Sciences; Awkward Moments at the Meeting," Science, Vol. 172, No. 3938 (May 7, 1971), pp. 539-42.

40. On this point see Dixon, op. cit., pp. 20-35.

41. In an only partially satirical sense, the lab coat has been pointed to as having outlived its necessity for use but been retained for other reasons. "The Modern scientist rarely works in his lab coat. When work is unavoidable, he will be found in his shirtsleeves, in a coarse brown smock, or in plastic. His lab coat, clean pressed, possibly even starched, hangs safely behind the door, to be worn only when he is lecturing or greeting official visitors. Like spurs

and shakos, the lab coat has been promoted to a new role; it is rapidly becoming not merely the uniform, but indeed the dress uniform of the scientists. Dress uniforms are worn solely for symbolic and ceremonial purposes." F.E. Warburton, "The Lab Coat as a Status Symbol," Robert A. Baker, ed., A Stress Analysis of a Strapless Evening Gown (Garden City, N.Y.: Anchor, 1963), pp. 175-76.

42. See Diana Crane, "Scientists at Major and Minor Universities: A Study of Productivity and Recognition," American Sociological Review, 30 (1965), pp. 699-714.

43. See Greenberg, op. cit.; see also Herbert J. Muller, The Children of Frankenstein (Bloomington, Ind.: Indiana University Press, 1970).

44. Blume, op. cit., pp. 131-45.

45. Dean Schooler, Jr., Science, Scientists, and Public Policy (New York: Free Press, 1971), pp. 26-27.

46. Ibid., p. 62. Schooler develops concepts introduced by several others foremost among them being Theodore Lowi. See T. Lowi, "American Business and Public Policy," World Politics, Vol. XVI, No. 4 (July, 1964), pp. 677-715.

47. Schooler, op. cit., p. 241.

48. Ibid., p. 213. (Emphasis included.)

49. Ibid., pp. 208-09.

50. "There is a certain tendency within the scientific community to assume that all that is necessary is an expansion of the scientific subculture into more and more areas of life." Kenneth Boulding, "The Scientific Revelation," Bulletin of the Atomic Scientists, Vol. 26 (September, 1970), p. 17.

51. Arthur L. Stinchcombe, Constructing Social Theories (New York: Harcourt, Brace & World, 1968), pp. 31, 32.

52. R.M. MacIver, "Causation in Social Research," L.D. Hayes and R.D. Medlund, eds., The Conduct of Political Inquiry (Englewood Cliffs, N.J.: Prentice-Hall, 1970), p. 92.

53. Blume, op. cit., p. 179.

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CHAPTER FOUR

Scientific Authority

In considering the concept of scientific authority, it will be argued that the language of authority best conveys a key element in the link between contemporary science and society, and that this link has particular significance with regard to public policy making. This entails, first, an examination and comparison of the three primary concepts used by political scientists to denote a relationship in which one actor brings about a specific behavioral change on the part of another--power, influence and authority. Second, it involves considering how well each of these concepts represents this relationship when one of the actors is: a single scientist, several scientists, or science itself. A case study will be utilized to illustrate that scientific authority encompasses a relational dimension which cannot be usefully comprehended through recourse to the concepts of power and influence. This consideration, in turn, leads to an argument that the concept of scientific authority as used here requires an elaboration of the general notion of authority to a point beyond its common conceptualization in the social science literature.

To begin, one may return to consideration of Schooler's hypothesis of a positive correlation between scientific influence and the availability of technological fixing. An important contextual element missing from Schooler's analysis, we suggested, might help to

provide some theoretical insight into the subject under present analysis. This can be shown best perhaps through reference to his analogy of the scientist with the physician. He noted that both doctor and scientist are more likely to be successful in influencing the behavior of others when "available means to ... objectives, are material or 'technological' rather than behavioral."¹ With regard to the physician, he reminds us that the prescribed cure is readily accepted when offered in the form of a pill, but more often rejected if it requires exercise, the cessation of smoking, etc. But is this all? Let us consider the contextual aspects of the doctor-patient relationship which are cumulatively revealed in what is called the placebo effect.

We are referring to the well documented fact² that physicians (as well as biomedical scientists) have reported many cases where the medical results of sugar pills equalled those of prescribed drugs for treating certain ailments. This tendency has been so well confirmed that studies on the effects of drugs are expected to include a control group of subjects receiving placebos. The reasoning is that the cure sometimes lies less in the objective value of this particular technological solution, and more in the subjective determination on the part of the patient that this is the required solution. The patient believes in the physician-drug cure and not simply because it is the easiest or most technologically advanced solution to his problem; there are many well documented cases of faith healers and the like who have performed cures which required arduous exertion, even suffering, and involved either very primitive technology or had no technological basis at all.³ Clearly then the patient's faith in the healing powers of scientific medicine, or black magic, or whatever,

is of unquestionable importance to the process of healing.

The placebo effect, as understood by psychologists, is evoked by anything other than the direct effect of the active chemical ingredient in the drug, including all the trappings which may go with its prescription and administration. This involves the setting, such as the hospital or clinic, the presence of the physician--even his "bedside manner"--the regularity of the dosage, etc. And, of course, it involves as well the more general quality of the patient's faith in the value and legitimacy of the knowledge assumed responsible for developing the drug, i.e., scientific medicine. When analyzing the role and effect of the scientist, as when analyzing that of the physician, the placebo factor, it is suggested, should not be ignored.

Schooler, however, made no effort to analyze this aspect of the scientist-physician analogy. Hence his conclusion from it is superficial. The policy maker's decision to allow scientists a decisive role in treating a particular problem, while apparently based on the objective fact that science can provide a technological fix, may in fact turn out to be based on his subjective acceptance of the problem situation as potentially responsive to scientific or technological fixing. The process is thus a dialectical one. Whether or not a problem responds to technological fixing is not dependent only upon the objective factors associated with it; but also upon the propensity of those involved to accept it as a technological problem at all--consequently allowing it to be defined in scientific terms. The actual outcome of the intervention depends in part on this generalized subjective factor or context in a manner analogous to the placebo effect in medical practice. The potential effectiveness of scientists, like that of physicians, should be

conceived broadly enough to incorporate this subjective and yet sometimes crucial placebo effect.

Another associated difficulty is that Schooler's definition of influence seems to be inadequate, when he attempts to assess the particular influence of scientists. For him influence is defined as "the change in policy or the probability of a given policy induced by the presence of the scientist and his behavior in the policy process."⁴ The vagueness of this definition, we maintain, allows its user to conduct an empirical study of behavior at once too broad and too narrow. To illustrate this requires a short foray into political linguistics. Let us imagine a scientist (X) making terse statements, of a sort which J.L. Austin would call "speech acts,"⁵ as follows:

- 1) "If you do not in a week begin slum clearance in the core of Mytown, I shall do it for you with a high explosive of my own invention."
- 2) "If you do not prohibit the sale of cigarettes, one million Americans will unnecessarily die of cancer in the next nine years."
- 3) "Don't touch that test tube!"

Adapting a perspective suggested by David V.J. Bell,⁶ to be elaborated in greater detail below, we might, for the moment, label the above as statements manifesting relationships of (1) power, (2) influence, and (3) authority. If X is believed to be intent upon and capable of performing the promised act, the first is a threat which functions as an "order." The second bears no such imperative mood; it merely describes or predicts a contingent outcome. However convincing, the prediction's fulfillment lies beyond the power of

its maker. Such statements probably influenced Congress to pass legislation requiring (exercising power over) cigarette makers to print warnings on their packages--warnings which seem to have had little influence upon smokers. The third again is an order. However, it is presumed to be given in a context where X's role and relationship to the listener make such a command legitimate and virtually unquestionable.

The statements were posed in hopefully unambiguous terms for illustrative purposes, but it is often very difficult to make practical distinctions between these three types of statements. There may be a fine line between a warning and a threat, for example, and where it is drawn may depend more on how the listener decodes the message than on the content of the message. Still the distinction is useful in calling attention to a trap which is especially difficult to see by those who have fallen in.

Schooler's definition of influence as intended change brought about by someone's presence and behavior would encompass, it seems, all three types of speech acts. If so, in Ball's terms, Schooler was studying power and authority as well as influence. His inability to distinguish power from the other forms does not appear to cause serious problems, for power exercised directly by scientists as such cannot be extensive. They have as a rule neither the independent resources nor the ruthlessness on which to base serious promises or threats concerning their own behavior--the Dr. Strangeloves still are largely characters of fiction. However, the failure to consider what might be termed scientific authority separately from influence is another matter. This kind of relationship, it appears, cannot be easily dismissed nor can it usefully be subsumed under a very wide

definition of influence. This failure affected what we consider Schooler's central conclusion.

To conclude that scientific influence is greater where technological solutions are available is trivial when influence is defined narrowly as expressed in predictive statements, and unsubstantiated when influence is defined widely, as Schooler chooses to do, inclusive of relationships characterized by statements of command. Consider the role of a municipal health officer in successfully bringing about the flouridation of his town's water supply. Either the decision is implemented through the normal channels of the municipal health department; or it is questioned either by government officials or members of the public, and implemented only when he persuades those with decisive power that flouridation will benefit the (children of the) town. We shall return briefly to flouridation as a case study below; for now we raise it as an example of how authority operates in contrast to influence even in, to use Schooler's term--the same policy arena.

The key statements of the health officer in the first case would be in the form of commands: "Place x milligrams of flouride into the water supply in the following manner...." In the second case they would take the form of predictions: "If the water is not flouridated, the children in this community will have an average of 1.3 unnecessary cavities per year...." In the first case compliance is based on decoding the message as within the established role of the health officer; in the second it is based on the merits of the content of the statement. If influence is defined to include both these relationships, then we have no basis for making what we shall argue to be a crucial distinction.

One should add, in conclusion to this discussion, that Schooler's generalization with regard to technological fixing is largely irrelevant to relations termed scientific authority since he assumes compliance to be based on the content of the predictive element of the communication, namely the anticipated (technological or behavioral) results of compliance, while we have seen that authority communications tend to take the form of simple commands without predictive implications. Analysis of likelihood of compliance must thus be undertaken in an entirely different manner, focusing not only on the content but also on the contextual factors leading to its being decoded as authoritative--including the placebo effect described above.

At this point we must go beyond Schooler's conceptualizations, and seek greater clarity by turning directly to the key concepts in question. This means a focus on the concepts of power, influence and authority, beginning with their general usage in political science, next returning to the communications perspective suggested by Bell and assessing its contribution, and only then returning to the particular case of science.

Perhaps the most common approach, especially among political scientists, is to treat influence as "a generic term including authority, control, and power."⁷ Edward Banfield, quoted here, may be likened to Robert Dahl, who defined power as the "domain of coercive influence,"⁸ and influence widely as "a shift in the probability of an outcome" in the action or actions of A caused by the action(s) of B.⁹ (Dahl does usefully note that the "manipulation of substantial rewards can also be considered coercive;" thus accepting the idea that both "negative" and "positive" coercion should be included in the concept of power.)¹⁰ Similarly David Easton states

that: "A person stands in a relationship of influence to another when his behavior modifies the acts of the other in some way."¹¹

Power, he maintains, is "intended" and "effective" influence.¹²

A more useful recent definition similar to that of Dahl treats power as "the ability and willingness of one person to sanction another by manipulating rewards and punishments which are important to the other."¹³

Another approach is to treat power as the generic term and thus to view influence and authority as subcategories within it. Gerhard Lenski, for example, defines both authority and influence as "institutionalized" forms of power:

Authority is the enforceable right to command others. Influence, by contrast, is much more subtle. It is the ability to manipulate the social situation of others, or their perception of it, by the exercise of one's resources and rights, thereby increasing the pressures on others to act in accordance with one's own wishes.¹⁴

Lenski is here following in the tradition of Max Weber, who defined power as "the probability that one actor within a social relationship will be able to carry out his own will despite resistance, regardless of the basis on which this probability rests."¹⁵ Our own logic toward this path is explained later in the discussion.

These scholars differ mainly in their selection of either influence or power as the generic term. Dahl, in fact, vacillates on this, for at one point he appears to treat power as the generic term as he states (following Weber) that, "A has power over B to the extent that he can get B to do something he would not otherwise do."¹⁶ None, for good cause, view authority as a possible generic term. "Most definitions of authority in the ... social sciences ... view authority as a special case of power or influence."¹⁷ To sum

up the conventional discussion of power and influence to this point, we may say that it consists of three elements:

- A) A relationship where compliance¹⁸ on the part of one is brought about by the potential use of sanctions or rewards on the part of the other--often termed power.
- B) A relationship where compliance on the part of one is brought about without reference to sanctions--often described as influence.
- C) The genus of the relationships of which A and B comprise the species which is sometimes termed power sometimes termed influence depending usually on which term has already been used to denote either A or B singly.

The political linguistics approach, as developed recently by Bell provides us with a valuable way to distinguish these different relationships and thus gives added reason to be less concerned about the selection of a generic term and more concerned with the distinction between A) power and B) influence. Bell argues that politics is most usefully viewed as a form of communication and, hence, the best way to distinguish power, influence and authority is in the different kinds of statements associated with them. These three political relationships are manifested in three forms of communication and each communication involves a process of encoding, transmission and decoding. The distinction between influence and power is well articulated in these two excerpts.

It is to communications which involve either threats or promises that I wish to attach the name power.... The concept refers to a certain relationship, embodied in a communication, by which A presents B with an 'offer' to which is attached a contingency in the form of a reward (promise) or a penalty (threat).

Influence [is] ... a communication intended to affect the action of B by A in the absence of sanctions.... The influencer, rather than manipulating the contingencies of reinforcement, (as in power) is attempting to manipulate perceptions of these contingencies. He says, in effect, 'If you do X, Y will happen,' where Y is beyond his control.¹⁹

Because they lack a concept to describe the relationship manifested in the latter excerpt, it appears that thinkers such as Schooler and Banfield err when they use the term influence generically.

The concept of authority poses a thornier theoretical problem. Authority is widely regarded as a concept central to political science.²⁰ This is as it should be. However, it has with some merit been suggested that political scientists have not utilized it effectively.²¹ While it is not possible here to enter a comprehensive discussion of the literature of authority as it has developed in sociology, philosophy, and political science, a brief survey of some major points is required. Such a discussion, it is almost universally conceded, begins with the typology formulated by Max Weber. For Weber, authority is "the legitimate exercise of imperative control." In his well known classification:

There are three pure types of legitimate authority. The validity of their claims to legitimacy may be based on:

1. Rational grounds--resting on a belief in the 'legality' of patterns of normative rules and the right of those elevated to authority under such rules to issue commands (legal authority).
2. Traditional grounds--resting on an established belief in the sanctity of immemorial traditions and the legitimacy of the status of those exercising authority under them (traditional authority); or finally,
3. Charismatic grounds--resting on devotion to the specific and exceptional sanctity, heroism or exemplary character of an individual person and of the normative patterns or order revealed or ordained by him (charismatic authority).²²

In another famous passage, Weber lists four bases of legitimate order:

Legitimacy may be ascribed to an order by those acting subject to it in the following ways:

(a) By tradition; a belief in the legitimacy of what has always existed; (b) by virtue of affectual attitudes, especially emotional, legitimizing the validity of what is newly revealed or a model to imitate; (c) by virtue of a rational belief in its absolute value, thus lending it the validity of an absolute and final commitment; (d) because it has been established in a manner which is recognized to be legal.²³

Basing his argument on the above passage, Martin Spencer argues that along with traditional, charismatic and legal-rational authority, Weber should have added a fourth type which Spencer calls "value-rational," and which is "typically found in the higher realms of political office ... and exists in relation to principles. Thus if legal-rational authority is celebrated as an administration of laws, not of men, value-rational authority is a government of principles, not of men."²⁴

Among students of political science, the concept of authority has been applied and developed foremost in organization theory.

Chester Barnard, in a pioneering work states:

Authority is the character of a communication (order) in a formal organization by virtue of which it is accepted by a contributor to or 'member' of the organization as governing the action he contributes; that is, as governing or determining what he does or is not to do so far as the organization is concerned....

A person can and will accept a communication as authoritative only when four conditions simultaneously obtain: (a) he can and does understand the communication; (b) at the time of his decision he believes that it is not inconsistent with the purpose of the organization; (c) at the time of his decision, he believes it to be compatible with his personal interest as a whole; and (d) he is able mentally and physically to comply with it.²⁵

Herbert Simon defines authority simply as "the power to make

decisions which guide the actions of another."²⁶ Authority presupposes a relationship, though only in relation to particular actions, between a superior and a subordinate. Among others, Lenski, as we noted, defined authority as one form of institutionalized power. Authority has also been defined by social scientists as "legitimate influence," "legitimate power," "formal power," and "socially sanctioned power."²⁷

Let us briefly look at two other usages of this concept by contemporary students of politics. Carl Friedrich views authority generally as based on the capacity for explanation on the basis of traditionally accepted values: "Authority rests upon the ability to issue communications which are capable of reasoned elaboration."²⁸ Dahl identifies three beliefs as the basis on which an individual accepts authority; one, that the decisions made correspond to his personal choice; two, that they are arrived at through the exercise of special competence; and three, that the decision allows the individual an economy of time and effort.²⁹

While this series of excerpts is abbreviated, it is not unrepresentative. Although the concept has not been systematically developed in social science, the points listed above do provide a useful starting point. First, authority describes a kind of relationship between individuals and/or groups we may conveniently label "A" (agent) and "S" (subject). Second, the relationship is one requiring communication between the two. (Note that communication can be implicit; S can know what A wants of him in other ways than being directly told.) Third, the relationship is one that normally presupposes (within certain limits or within a certain context) an inequality between S and A. (Note that this is not the case with respect to influence; influence can describe a relationship between equals or

even that of a subordinate to a superior; for example a secretary can influence her boss as to how to carry out his work.) Fourth, such a relationship may include elements of power or be communicated in the form of logical persuasion but it must go beyond both. In other words, the possibility of enforcement or of reasoned explanation may lie at the back of an authoritative communication but if either possibility is put into play then authority as such disappears. When the "capacity for reasoned explanation" is presumed rather than demonstrated, authority instead of influence is at play. If the boss were continually forced to voice his orders to his employees as threats or to promise rewards upon their being obeyed, then power is being exercised but not authority.

Furthermore, Dahl seems to misuse the concept of authority when he suggests that personal choice and economy can be the bases for acceptance of authoritative communication. They are among the motivations for action in a social setting, but in themselves have little to do with authority. A choice of action made on the basis of economy could just as well indicate the absence of authority. In an interpersonal setting the kind of response S would give to demonstrate that economy was the deciding factor might go something like; "All right I'll do as you say; just stop pestering me."

In addition, we should emphasize an element that best emerges from Barnard's formulation, namely, his stress on membership. For a relationship of authority to exist between A and B they must be "members" in some kind of institution. Membership here need not imply formalistic (membership card) relations but rather a shared cultural context which provides an underlying structure to their relationship. Basic to this context is the ability to communicate

meaningfully, which presupposes a shared language so that on-going common activities can be jointly understood. In Friedrich's rather restricted terms, such a relationship requires shared values based on traditions. For Weber, of course, traditional authority is only one of several different types.

Summing up briefly, we may take the above statements as one perspective of the state of the discipline with respect to the concept of authority, save for the contributions of the political linguistics approach.

Bell postulates that authority relationships are manifest in statements that take the form of simple commands without reference to threats, warnings, or promises:

Power communications and influence communications ... present the individual with a choice to be made.... Authority ... [denies] the very ... necessity of choice.... It presents for immediate adoption an already formulated 'plan for action....' To accept authority ... is to place one's own decision making process ... in the hands of someone else.³⁰

In other words the decision to obey authority unlike the decision to heed influence or comply with power has been made prior to the receipt of the communication itself. In decoding the message, the subject in an authority relationship (unlike in the case of influence or even power communications) is not interested in assessing the content of the statement but decodes it in terms of the context (that is, the prior acceptance of the relationship).

From the previous discussion, it should be clear why the concept of authority has been applied usefully in social science almost exclusively in the study of formal organizations. Aside from Friedrich's more restricted treatment and Dahl's less than rigorous one, no real attempts have been made to apply this term to contemporary

social and political reality outside these rather narrow bounds. If membership, subordination, etc., are taken to be the identifiable indicators of authority, this inevitably limits the application of the concept to formal legitimate "organizations." But this is insufficient. The political linguistics perspective is instructive in distinguishing authority communications from others and in drawing our attention to the importance of the decoding process. The decoding which takes place is essentially a judgment as to the suitability of the form of the communication. Focussing on the decoding process by which the subject interprets the overall context (which is not necessarily institutional) cuts the implicit tie between authority and formal organizations. The context is important essentially because of the manner in which it affects the decoding process. In this way it may be viewed in as wide or narrow a sense as is relevant to any particular case. For Talcott Parsons, the context is viewed in the widest terms: "Authority ... is the institutionalization of given modes and levels of integration of the collectivity insofar as these are essential conditions of effective and legitimized collective action. It is the institutionalization of the rights of 'leaders' to expect support from the members of the collectivity."³¹ Robert Bierstadt stresses limits but retains a broad view of possible contexts (in so doing, and we tend to agree, excluding Weber's conception of charismatic authority);

Authority is always a property of social organization.... The exercise of authority furthermore never extends beyond the limits of the association in which it is institutionalized and which gives it support and sanction.... A policeman may not over-rule an umpire on a close play at second base.... In addition, authority is always attached to statuses and never to

individuals. Furthermore, the exercise of authority is a function of norms that are themselves as impersonal as a traffic light.... The person who exercises it is recognized as an agent of the group. He represents the group. He acts not in his own but in the group's name.³²

Generally, in the words of philosopher Peter Winch: "The acceptance of authority is conceptually inseparable from participation in rule-governed activities."³³ Are A and ~~B~~ engaged in some kind of joint activity governed by certain rules, standards and/or norms, of which A is acknowledged to be the arbiter? This remains the key question. Yet the answer is not always a simple one, once we leave formal organizational settings and especially when the kind of knowledge required to understand the rules, standards and/or norms that govern the activity is of the esoteric nature of contemporary science--a consideration we shall explore below.

One final point. It should now be clear why a precise conceptualization of these terms is so important. For example, the vagueness noted above in Schooler's usage of the concept of influence will not help but cause confusion. Specifically, in our society authority tends to be used commonly as a synonym for expert advice or expertise as in the statement: "He is an authority on the subject." In its pure form, a relationship manifested in a communication based on expertise is one of influence rather than authority. Because of this, a failure to define terms rigorously tends to lead one simply to subsume authority under influence and thus eliminate it from separate consideration. We maintain that while it may not yet appear readily apparent, the subsuming of scientific authority under influence is a serious obstacle to furthering analysis, and that only a clear and distinct conceptualization of scientific authority per se will do.

Scientific Authority

The task is now to move from a general discussion of authority to a more specific consideration of scientific authority. (To facilitate this we shall adopt the generic term "Power" to denote behavior changing relationships including what we define to be power, influence and authority. The reason for our selection of power as being, among the three concepts under consideration, apparently least suitable for our specific purposes, was noted above and will be elaborated below.) We have defined expertise based Power as a form of influence. Our definition of authority might be stated thus, following both Ball and Bierstadt:

Authority is a relationship in which communications are transmitted in the form of commands by individual or group A to individual or group S and decoded as legitimate (and normally obeyed) by S by virtue of an attribute or attributes possessed by or ascribed to A in relation to an institution in which both A and S may be seen as members. The institution may be formal but it may also be informal in structure and operation. The authority thus established exists within the context of this institution.

We certainly make no claim to be the first to use the concept of scientific authority. Indeed, scientists themselves are often aware of and make reference to what is meant by the authority of science. For instance a leading American physicist, Polycarpe Kusch, told an audience in 1955: "Social, political and military decisions are made on grounds other than those on which science is authoritative.... I think a more cautious use of the phrase 'science says' ..., a more temperate view of the authority of science is necessary."³⁴ (Note that Kusch does not reject scientific authority;

the use of "science says" is not challenged, rather caution is advised.) Harold L. Nieberg, one of the most outspoken critics of the science-military complex referred to by Eisenhower in his Farewell Address, relies on the same concept as he attacks deeds and developments justified "in the name of science:"

The word 'science' which in its root sense means simply 'to know' has acquired in our civilization a highly authoritative quality whose unmistakable halo effect is borrowed by many different self-serving groups ... [due to] its popular association with all things that are good, true and beneficial. In short, 'science' has become a dangerous word, frequently used to invoke authority....

The average man views science very much as a believer does a revealed faith. For him, subatomic particles are part of an invisible world whose existence he accepts on authority. The mathematical formulas of the physicist have become a species of sacred text. The initiated ones, the scientists, are thought to penetrate these mysteries and can awe the laity by the miracles they perform in modern agriculture, transportation, medicine, and weaponry.³⁵

Or, as de Jouvenel put it, "How do I know that there can be no speed greater than that of light, or that the proton has a mass of 1,836 times that of the electron, or indeed that such things have any existence.... Science says so, and I accept these beliefs on its authority with no more thought of checking them than if I were a medieval peasant."³⁶

What the three writers quoted above mean exactly by the "authority of science" is not spelled out. All they tell us is that the authority of science comes into play when science, or Science, or the principles of science, are used to justify or rationalize some belief or action. Let us, therefore, attempt our own definition, being careful to incorporate the distinctive character of authority.

A relationship of scientific authority is one where

communications are transmitted by scientist(s) A to (usually) non-scientist(s) S in the form of (explicit or implicit) commands and decoded as legitimate (and normally obeyed) by S by virtue of the fact that the relationship takes place in an institutional setting where science is acknowledged as the source of the right or normal way of carrying out the activities implied in the content of the communication. The term scientist here is taken to mean the acknowledged representative of science. One would expect the extent to which A needs to be formally certified by the scientific community to vary directly with the societal importance (which may be assessed in terms of the rewards in money, social deference or security allocated to the participants) of the setting.

This definition is arrived at by placing the words scientist and science at the appropriate spots in the definition of authority derived above. As such it does not take us very far in our search for an analytical framework which may be applied to the general questions we have posed. We may say at this point that the sum total of scientific Power and its observable effect is the total of the sum of societal activity guided by the acceptance of scientific expertise (influence) plus (and this is a separate consideration) the total of societal activity carried out in obedience to orders given by scientists by virtue of their holding positions of "authority" in various organizations. While an improvement on Schooler, this interpretation is still

insufficient for it does not take into account the significant and distinctive characteristics of science, its social nature and its wider cultural impact. Perhaps such a statement might accurately sum up the power (generic) of another knowledge based group, for example, pilots or bankers, or perhaps that of scientists in the U.S. one hundred years ago. We maintain that in the contemporary period such an assessment is gravely circumscribed. What must be investigated is the nature of the decoding process as it applies to the institutional relationship context which results from the type of knowledge which governs it, the manner in which communication takes place, the extent and form in which it is embedded in the general culture, and other aspects of what we have called by analogy the placebo effect.

How does scientific authority compare with other forms (sources) of authority? We see little point in attempting to fit scientific authority into a classification of all different possible sources of authority. Such sources are many, in theory and in fact. The number of different sources of authority, we may say, reflects the number of different types of belief which have been institutionalized. Analysts may arrive at different assessments as to the number and types of different sources of authority, but there can be little doubt that scientific authority should be included as one of these. Furthermore, it seems uniquely distinctive. (However, in a complex case this must be difficult to demonstrate; the following chapter will serve to caution students on this score.)

The types of belief which underlie the rules governing the subject's relation with the agent allows us to distinguish different sources of authority. Anticipating our investigations below, we raise here a crucial aspect of scientific authority--that of limits.

And here scientific authority appears different from the case of political leaders, policemen, priests, generals, bosses, shop-stewards, wardens, umpires, etc., where, except in cases of military or state secrecy, the general knowledge at the base of the relationship which determines its institutional context, is roughly shared by S and A. In decoding commands from A, S can determine whether A is operating within the limits of the institutional context of the relationship. The baseball player knows to resist when ruled "strike two and you're out" by the umpire, but the subject has no analogous knowledge to challenge a command from a scientist.

In relation to other sources of authority, the would-be subjects usually have discretion in deciding which activities legitimately qualify as within the limits of the institution putatively governing it. The ignorance of the layman in relation to science coupled with the widespread societal legitimacy accorded to science, it would appear, result in his virtual inability to make judgments concerning its boundaries and therefore to gain a significant degree of control over his relationship to authority exercised in its name.

This does not mean that scientific authority cannot be challenged, nor does it mean that non-scientists cannot appropriate the authority of science for purposes of their own, such as economic or political gain, "in the name of science." The exact implications of these characteristics must be carefully spelled out. We shall attempt to do so by analogy and then with reference to a "laboratory" case study. First, let us hypothesize two "ideal types" of religious authority. (The analogy between science today and religion in the past as already seen in our brief quotations from Nelberg and de Jouvenal above, and

by its continued usage by no less an observer than Don K. Price (see Chapter One) remains quite popular.)

In the first case, let us imagine a society without any established religion. Those who are believers join together in organizations called churches designating certain individuals as ministers authorized to make decisions affecting members' activities relating to the organization on the basis of the beliefs of the church. In such a case, religious authority may be communicated as a command from the minister to members of the congregation within the mutually and clearly understood context of the normal operation of the church. A typical communication might go: "The Congregation shall rise and read psalm 95." In addition, one may expect cases of influence communications transmitted from "authorities" on the church's teachings to other members which take the form "If you (don't) do this ..., God will...."

In the second case let us imagine a society where a particular church doctrine is firmly established in the social structure and culture of the society. Let us imagine, further, that the church's beliefs, while they may have been originally known to all members, have over the years become enshrouded in what appears to be dogma and expressed in an obscure terminology, which none but the annointed are expected to have mastered. In such a case, the typical subject needs only to be made aware of the appropriate trappings (symbols to be admired) of religious authority, "miranda" as Bell, following Charles Merriam, describes them,³⁷ to decode practically any command type statement on the part of, or attributed to, the minister as authoritative. The church itself, then, effectively sets the limits of its own authority.

Before comparing the difference in the form of authority in these two examples, a brief digression on power is appropriate. We have so far paid little attention to power as a specific kind of relationship, allowing it instead to serve as a convenient generic term. Let us now look at specific power relationships which link science and scientists to society.

In our hypothetical religious situations, power relationships imply the existence of sanctions which the church is able and willing to manipulate. In the first case power serves potentially as a buttress for authority manifest in communications which in effect state: "If you do (not) ... then you will be unable to sing in the choir, (or) have to leave the hall, (or) be expelled from the congregation, (or) be excommunicated." Essentially, once its authority is challenged the church can resort to power only in reminding the recalcitrant that by challenging its authority he or she is putting his own membership within the church in question. In such a case, obviously little is added by considering relationships of power beyond those of religious authority, since the sanction ultimately amounts to "if you don't do X, then you won't be able to do Y." In the second case, the power potential also consists of the ability of the church to withdraw some or all of the rights of membership. But here the implications of the use of these sanctions are much wider, for they carry with them possible social ostracism, denial of access to education, charity, marriage, etc. Although formally the same, then, in each case, the power available through the (threat of) denial of membership is different from that in terms of the rewards that membership confers.

If we compare scientific authority with religious authority as depicted in these cases, certain points stand out. With regard

to power the analogy of contemporary science to the medieval Church of Rome does not stand up. The position of science may be compared to that of the church in the first case, namely that its resources of power in the ability to (threaten to) deny membership extend only within the scientific community. We exclude the cases of the "mad scientists" threatening to apply or withdraw knowledge with aversive results on society unless obeyed since they are (as yet) confined as far as we know to science fiction and the horror cinema. The symbolic "science strike" of the March 4th movement and current rumblings of unionization of scientists raise the possibility of scientific power through threatened denial of services but this too is at present more fanciful speculation than empirical assessment. However, as far as authority is concerned, the second case provides a far broader, "external" analogy to contemporary scientific authority.

Science in the U.S. today exhibits the two significant characteristics of the latter ideal type, its esoteric content and the societal legitimacy accorded to it significantly, more than any other source of authority we can name. From this perspective, the analogy is thus not misplaced, and it forces us to reconsider our whole definition of authority--bringing in elements which might have been more apparent to the medieval theologian than the modern student of organization. The emphasis upon institutions in our definition may be appropriate to the authority of science as expressed within formal institutions as analogous to the first example but inappropriate to the wider authority science may exercise as analogous to the second example. So far, we have attempted to make this point essentially indirectly, rather than directly by providing illustrations of scientific authority at work. The bulk of the remainder of this chapter is devoted to a

description and discussion of an important concrete example of a particular relationship illustrating the nature of dyadic $\sqrt{\text{scientist(s)}}$ A to non-scientist(s) $\sqrt{\text{scientific authority}}$.

The "Electric Chair Experiments"

Finding clear-cut examples of scientific authority in contemporary American society is difficult since scientists normally operate within the confines of large and complex institutions and come into contact with non-scientists infrequently in such capacity. We cannot point to a "corner scientist," or "family scientist," or "Ph.D. on the beat." Looking around at normal social behavior we can think of only a few common relationships which might qualify under this heading:

- 1) The doctor/patient relationship between physicians (and also, perhaps practitioners of related professions such as psychiatrists, psychologists, pharmacists, nutritionists, etc.) and patients in the context of the work of the former.
- 2) The relationship between scientists as teachers and non-scientists as students.
- 3) The relationship between scientists as experimenters in psychological or medical research and non-scientists as subjects.

Of these three categories, the first two, while clearly related to scientific authority, cannot be assuredly interpreted as such per se. The medical professionals' and professors' exercise of authority clearly includes elements other than the purely scientific. This leaves the final example; the case of the non-scientist who as subject gives over his or her services to one or more people identified as scientists for the purposes of scientific experiment and research.

While not as common as the others described above, it is by no means rare, given the vast amount of scientific research related to humans which is being carried out today in the U.S. We have noted that the most common are drug and behavioral experiments carried out in psychology or related fields. In such instances, the non-scientist usually knows he is being used as a subject in a scientific experiment though he is frequently kept in the dark, sometimes even deceived, as to its true nature and goals. The scientist-experimenter sets the goals, operational mechanisms and rules; the subject accepts them to the extent that he is aware of them and acts accordingly.

The above is true generally of scientific experiments on human beings and particularly of laboratory experiments in the field of experimental human (or social) psychology. There have been several thousands such laboratory experiments which could serve as examples, as the authority of science may be postulated to be found in all of them in the very acquiescence by the subject to participate in a scientific experiment. Rather than having to investigate these directly ourselves, we are fortunate that among these there has been a small number carried on with the express purpose of studying human behavior in the scientific experiments themselves and among them a few which used this setting to investigate obedience to authority. Of particular interest are those which, when examining authority, utilized the authority of the experimenter himself by studying the responses of non-scientist subjects. Such experiments, of course, require a great deal of deceit and manipulation because the subject must be kept in the dark as to their purpose. By far the most famous of these is a series which have now come to be known by cognoscenti as the "electric chair experiments."

Some years ago, Yale psychologist Stanley Milgram carried out a now well known set of experiments³⁸ using as his subjects 1000 males (and later a smaller number of women) living in the New-Haven-Bridgeport area of Connecticut. These individuals answered a newspaper advertisement and found themselves in a laboratory situation set up by Milgram. In the core experimental situation, each subject arrives as one of two volunteers responding to the ad. Unknown to him, the other "volunteer" is in fact a confederate of the experimenter. The experimenter explains that the goal is to study punishment and learning. A rigged drawing takes place and the subject is made the "teacher" in the experiment, while the confederate is the "learner." The "victim" is then strapped into a chair and attached to electrodes, and the subject is seated at a machine that (he is told) can send out amounts of electric current. In front of him are buttons registering in ascending order up to 450 volts. The higher amounts of voltage are signified as being dangerous by the sign "Extreme Shock, XXX" and the "teacher" is permitted to feel the rather noticeable sting of 45 volts.

The "test" begins and the learner soon misses many answers so the teacher is told to administer higher and higher levels of shock. The "learner" soon begins to complain, then to shout out; at 300 volts he refuses to continue and he screams in agony. Finally, there is silence at the highest levels. During all this time, the experimenter, a male biologist 30 years of age, wearing glasses and a lab coat, maintains an absolutely cool demeanor and whenever questioned by the "teacher" tells him that the experiment must go on.

Like other experts (psychiatrists and psychologists) he consulted prior to the experiment, Milgram had expected disobedience to occur frequently and relatively early in the experiment; the opposite occurred.

The incredibly high levels of obedience (almost two-thirds) right to the (bitter) end surprised and dismayed him. In an effort to determine why the obedience levels had been so high, Milgram varied the experiment in many ingenious ways. First, he gradually brought the victim closer to the subject in three stages. Under the original conditions, the subject did not see the victim at all. In the second, the subjects heard banging on the wall at given points. In the third, the victim was present in the room. In the fourth, the subject had to administer the shocks by bringing the victim's hand to the machine.

Not unexpectedly, there was a clear downward progression of the average highest shock the subjects in each of the four experimental situations were willing to administer; the men tested under the first set of conditions administered higher shocks than those under the second set of conditions, and so forth. While only thirty-eight percent of the subjects had defied the experimenter at some point under the original conditions, this went up to seventy percent in situation four.

A second variation had to do with the proximity of the experimenter. In one situation the experimenter sat right next to the subject; in the second, after giving initial instructions, he left the room and further communications were on tape. Here the levels of obedience declined more drastically, with the disobedience as high as seventy-eight percent. Interestingly, much of this disobedience took the form of deceit; frequently the experimenter was assured on the telephone that higher levels of shock were being administered when in fact the teacher remained at the lowest levels throughout.

Other of Milgram's many variations should be mentioned at this point. He moved out of the plush laboratory to a rather austere

one--no change. Then, suspecting the authority of Yale University was somehow bound up with the authority being obeyed in the experiment, Milgram sought to sever this connection. He moved his operation out of New Haven altogether, into a commercial building in Bridgeport, and he identified the group in charge as the "Research Associates of Bridgeport." Identical tests were run as in New Haven and the results were not too different. There was some drop in the obedience rate, but under original conditions the average of those who never disobeyed only went down from 62 to 48 percent.

Milgram reacted to his results with justifiable consternation. Upwards of half of the male population of a part of the U.S., it would appear, will torture a complete stranger simply on the say-so of another individual. This holds true as well in Munich, Rome, and in Australia, judging by the results of replications conducted there; in fact the level of obedience was often even higher.³⁹ Milgram concludes:

The results ... are disturbing. They raise the possibility that human nature, or more specifically the kind of character produced in American democratic society, cannot be counted on to insulate its citizens from brutality and inhumane treatment at the direction of malevolent authority. A substantial proportion of people do what they are told to do, irrespective of the content of the act and without limitations of conscience, so long as they perceive that the command comes from a legitimate authority. If in this study an anonymous experimenter could successfully command adults to subdue a fifty year old man and force on him painful shocks against his protests, one can only wonder what government, with its vastly greater authority and prestige, can command on its subjects.⁴⁰

Milgram, and others too, were quite moved by the results of his experiments. Later, in a more theoretical vein on some of the wider questions he had raised, Milgram wrote:

If people were asked to render a moral judgment on what

constitutes appropriate behavior in this situation, they unfailingly see disobedience as proper. But values are not the only forces at work in an actual situation. They are but one narrow band of causes on the total spectrum of forces impinging on a person. Many people are unable to realize their values in action, and find themselves continuing in the experiment even though they protest what they are doing....

What, then, keeps the person obeying the experimenter? The answer consists of two parts. First, there are a set of 'binding factors' that lock the subject into the situation. They include such factors as politeness on his part, his desire to uphold his initial promise of aid to the experimenter, and the awkwardness of withdrawal. Second, a number of adjustments in the subject's thinking occur that undermine his resolve to break with authority. The adjustments help the subject ... to become so absorbed in the narrow technical performance of the task that he loses sight of the broader consequences of his action. He entrusts the broader tasks of setting goals and assessing morality to the experimental authority he is serving.

The most common adjustment of thought in the obedient subject is merely to see himself as not responsible for his own actions. He divests himself of responsibility by attributing all initiative to the experimenter, to a legitimate authority. He sees himself not as a full person acting in a morally accountable way, but as the agent of an external authority.

Persons under authority perform actions that seem to violate standards of conscience, but it would not be true to say that a moral sense has really disappeared. Instead, it acquires a radically different focus. Once a person has entered an authority system, he does not respond with a moral sentiment to the actions he performs. Rather, his moral concern now shifts to a consideration of how well or how poorly he is living up to the expectations that the authority has of him.... Thus, when the experimenter says, 'The experiment requires that you continue,' the subject feels an imperative that goes beyond human desire. He does not ask the seemingly obvious question, 'Whose experiment? Why should the designer be served while the victim suffers?' The wishes of man--the designer of the experiment--become embodied in a scheme which exerts a force on the subject's mind that transcends the personal. 'It's got to go on. It's got to go on,' repeats one subject. He fails to realize that a man like himself wants it to go on. For him the human agent has faded from the picture, and 'The Experiment' acquires an impersonal momentum of its own.⁴¹

How does such obedience occur? Milgram suggests that the subject first feels the agent's authority to be legitimate by identifying with such factors as the uniform worn by the agent and his relationship to the setting--this sets up the context of his activity. In addition, the subject shares an "overarching ideology" which gives legitimacy to the general setting in which the activity takes place. The subject then enters an "agentic state"--the context of his activities have been so defined that the individual becomes a willing subject of authority, having divested himself of responsibility for his own actions onto the agent.

Milgram's conclusions are clearly perceptive and their contribution to our understanding of the exercise of authority is well founded. Before they can be directly considered and applied, however, serious questions must be raised both about their validity outside the laboratory and, especially in relation to our own concerns, whether the exercise of authority in Milgram's experiment can be compared to all authority as he assumes, or whether it is of a specific case of scientific authority. Fortunately, Milgram's work has been subjected to a significant amount of critical review, especially in relation to the former point.

One line of criticism, directed not only at Milgram but also against all psychological experiments on humans which involve both deception and the causing of anxiety, is of a mainly ethical nature. An example of such criticism was that voiced by Diana Baumrind shortly after the publication of Milgram's first results.⁴² It was directed in particular at the more intense reactions undergone by some of Milgram's subjects. Milgram had noted that a good number of subjects showed signs of "nervous laughter" which among a minority turned into

"uncontrollable seizures." Other reactions included sweating, trembling, stuttering, biting of lips, groaning and the digging of fingernails into the subject's own flesh. Milgram had described, for instance, a "mature and initially poised business man" whose confidence and demeanor melted within twenty minutes into "the state of a nervous wreck:"

He constantly pulled on his earlobe, and twisted his hands. At one point he pushed his fist into his forehead and muttered: 'Oh God, let's stop it.' And yet he continued to respond to every word of the experimenter, and obeyed to the end.⁴³

Baumrind expressed concern about the after-effects on subjects of such an experiment as well as damage to the public image of psychology which she deemed likely in cases where the laboratory is used to study obedience and suggestibility. Most important to us, her conclusions were based on what she describes as the general conditions inhering in laboratory experiments of this kind: "Experiments in which the relationship to the experimenter as an authority is used as an independent condition are imperfectly designed for the same reason that they are prone to injure the subjects involved. They disregard the special quality of trust and obedience with which the subject appropriately regards the experimenter."⁴⁴

Milgram's rejoinder⁴⁵ contained a justification of his technique stressing both the ethical importance he attached to studying human obedience, especially after the Nazi experience, and the steps he took in talking to ("debriefing") the subjects after the experiment, pointing out that the great majority considered the experience beneficial. The question of just what ethical requirements are needed to justify violating this "special quality of trust" was not resolved.

A very interesting criticism levelled at Milgram by Lewis Brandt

raises the ethical question in an unexpectedly different way. Brandt notes that if one looked at what was going on in these experiments not from the perspective of their designer (Milgram), but from that of a complete outsider, the first thing which would be clear is that the only person in fact causing anxiety and pain was the researcher himself; the subjects--whatever they may have believed--were in fact causing no pain at all. "Had Milgram considered himself as just another human being from whose behavior something can be learned about human behavior in general, he would have known that human beings can inflict suffering on other human beings, if they can rationalize their behavior."⁴⁶ This is not done, says Brandt, because there is an implicit assumption of a fundamental difference between experimental psychologists and human beings in general:

When 'subjects' are viewed by the experimenter in the dictionary meaning of the word, the authoritarian relationship can lead the experimenter to consider their behavior as 'obedience.' The implicit assumption is then made that experimental psychologists differ from human experimental subjects to such an extent that similar overt behavior by the two groups cannot be assumed to result from similar covert causes (motivations, needs, drives, etc.). This distinction between experimenters and subjects is evidenced by explaining similar behavior of the two in terms of dissimilar motivations. In Milgram's experiments both experimenter and subjects inflict pain on others. This infliction of pain on others is explained by Milgram as 'obedience' when done by the subjects, and as 'examining situations in which the end is unknown' when done by the experimenter.⁴⁷

Brandt is indirectly getting at what will be our major analytical point in reference to Milgram's discussion of authority, namely the fact that it was in order to further not just any goals but scientific goals that the experiment was carried out and justified. From this perspective, the behavior of the subject is analogous to that of Milgram, the ends they served were the same. Caught up as he is within the

system of science, Milgram (like many other scientists, we suggest) is unable to disentangle the authority inherent in science itself from his own particular findings. Both Milgram and his subjects caused pain "in the name of science." There is one important difference: Milgram was obviously in a position to comprehend the parameters and the scientific rationale of the activity taking place; his subjects were not--they relied on the standards of science which lay with the scientist and him alone to interpret. We shall return to this.

The most direct criticism of the experiments takes up a question raised implicitly but not explored by Baumrin. While she was fearful of the ethical implications raised by studying and thereby tampering with the "special trust and obedience" of the experimental lab, more crucial is whether this special trust in fact makes the laboratory situation unique. There has been an important debate on the question: can one generalize from these laboratory experiments to U.S. society generally? The most complete argument against such generalizations is probably to be found in the writings of Martin T. Orne and Charles C. Holland.

In a careful discussion of Milgram's work, Orne and Holland pointed out how the laboratory situation he constructed may not have been testing obedience and willingness to inflict pain at all. They hypothesized that the subjects may very well have seen through the deception but continued to play the role they deemed appropriate because they did not want to ruin the experiment. They concluded:

In asking the subject to appear in the experiment, the experimenter implicitly says, 'Will you do whatever I ask for a specified period of time? By doing so you earn a fee, contribute to science, and perhaps even learn something of value to yourself. In return I promise that no harm will befall you. At the completion of the experiment you will be no better or

worse off than you are now and though you may experience temporary inconvenience, this is justified by the importance of the experiment.⁴⁸

In an earlier paper Orne had described the lengths to which individuals will go when they believe they are part of a scientific experiment.

For example, one task was to perform serial additions of each adjacent two numbers on sheets filled with rows of random digits. In order to complete just one sheet, the subject would be required to perform 224 additions! A stack of some 2,000 sheets was presented to each subject--clearly an impossible task to complete. After the instructions were given, the subject was deprived of his watch and told 'Continue to work; I will return eventually.' Five and one-half hours later, the experimenter gave up...! We tried to create a more frustrating situation as follows:

Subjects were asked to perform the same task described above but were also told that when they finished with the additions on each sheet, they should pick up a card from a large pile, which would instruct them on what to do next. However, every card in the pile read:

'You are to tear up the sheet of paper which you have just completed into a minimum of thirty-two pieces and go on to the next sheet of paper and continue working as you did before; when you have completed this piece of paper, pick up the next card which will instruct you further. Work as accurately and as rapidly as you can....'

Somewhat to our amazement, subjects tended to persist in the task for several hours with relatively little sign of overt hostility.... Thus far, we have been singularly unsuccessful in finding an experimental task which would be discontinued or, indeed, refused by subjects in an experimental setting.⁴⁹

In drawing attention to these findings, Orne and Holland warn against any attempt to generalize from them outside of the experimental context.

Anyone who believes direct inference about obedience in real life can be drawn from an experimental context should ask his secretary to type a letter, and after making certain there are no errors, ask her to tear it up and retype it. With rare exceptions, two or three such trials should be sufficient to ensure that he will

require a new secretary! It should be noted that the activity required of the secretary is no different in kind from what she is normally required to do. She is paid for the work at her usual rate, no one is hurt and yet as long as she has an option, there is little question about her behavior in real life. Incidentally, the same individual would likely be 'obedient' if she had agreed to participation in an experiment.⁵⁰

In sum, according to Orne and Holland, the "ecological validity" of Milgram's conclusions, that is the extent to which they can be taken out of the laboratory situation, is very much open to debate. The "demand" characteristics of the experiment, the implicit assumptions made by subjects on which they judge their performance as meeting the standards required of a "good" subject, must somehow be included. In a separate study, Holland attempted to study to what extent, if any, the demand aspects of the apparent obedience of the subjects can be separated from genuine obedience.

Holland set up conditions replicating those of Milgram using different individuals as experimenters. One group of subjects was given the original instructions. Another group was told to figure out the hidden intention of the experiment but to act naive; a third group was told that the shocks they administered were only one tenth of the registered strength but to act naive. The experimenters, who were kept "blind" as to the identity of the membership of each group, were asked to rate each subject and identify to which group he belonged. In the end, "analyses ... showed that it was not possible for the experimenter to distinguish between the subjects of the three groups. On the other hand, it was possible to demonstrate that the varying experimental instructions had been effective in influencing subject behavior in the way in which they had been intended to."⁵¹

Milgram's critics have made a very strong case to support their charge that he was improper in generalizing directly from his study of

obedience to authority in the laboratory to obedience to authority in the world outside. It is clear that some element of role-playing on the part of the subject must enter and it is impossible for the experimenter to isolate this factor completely from his results. This fact makes any concrete conclusion as to man's susceptibility to authority highly questionable. Undoubtedly, nevertheless, some of the individuals were obeying authority--consciously doing something on the command of the experimenter they would otherwise not do. Were this not the case, one could not account for the genuine anxiety expressed by many of the subjects. Furthermore, Milgram's finding that when the experimenter communicated his instructions by telephone many subjects feigned obedience also negates any such interpretation. If they were responding merely to the demand characteristics of the scientific experiment per se, why "cheat" in this way? Surely such behavior can only be explained by admitting that authority was definitely operative in the situation. Furthermore, in Milgram's post-experimental interviews, the great majority of the subjects said they had believed the victim was receiving painful shocks. In a replication carried on by Rosenhan, the post-experimental interviewer asked, "You really mean you didn't catch on to the experiment?" Even under such pressure, "60 percent of the subjects thoroughly accepted the authority of the experiment." Even when the findings were adjusted to exclude from consideration those who displayed suspicions in the interviews as to the authenticity of the experiment, the findings did not significantly change.⁵²

From our point of view, however, the issue in question is neither obedience to authority in general, as Milgram supposes, nor is it simply the special authority inherent in laboratory experiments, as

his critics' argue. As we hypothesized previously, scientific authority tends to be different from other forms of authority generally encountered in U.S. society in that the layman subject of such authority lacks the necessary knowledge and information to determine its legitimate boundaries. He cannot easily prejudge the appropriateness of the context and institutional framework in which it is exercised. These boundaries therefore are set, though generally unintentionally, by scientists themselves. When confronted with scientific authority not only in laboratory situations but in other institutions, the non-scientist tends to leave it up to the scientist (and science) to define boundaries and set limits. For these reasons, we suggest that what goes on in the experimental psychology laboratory should be seen as typical not of authority in general but specifically of scientific authority.

Orne and Holland, however unwillingly, buttress this argument strongly in their discussion of the secretary. If someone else in authority, say her employer, attempts to have her do the kind of absolutely useless and tedious work Orne had his subjects do, she soon balks. But if such work is assigned to her in the context of scientific experiment, her behavior will resemble that of the subjects. But only the scientist, the agent of scientific authority, can bestow upon a certain activity the title of scientific experiment; the person now deemed "experimenter" is exercising the authority of science. If, in their assertion that the laboratory situation is unique, they were arguing that the phenomenon of scientific authority is different from others, then Milgram's critics would be correct in denying the validity of generalizing his conclusions. In fact, however, being themselves experimental psychologists, they too are

caught up within the confines of the system of science and do not take into account the likelihood that scientific authority extends beyond the laboratory.

Milgram's error, as we noted, is in the opposite direction. As he points out correctly:

Consider, for example, how closely our compliance with the imperatives of others is tied to particular institutions and locales in our day-to-day activities. On request, we expose our throats to a man with a razor in the barber shop, but would not do so in a shoe store; in the latter setting we willingly follow the clerk's request to stand in our stockinged feet, but resist the command in a bank. In the laboratory of a great university, subjects may comply with a set of commands that would be resisted if given elsewhere. One must always question the relationship of obedience to a person's sense of the context in which he is operating.⁵³

This is what led him to replicate his experiments in an office in a commercial building in Bridgeport. His assumption was that if results tallied, then no one could argue it was simply a matter of a single context to explain the behavior of the subjects here. Hence he felt confidence that he could generalize from the behavior of the subjects in Bridgeport to all obedience to authority regardless of context. We suggest this is mistaken, for the context of the activity is far more broad than the extent of any association with Yale University. However subtle its manifestations, authority is always evoked in the context of some institution, but in this case the institution transcended both ivy and office.

The subjects are not merely obeying authority, they are obeying a particular kind of authority. They are placed in front of a powerful machine to which the victim is connected. (See Figure 4-1 below.) The environment is a reflection of the authority of

Figure 4-1

Shock generator.

Victim is strapped into chair.

Subject receives sample shock.

Subject breaks off experiment.

Source: Stanley Milgram, Obedience to Authority (New York: Harper and Row, 1974).

science and technology. The experimenter, the one who invariably insists that a further and higher shock be given, is not just another individual. "The E throughout this procedure remains calm and instructs the S to continue.... Regardless of the degree of agitation shown by the S or the anguished screams of the victim, the E remains imperturbable and demands that S continue."⁵⁴ The "S" becomes virtually an appendage of the machine, a connector between the command component in the form of the scientist experimenter and the shock mechanism. The brief set of instructions he receives at the outset from the experimenter includes the following phrases:

Psychologists have developed several theories....
 Some of the better theories are treated in this
 book (subject shown book). ... one theory is....
 A common application of this theory would....
 Almost no truly scientific studies have been made....
 So in this study....⁵⁵

Clearly, this relationship is a vital aspect of the situation, whether contrived under the auspices of Yale University or the "Research Associates of Bridgeport." It seems evident that the "uniform" of the experimenter, his manner of speaking, and the experimental apparatus he controls, sufficiently attest to the scientific nature of the goals of the tasks demanded of the subject.

What we are arguing, then, is that Milgram's description and analysis of the general phenomenon he had glimpsed in his experimental laboratory turns out to be a strong and interesting case study of scientific authority. His vivid description of what happens to the morality of the subject when he enters the authority relationship cannot be ascribed to all authority but rather to that one kind of authority which is virtually limitless from the subject's point of view. In such cases the subject sees himself as ignorant of vital information and therefore incapable of deciding on the appropriateness

of the authority encountered. He therefore abdicates this function to the agent of the authority. This is what Milgram's subjects' anxiety indicated--they were in effect asking the experimenter whether what he was asking of them was justified; they felt it was not, but they also accepted the fact that only he could know.

There are two objections which might at this point be raised to our description of the Milgram experiments as instances of scientific authority; one hinges on whether authority was actually exercised; the second on whether it was scientific authority as opposed to some other. On the first question we are quite convinced--as was Milgram himself--that authority was being exercised. He carefully designed the entire experiment to investigate this phenomenon as "purely" as possible. No critic doubts this. There is never a suggestion of power; the experimenter has no resources at his disposal to distribute. The few dollars promised each volunteer was guaranteed in the advertisement and given to him simply upon his arrival; he could earn nothing more. As for influence, certainly a communication of this kind (though deceitful in nature) was transmitted in persuading the volunteer to enter the experiment in the first place. The advertisement in the local daily newspaper to which the subjects responded stated in effect that: "if you wish to help us complete a scientific study of memory and learning and ... be paid \$4.00 ... fill out the coupon...."⁵⁶ (Note that the original advertisement included the statement: "The study is being conducted at Yale University.")⁵⁷

Nevertheless, there is only authority once the experiment begins. In fact the experimenter carefully resists any attempt to justify or even explain anything to comfort the subject in any way. Milgram is unquestionably right in calling the behavior he had evoked obedience

to authority. The language of the experimenter is clearly the language of authority:

The subject was told to administer a shock.... Moreover, and this is the key command, the subject was instructed He was also instructed.... At various points in the experiment the subject would turn to the experimenter The experimenter responded with a sequence of 'prods,' using as many as necessary to bring the subject into line.

Prod 1: Please continue, or Please go on.

Prod 2: The experiment requires that you continue.

Prod 3: It is absolutely essential that you continue.

Prod 4: You have no other choice, you must go on....

The experimenter's tone of voice was at all times firm, but not impolite.... If the subject asked if the learner was liable to suffer permanent physical injury the experimenter said: 'Although the shocks may be painful, there is no permanent tissue damage, so please go on.

(Followed by Prods 2, 3 and 4, if necessary.) If the subject said that the learner did not like it E said you must go on until he has learned all the word pairs correctly. So please go on. (Followed by Prods 2, 3 and 4, if necessary.)

A related objection which might be raised is whether a psychologist can legitimately lay claim to scientific authority, given the discipline's still unclear position vis-a-vis the "hard" sciences.

This too seems not particularly overwhelming. Non-scientists do not apply the criteria of the philosopher of science in choosing to regard a particular context as scientific. The criteria that are applied are twofold; the first has to do with the appropriate symbolism, (Miranda) such as the lab coat uniform, the setting of the laboratory, and words such as "experiment" used to describe the proceedings. The second might be considered more general. Is there anything in the experience or knowledge of the non-scientist which would lead him to question the appropriateness of the symbols? Does identification of the experiment as psychological weaken the scientific quality of the context? There is no reason to suggest so. We can think of no general cultural attitudes among Americans today which would lead them

to question that a psychologist is a scientist. After all, psychology, like the natural sciences and unlike the social sciences, has a technology--psychotherapy.

Milgram is wrong when he compares his results to the obedience given to government officials with presumably greater authority and prestige. Some of his own subjects admitted fearing prosecution for grievous assault for their actions--yet they continued nonetheless! In this situation, then, the authority of science took precedence over political or legal authority. This should not be surprising, for we suggest that Americans do not see political authority as "self-limiting" to the same extent that they so regard the authority of science. Political leaders are not as readily assumed to have special knowledge beyond the ken of others.

It appears, therefore, not unreasonable to assume one may generalize from the electric chair experiments in considering scientific authority. What is true in other instances where scientific authority is exercised seems in principle to be in part demonstrated in these experiments. Using Milgram's own examples, we know we do not submit our throats to barbers and our feet to shoe salesmen, except under quite specific circumstances. But what matters is that we are usually quite capable of deciding and articulating just what these circumstances are--and just when such behavior on our parts is called for. It is the subject rather than the agent who essentially defines the limits of these other kinds of authority. This is apparently not so for many people with respect to science. The authority relationship found in the laboratory experiment can be recreated outside the laboratory by emitting the aura of science. The reader is reminded of the author's adolescent "experiment" recounted

in the Introduction, where commanded by one of their group, twenty youngsters and one 26 year old group leader acted rather ludicrously simply on the assurance that they were participants in a "scientific experiment." Though in this case set within a specific group with common goals, such occurrences should make us question Milgram's assumption that "should the experimenter encounter the subject on the street, he would have no special influence on him."⁵⁸ On the contrary, it seems that science potentially provides for the scientist a sort of portable laboratory if he wishes to make use of it.

In sum, Milgram is not measuring general obedience to authority, but he is getting at something in the real world outside the laboratory--scientific authority.⁵⁹

One factor in experimental situations briefly meriting our continued attention is what has been called the effect of experimenter expectancy. Working on concerns similar to those raised by Orne and Holland, Rosenthal, Friedman and their collaborators have tried to identify some of the more subtle patterns found in scientific experiments with human subjects.⁶⁰ Their major conclusion is to confirm that experimental subjects tend to identify themselves as such, and want to succeed as "good" subjects. These researchers have found that subjects often respond not only to those instructions consciously articulated by the experimenter, but also to involuntary and unconscious clues in the demeanor, appearance and personality of the experimenter and in the atmosphere of the setting. There is a tendency on the part of the subjects to attempt to identify the experiment's goals and to act so as to further these. Such expectancy effects clearly colour the results of experiments in which they play a role. The normal experimental solution is to employ

"blind" experimenters who themselves are unaware of the goals of the experiment.

Rosenthal designed various tests for measuring expectancy effects. One was to have several experimenters each ask a group of subjects to rate the character of people in a series of photographs, instructing some experimenters that the desired outcome is a positive orientation on the part of the subjects to the people in the photographs, and others that the goal is for negative orientation. A significant statistical skewing in the direction of the experimenters' expectations was found. Friedman concludes: "Taken together, these ... interpretations amount to a projection hypothesis ... the subject sees in the photos what his interaction with the experimenter leads him to feel himself."⁶¹

The most significant factor associated with the increase of expectancy effects was the "status" of the experimenter. High status individuals, in general, are more successful in obtaining such conformity. Analogous results were obtained with non-laboratory experiments. In one instance, a dime was left in a phone booth and the person next making a call was asked to return the dime. It was found that only 38% returned it to the person of low status attire, while 77% returned it to the person whose appearance was clearly upper status.⁶² In another experiment, an individual walked against a "wait" signal at a crowded intersection. He wore high and low status attire in an equal number of cases. A significantly greater number of other pedestrians joined him in violating the signal when he dressed in suit and tie, etc., than when "scruffy."⁶³

While in such cases, higher status conforms generally with

higher social class, in the laboratory situation the most significant aspect of status is professional appearance; as Rosenthal noted, "greater expectancy effects were shown ... by those experimenters who were judged as more professional. Professional status was found as the most significant factor in inducing conformity to expectancy,"⁶⁴ while the second was a relaxed and interested style on the part of the experimenters--though only so long as they maintain a professional manner."⁶⁵ Other elements of the appearance of the experimenter which affected his performance were found to include whether or not he articulated the instructions competently and whether he wore glasses.⁶⁶ This last factor probably relates to the popular stereotyped professional image of the scientist; one doubts whether the wearing of glasses by the jaywalker would have induced people to cross against the signal.

The above strengthens our contention that the professional appearance and demeanor of the scientist-experimenter (plus the technological/scientific appearance of the surroundings) are primary aspects which increase his perceived authenticity as an agent of scientific authority--that these somewhat intangible factors make up what might be called the "scientific uniform."⁶⁷

This fact has, of course, not been lost on others who would take recourse to scientific authority such as actors in laboratory coats to sell pills, toothpaste, deodorants, gasoline brands, etc., on television commercials. Even in closely related aspects of knowledge, one can sometimes gauge the extent to which appeal is made to scientific aspects of the authority inherent in the relationship. This often takes rather subtle forms. For example, Dr. Basil Yanofsky, in describing his case work with over three hundred patients

suffering from venereal disease, found that many were reluctant to divulge to him the necessary highly personal information. However, "if I hold a pen in my hand and look at the chart, I can ask the most important questions and always get a reply. That is considered objective, scientific, and of possible use in the treatment."⁶⁸

Summing up thus far, we have argued that such laboratory experiments as Milgram's may plausibly be viewed as examples of the exercise of scientific authority. As such, three main generalizations emerge. First, science tends to set the limits of its own authority. Second, we conclude, and here we apply some of Milgram's more general assertions, that this kind of authority often entails a suspension of normal moral judgment on the part of the subject, an acceptance of conditions and actions otherwise morally unacceptable if not morally reprehensible to him. However cogent the point of Orne and others in relation to "demand" characteristics appears to be, the overpowering and unrefuted fact is that Milgram or an "evil" scientist (or "malevolent authority"--to use Milgram's term) could very well have been doing just exactly what the experimenter told the subjects he was doing, and there is no good reason to believe that the subjects' response would have been markedly different. Finally, and of course the actual proportion is merely speculative, a large number of present-day Americans can be expected to react to scientific authority in just these ways.

Chapter Conclusion

We are now in a position to conclude this discussion by bringing together the main points which emerge from each of the three sections. Here we tentatively attempt to synthesize in order to suggest how a start may be made to apply our conclusions in an endeavour to overcome

some of the conceptual difficulties in the analyses considered in earlier chapters. We have investigated the concepts of power, influence and authority and applied each to the relationship between science and society in many of its manifold forms and manifestations. We have treated each of these three as different kinds of relationships and found them to be most usefully distinguished by the linguistic paradigm⁶⁹ which characterizes each. We found power communications not normally manifest in scientist-non-scientist communications; we found influence adequate to describe some (often unfortunately referred to as authority-based) but not all; and we suggested that the concept of scientific authority best described a highly significant relationship, thus far virtually ignored in the literature. Further, while arguing on the one hand that no concept other than scientific authority could optimally serve, we found it necessary on the other to expand the definition of authority beyond its normal social science usage to encompass scientific authority. This general argument was buttressed by a description and analysis of the electric chair experiments which we found to be an almost "made to order" demonstration of the workings of dyadic scientific authority. Moreover, we found that scientific authority can be distinguished not only from influence and power, but also from more traditional forms of authority.

Summing up the various relevant forms, we conclude that scientific Power in the U.S. today may be usefully viewed as a combination of:

- 1) Scientific Influence: behavioural change on the part of non-scientists in response to reception and sympathetic decoding of communications from scientists in the form of predictions (warnings,

recommendations) understood to be based on their specialized knowledge or expertise.

2) Bureaucratic Authority of Scientists: commands (frequently leading to behavioural change by non-scientists) issued by people trained as scientists holding positions in formal organizations. In such a case the content of the commands is decoded by the subject as legitimate because of the position occupied by the scientist as set out in the accepted rules of the institutions. (The authority of the public health officer in our earlier example would be of this kind.)

3) Scientific Authority: commands (frequently leading to behavioural change by non-scientists in relationships even outside or beyond recognized formal institutional settings) in which scientists exercise command by virtue of the fact that the subjects decode acceptingly and without question the definition imposed (implicitly) by the scientist that obedience should be forthcoming because of the socially accepted legitimacy of science and scientific knowledge. (For reasons delineated above we exclude power as a relevant and distinct relationship. However, this kind of relationship may be included in a discussion of the "bureaucratic authority of scientists," when such authority is enhanced by power analogous to our example of the established church. Such a case could be said to apply usefully only when the institution in question is unmistakably controlled by some part of the scientific community and where denial of membership privileges is understood as a threat, that is, constitutes a sanction, to individuals outside the scientific community. This point is taken up in Chapter Five.)

The third case, scientific authority, does seem clearly to distinguish scientific Power in the U.S. today from the Power of other

knowledge-based groups. We noted also that such cases are hard to locate empirically outside the rather rarefied confines of the psychologist's laboratory. Moreover, one should add that the other two types of relationships are also rare in their pure sense. This is because "pure" or "ideal type" relationships of any nature are not likely to be found in the complex and variegated social world.⁷⁰ Conversely, the real world is often easier to comprehend by utilization of such concepts.

When applying our concepts to the real world, as we shall attempt in Chapter Five, it will be more advantageous, thus, to view scientific Power as a compound of these three forms where each socially important manifestation features a "mix" of the different forms. We might hypothesize in addition that the more important the empirical cases of scientific power, the more likely they are to demonstrate all three forms in some combination, that is, the more complex will be the mix. Finally, we might hypothesize that a key to the "formula" of the mix, if there is one, is to be found within the linguistic structure and the pattern of encoding, transmitting and decoding of the communication which goes on within each case.

Let us now see if the above may potentially be applied usefully to threads of theoretical problems left hanging from earlier chapters. In Chapter Two we concluded that while there existed more complex theoretical and ideological divisions, when it came to one's assessment of the present relationship of science and society and one's outlook on future possibilities, there existed fundamentally three positions which we labelled "supportive," "reformist," and "radical." The first entailed a positive attitude toward the perceived developing status quo. The second evinced a displeasure with many of the present

workings of the interrelations between society and science but also a belief that these could be reformed without a major transformation of either. The third involved rejection of the entire science-society relationship as it currently exists combined with a belief that only a radical transformation of the society, its culture and organizational structure, can bring about the desired changes.

We also noted that there are essentially three levels of analysis used by the different "authorities" on the subject. At the first level the subject is seen, discussed and examined in relatively microscopic terms. This kind of analysis seeks to measure the effect of a particular scientist or a particular technological unit, for example a computer, in a limited and highly empirical way. Weinberg's notion of the technological fix as furnished by scientists organized in a particular fashion to solve a specific problem, provided a concrete instance of this level of thinking. Crozier's methodology provided another example in that it was concerned with describing the technologists' day-to-day, normal activities and assessing their influence on particular issues which arise in those institutions--and generalizing from these cases. In general, this analysis views scientists as able to provide their "input" in the form of technology or advice; society in its wisdom may (does) choose what is needed.

At the second level of analysis one looks not so much at the role of the individuals or the impact of particular technologies, but rather at the structure and content of policies, decisions, organizations, and other intermediate level social phenomena which one takes to be important. Education is a subject close to the hearts of those who use this level of analysis: they strongly advocate the educating of laymen and politicians as well as scientists, so that all are taught

to play their proper roles: the scientists must be educated to be socially responsible in their work, while the public and politicians must be sufficiently trained to gain a basic comprehension of the real and potential consequences of science and technology. This view would wish to see scientists and properly educated laymen filling the key decision making positions. The ideal tendency would be to investigate a certain issue facing society in which science and technology are implicated and to analyze the process for the taking of a decision. Is the public educated as to its consequences? Does the structure place scientists in a proper relationship with both public and policy maker (through a fourth branch of government if necessary)? Is there built-in scientific advocacy available for public participation in decisions where science and technology are involved in a manner analogous to the right to defense counsel before the court?

Finally, from the third position, the problem is seen not in terms of individual issues or roles but rather as one which is inherent in the social fabric and culture of the society itself, in the convergence of different techniques, structures, cultural attitudes, etc., upon each individual's present condition and prospects--and thereby the future of society as a whole.

We noted at the end of the last chapter that there appeared to be some correspondence between the three positions and the levels of analysis, though not a one-to-one correspondence. Those who took the "supportive" position tended to see the workings of society at the microscopic level; likewise the radicals tended to adopt the most macroscopic viewpoint, with the reformers' perspective usually at the intermediate level. Hence by integrating the three levels of analyses utilizing one conceptual framework, one is also potentially beginning

to provide a way of effectively comparing and criticizing the three different and, most often, differently conceptualized positions. We suggest that the framework employing the above derived analysis of scientific authority is a fruitful point of departure.

Let us look at that type of relationship we labelled as scientific influence. The image here is one of scientists and laymen acting together in a joint activity, certain elements of which are understood to be within the boundaries of science, with scientists acting as experts within a circumscribed domain, so that there need be no conflict or confrontation. This type of situation corresponds to the analysis of the establishment thinkers where the basic question is seen as pertaining to the specific and concrete relationship of scientists and non-scientists concerned with a particular, concrete issue. The specific role of the teams of scientists, experts, or systems analysts favored for instance by Ramo, Brzezinski, and Brownell, to design technology to "fix" concrete, identifiable, and immediate social problems, corresponds quite closely to the picture of scientific influence.

The bureaucratic authority of scientists relates to the Power of individual scientists in complex institutions. The authority is exercised directly by individual scientists through their positions in the management of the institution, and both the agents of authority and its subjects are expected to act according to the rules, standards and/or norms of the institution. If one looks at the more general kinds of arguments and methods of the group we have labelled as "reformers" one sees that these typically are most concerned with such roles as fit in this category. We have noted that the exponents of this position are mainly concerned with the social responsibility of the scientists

and the informed rationality of decision makers and the public. This concern can be characterized first as the hope that properly educated people will be placed in positions of decision making when important issues are at stake and where the proper use of science and technology is a crucial element. This requires scientists who are both socially responsible and influential. One way of conceptualizing such situations, as the bureaucratic authority of scientists, is where the analysis would relate to the question of how scientific goals are maintained and transmitted via the structure of certain key institutions to non-scientists affected by those institutions, and the roles of scientists therein. The interests of reformers in setting up such new bodies as scientific assemblies, or a "fourth branch" of government, fits within this kind of framework.

Finally, our conception of scientific authority which ultimately relates to the cultural values of a highly complex social system where science increasingly sets societal standards, obviously relates most readily to the views and methodologies of the "radicals." Hence, we suggest, the kind of framework outlined here may be usefully incorporated into the wider debate with possibly beneficial results.

Notes

1. Schooler, op. cit., p. 241.
2. See Jerome Frank, Persuasion and Healing (Baltimore: Johns Hopkins University Press, 1961), especially Chapter 6. See also, Arthur K. Shapiro, "Placebo Effects in Medicine, Psychotherapy, and Psychoanalysis," A.E. Bergin and S.L. Garfield, eds., Psychotherapy and Behavior Change (New York: Wiley, 1971), pp. 439-73.
3. See Frank, op. cit., Chapters 1-3. See below for several examples as to the kinds of arduous, unpleasant, and useless tasks that people will carry out under what they consider legitimate-authority.
4. Schooler, op. cit., pp. 28-29.
5. Austin describes speech acts as statements that are not descriptive (constative) in that they cannot be judged true or false, but rather performative, that is, indications "that the issuance of the utterance is the performing of an action." J.L. Austin, How to do Things with Words (Cambridge: Harvard University Press, 1962), pp. 6-7. Austin modifies this distinction later in this work but not significantly. See also John Searle, Speech Acts (Cambridge: Cambridge University Press, 1969).
6. David V.J. Bell, Power, Influence and Authority: An Essay in Political Linguistics (New York: Oxford University Press, 1975).
7. Edward Banfield, Political Influence (New York: The Free Press, 1961), p. 348.
8. Robert A. Dahl, Modern Political Analysis (Englewood Cliffs, N.J.: Prentice-Hall, 1963), p. 50.
9. See Robert A. Dahl, "The Concept of Power," Behavioral Science, Vol. 2 (July, 1957), pp. 201-18.
10. Dahl, Modern Political Analysis, op. cit., p. 51.
11. David Easton, "One Perception of Authority and Political Change," C.J. Friedrich, ed., Authority: Nomos I (Cambridge: Harvard University Press, 1958), p. 178.
12. Ibid.
13. Sanford M. Dornbusch and W. Richard Scott, Evaluation and the Exercise of Authority (San Francisco: Jossey-Bass, 1975), p. 33.

14. Gerhard E. Lenski, Power and Privilege (New York: McGraw-Hill, 1966), p. 57.
15. Max Weber, The Theory of Social and Economic Organization (New York: Free Press, 1964), p. 152.
16. Dahl, "The Concept of Power," op. cit., pp. 202-03.
17. Robert L. Peabody, Organizational Authority (New York: Prentice-Hall, 1964), p. 5.
18. The standard definitions tend to build compliance into the concepts, a practice which David Bell and others find unnecessary and perhaps unsuitable and one we try to avoid. Bell states: "Whether compliance occurs should be an empirical question, open to investigation, not an a priori assumption entailed by the concept of power." David V.J. Bell, "Political Linguistics and the Study of Power," (Unpublished paper presented at the XVII annual convention, International Studies Association, Toronto: February, 1976.)
19. Bell, Power, Influence and Authority, op. cit., pp. 21-26.
20. For instance, one of political science's most widely accepted definitions is David Easton's. He describes authority as dealing with the "authoritative allocation of values."
21. "I found that the status of 'authority' in the other discipline most vocal on the issue, political science, was that of confusion." J.H. Campbell, "The Concept of Authority: An Analysis," (Unpublished Ph.D. dissertation, Duke University, 1966); from the abstract.
22. Weber, op. cit., p. 328.
23. Ibid., p. 130.
24. Martin E. Spencer, "Weber on Legitimate Norms and Authority," British Journal of Sociology, Vol. 21 (1970), p. 129.
25. Chester I. Barnard, The Functions of the Executive (Cambridge: Harvard University Press, 1968, first published in 1938), pp. 163-65. (Emphasis included.)
26. Herbert Simon, Administrative Behavior (New York: Free Press, 1965), p. 125.
27. Bernard Barber, Social Stratification (New York: Harcourt Brace and Co., 1957), p. 236; Amitai Etzioni, A Comparative Analysis of Complex Organization (New York: Free Press, 1961), p. 41; H.D. Laswell and A. Kaplan, Power and Society (New Haven: Yale University Press, 1950), p. 133; Richard Schermerhorn, Society and Power (New York: Random House, 1961), p. 5.
28. Carl J. Friedrich, "Authority, Reason and Discretion," Friedrich, ed., op. cit., p. 29. See also his Tradition and Authority (New York: Praeger, 1972), pp. 47-56.

29. Robert A. Dahl, After the Revolution (New Haven: Yale University Press, 1970), p. 8.
30. Bell, Power, Influence and Authority, op. cit., pp. 76-77.
31. Talcott Parsons, "Authority, Legitimation and Political Action," Friedrich, ed., op. cit., p. 210.
32. Robert Bierstedt, The Social Order (New York: McGraw-Hill, 1970), pp. 333-38.
33. Peter Winch, "Authority," A. Quinton, ed., Political Philosophy (Oxford: Oxford University Press, 1967), p. 102.
34. Quoted in Hillier Krieghbaum, Science and the Mass Media (New York: New York University Press, 1967), p. 156.
35. Harold L. Neiburg, ed., In the Name of Science (Chicago: Quadrangle, 1966), pp. 103-04.
36. Bertrand de Jouvenel, "The Political Consequences of the Rise of Science," Bulletin of the Atomic Scientists, Vol. 19, No. 10 (December, 1963), p. 3.
37. Bell, Power, Influence and Authority, op. cit., pp. 36-37.
38. Milgram published his results in several places; the most important of these are: "Behavioral Study of Obedience," Journal of Abnormal and Social Psychology, Vol. 67, No. 4 (1963), pp. 371-78; "Liberating Effects of Group Pressure," Journal of Personality and Social Psychology, Vol. 1, No. 2 (1965), pp. 127-34; "Some Conditions of Obedience and Disobedience to Authority," Human Relations, Vol. 18, No. 1 (1965), pp. 56-57; Obedience to Authority (New York: Harper and Row, 1974).
39. Germany: David M. Mantell, "The Potential for Violence in Germany," Journal of Social Issues, Vol. 27, No. 4 (1971), pp. 101-12.

Italy: Leonardo Ancona and Rosetta Pareyson, "Contributo allo studio della aggressione: La dinamica della obbedienza distruttiva," Archiva di Psicologia Neurologia e Psichiatria, Anna XXIX (1968), fasc. IV.
- Australia: W. Kilham and L. Mann, "Level of Destructive Obedience as a Function of Transmitter and Executant Roles in the Milgram Obedience Paradigm," Journal of Personality and Social Psychology, Vol. 29, No. 5 (May, 1974), pp. 696-702.
40. Milgram, "Behavioral Study of Obedience," op. cit., pp. 375-76.
41. Stanley Milgram, "The Compulsion to do Evil," Patterns of Prejudice, Vol. 1, No. 6 (1967), pp. 4-5.
42. Diana Baumrind, "Some Thoughts on Ethics and Research: After Reading Milgram's Behavioral Study of Obedience," American Psychologist, Vol. 19 (1964), pp. 421-23.

43. Quoted in ibid., p. 422.
44. Ibid., p. 421.
45. Stanley Milgram, "Issues in the Study of Obedience: A Reply to Baumrind," ibid., pp. 848-52.
46. Lewis W. Brandt, "Science, Fallacies and Ethics," Canadian Psychologist, Vol. 12, No. 2 (April, 1971), pp. 231-42.
47. Ibid., p. 239. (Emphasis included.)
48. Martin T. Orne and Charles C. Holland, "On the Ecological Validity of Laboratory Deceptions," International Journal of Psychiatry, Vol. 6, No. 4 (1968), p. 291.
49. Martin T. Orne, "On the Social Psychology of the Psychological Experiment: With Particular Reference to Demand Characteristics and their Implications," American Psychologist, Vol. 17 (1962), pp. 776-83.
50. Orne and Holland, op. cit., p. 291.
51. Charles C. Holland, Sources of Variance in the Experimental Investigation of Behavioral Obedience, (Unpublished Ph.D dissertation, University of Connecticut, 1968); from the abstract.
52. See Milgram, Obedience to Authority, op. cit., pp. 172-73. See also his "Interpreting Obedience," Arthur G. Miller, ed., The Social Psychology of Psychological Research (New York: Free Press, 1972).
53. Milgram, "Behavioral Study of Obedience," op. cit., p. 271.
54. Orne and Holland, op. cit., p. 286.
55. Milgram, Obedience to Authority, op. cit., p. 18.
56. Ibid., p. 15.
57. Ibid.
58. Ibid., pp. 21-22.
59. Since this was written, I have come across three suggestively similar interpretations:

1) "I do not think that this experiment permits any conclusion with regard to most situations in real life. The psychologist was not only an authority to whom one owes obedience, but a representative of Science and of one of the most prestigious institutions of higher education in the United States. Considering that science is widely regarded as the highest value in contemporary industrial society, it is very difficult for the average person to believe that what science commands could be wrong or immoral. If the Lord had not told Abraham to kill his son, Abraham would have done it, like millions of parents who practiced child sacrifice in history. For the believer neither

God nor his modern equivalent. Science can command anything that is wrong. For this reason, plus others mentioned by Milgram, the high degree of obedience is not more surprising than that 35 per cent of the group refused at some point to obey; in fact this disobedience of more than a third might well be considered more surprising--and encouraging." Erich Fromm, The Origins of Human Destructiveness (New York: Holt, Rinehart and Winston, 1973), p. 51.

2) "Why? Why? How could They?

"The answer is written on thousands of pages of history, if you can bear to read them. Stanley was Dr. Stanley Milgram ... the Scientist ... in a 'real scientific laboratory.' He wasn't just Stanley. Indeed, they would have electrified Stan too, if he had been only the man in the chair." Alfred de Grazia, Politics for Better or Worse (Glenview, Ill.: Scott, Foresman, 1973), p. 323.

3) "It is when the super-ego sanctions us to act freely and punitively in the name of some of humanity's highest ideals--whether it be the Deity or the Nation, or Democracy, or the Revolution, Science itself--that the great horrors of human aggressivity, violence, and destruction are unleashed. Milgram's book is an excellent exemplification of this notion, since it is really about the authority of Science rather than about authority in general. This explanation does not, of course, explain very much, but may at least begin to put matters in such a way that real understanding can become possible." Steven Marcus, "Review of Milgram's Obedience to Authority," New York Times Book Review (January 13, 1974), p. 3.

60. See Robert Rosenthal, Experimental Effects in Behavioral Research (New York: Appleton, Century, Crofts, 1966); Neil Friedman, The Social Nature of Psychological Research (New York: Basic Books, 1971).

61. Friedman, ibid., p. 56.

62. Leonard Bickman, "The Effect of Social Status on the Honesty of Others," Journal of Social Psychology, Vol. 85 (1971), pp. 87-91.

63. Monroe Lefkowitz, et al., "Status Factors in Pedestrian Violation of Traffic Signals," Journal of Abnormal Psychology, Vol. 51 (1955), pp. 704-06.

64. Other experiments have shown that ascribed professional authority lends credence to written material. This means a sample of subjects will evaluate the same piece of writing differently when the author is described in high status terms of expertise, or in low status terms. See, for example, Monte M. Page, "Role of Demand Awareness in the Communicator Credibility Effect," Journal of Social Psychology, Vol. 82 (1970), pp. 58-65.

65. Rosenthal, op. cit., p. 258.

66. Friedman, op. cit., pp. 50-52.

67. See Leonard Bickman, "Effects of Different Uniforms in Field

Situations," Proceedings of the 79th Annual Convention of the APA, Vol. 67 (1968), pp. 359-60.

68. Quoted in W.H. Auden, "Veni, Vidi, Vici," New York Review of Books (February 22, 1973), p. 7.

69. See Bell, "Political Linguistics and the Study of Power," op. cit., p. 12.

70. Bell, Power, Influence and Authority, op. cit., has attempted to explicate these cautions in several ways. He has noted that: "Almost universally, title to political authority simultaneously entails both power and influence." (p. 41) That is, "Individuals who are in an institutional position to use the language of authority, i.e., to issue commands, orders, directives, etc., to their 'subordinates,' can usually also use the languages of power and influence. They can 'threaten' to fire a subordinate, 'promise' to recommend him for a promotion, or 'convince' him that a certain policy will bring good results." (p. 40) Bell further states: "Through the mechanism of authority and the attendant division of labor large numbers of people are able to interact in a productive way to accomplish a wide range of collective activities. In coordinated action, authority can be seen as a vast improvement over either power (which may breed hostility) or influence (which is unstable). This statement should not be taken to mean that where authority is present, power and influence evaporate. On the contrary, all three tend to coexist. Authority, however, does stand out as a technique which, while 'backed' by power or influence, operates in a different and in this case far more efficient manner, replacing threats/promises or persuasive arguments with 'decrees,' 'instructions,' rules,' and so on." (p. 63)

CHAPTER FIVE

The U.S. Atomic Energy Commission:A Case Study of Criticism

In the last chapter we concluded by suggesting that today the "external component of science" or "scientific Power" can best be understood as a composite of three conceptually distinct though (usually) empirically diffused kinds of relationships; scientific influence, the bureaucratic (or formal) authority of scientists, and scientific authority. We suggested further that one might usefully in a general sense associate the unstated conceptual framework underlying each of the three major positions concerning science and society with these three kinds of relationships: those who hold the supportive position tend to focus on scientific influence, the oppositionists tend to see the bureaucratic authority of scientists, while the radicals may be said to be concerned with scientific authority as we have defined it.

This being the case, one might expect to be able directly to contrast the three positions usefully and evaluate their merit by assessing the contemporary societal importance of the three kinds of relationships. Unfortunately, the problem is somewhat deeper, for, as suggested above, these relationships--and especially that of scientific authority--tend to be manifested not in distinct forms but rather in complex institutions in which more than one type of relationship is present. The Milgram experiments were thus particularly useful since they served to isolate this third, most elusive and yet clearly important, type.

Hence our tack must be different and more tentative. Instead of attempting to identify distinct cases manifesting each of the three types of relationships, we shall select one highly important case where the "power" of science or scientists is unmistakably present and interpret its ramifications with reference to our typology, and particularly to scientific authority. Does this conceptual formulation add to our understanding of this experience in a manner not possible without recourse to it? Our goal here is essentially heuristic and explanatory. We make no attempt in this chapter to apply our findings in such a way as to "take sides" in the science and society debate. Only in the conclusion which follows do we allow ourselves some brief speculation in that direction.

Among those institutions where scientists play a major role are hospitals and other para-medical facilities, research-oriented corporations, and specific task-oriented government agencies and the like. Each of these institutions presents somewhat discrete difficulties for one interested in studying scientific power, influence and authority. Their scientific, medical, financial, political, or simply bureaucratic goals must enter any discussion of their actual operating values. Hence our selection of one of them must be taken on grounds other than scientific "purity." The main criterion must be the societal importance of the case in question. Our choice is a government agency, the United States' Atomic Energy Commission (AEC), probably the most important of science-oriented agencies from the end of the second World War until its recent demise. The AEC can be viewed historically as perhaps the American scientific institution par extraordinaire, the direct offspring of the Manhattan Project and a prime example of the institutionalization of American science since World War II. Because

of its extraordinary nature and importance, the ACE, even less than other possible cases, does not readily lend itself to generalization to other institutions. Instead, however, it should be seen as providing one of the best possible "testing grounds" for the general approach offered above. One may say: no other institution has both symbolized and manifested the special role of U.S. science and scientists as did develop during this crucial period to the same degree.

The Atomic Energy Commission can justifiably be seen as a prime symbol of the accomplishment, potential, and also weakness of contemporary American science. Its scientific accomplishments have on the whole been notable, its resources and expenditures were great, and it attracted many leading physical scientists to its ranks. In addition, it benefitted from the postwar prestige of atomic physics among scientists as a field of scientific endeavour and from the awe and respect for atomic development, particularly in weapons, accorded by the American public.

Nevertheless, in spite of or perhaps partially because of this, the AEC did not succeed in fully retaining the esteem of the nation. It was the subject of numerous, and some far-reaching, controversies throughout its 29 year history. In many ways one could characterize it as a political failure. An important question is: can this failure, when compared with the hopes which greeted its founding, be explained to some extent by the presence and misuse of scientific authority? In attempting to answer this question, we make no attempt to give a full account of the historical development of the AEC or even of the various instances selected.¹ We are merely using apparently characteristic examples of AEC operations--the explanation of which may be enhanced by an understanding of scientific authority.

Because of this what follows may appear unnecessarily harsh on the AEC. This is unavoidable in an analysis of this kind, and in this case the U.S. executive decision to kill the AEC was much harsher. We are not interested in the Atomic Energy Commission per se and make no attempt to provide a comprehensive account of its workings. Instead this is a study of criticisms laid against the AEC which appear to provide useful patterns for analysis.

In discussing the AEC we first briefly describe the scope of its operations and the evaluation of its role in American society, and then go on to look at several decisions involving the body in an endeavour to distinguish those aspects in which scientific authority was a factor. Some tentative general conclusions will follow.

The AEC was among the most powerful of federal agencies. Although there were only some 7,000 federal employees on its payroll, its annual budget totalled almost \$2,500,000,000 which it dispersed in all 50 states to private contractors and research institutions--thus indirectly employing approximately 120,000 people. The AEC's use of American experts in atomic physics was almost universal. NAS president Handler noted: "It is essentially impossible to find more than a small handful of such experts who have not, in relatively recent times, had significant support--either research grants or actual employment--from the U.S. Atomic Energy Commission."²

Like the Manhattan Project it succeeded, the AEC was without parallel in American society. This was due to its wealth (with the largest "per capita" allocation of any federal agency), the scope of its powers, the importance accorded to atomic energy by politicians and the American public, and to the special congressional relationship which developed. The AEC's tasks were several: it

promoted the use of atomic energy; it developed and tested atomic weapons and supplied them to the military; it supported atom-related scientific research in its own laboratories, and in universities and private corporations; and it regulated the nuclear power and uranium mining and processing industries. Its budget attested to its growth, as Ralph Lapp noted in 1970:

Back in the late 1940's we could not foresee that the U.S. investment in atomic facilities would exceed \$10 billion by 1970. Nor could we foresee a twenty-year procurement of uranium costing in excess of \$6 billion to insure the flow of raw material from which to fashion A-bombs. In fact, in the past two decades the Atomic Energy Commission has spent \$11 billion for the production of nuclear material and \$10.8 billion for weapons development and fabrication.³

While only a minority of the Commission's five member board were scientists for most of its life, the bulk of the AEC's work could be labelled as pure or applied science and was conducted or directed by scientists. At least two-thirds of the AEC's expenditures were considered part of the federal science budget.⁴

The AEC was set up in 1946 by an act of Congress known as the McMahon Act. The Commission was to be headed by four commissioners and a chairman appointed by the President. It was given the task of developing atomic energy for all purposes. The fact that atomic energy was given to a civilian agency rather than to the military was seen as a victory for those, mainly scientists, who cautioned restraint in the development and use of the atom.⁵ Even then, however, there was much questioning of whether the separation from the military was more apparent than real.⁶ Three of the first commissioners, for example, turned out to be military men--charged with the dual function of developing atomic energy and regulating its usage for both domestic power and weapons development.

The McMahon Act set up the Joint Committee on Atomic Energy (JCAE) composed of nine senators and nine congressmen, and gave it wide and unusual powers of action in the name of Congress, including access to all necessary C.I.A. and F.B.I. information, in exercising its task of overseeing the activities of an executive agency. Theodore Lowi in 1969 well summed up the special nature of the AEC:

The Atomic Energy Commission's powers within its vast domain exceed those of any department of the government ever before established. The Commission has, in effect a plenary charter to do anything in the field of atomic energy that will promote the public safety and welfare. Section 4 of the Act of 1946, for instance, turns over all ownership of fissionable materials and control of all production facilities. Decisions on fissionable material to be released for research or as irrelevant to bomb production are left to the Commission. The Commission was set up deliberately and legally as a monopoly....

Thus, the AEC became its own boss. It was made independent, and apparently continues to be just as independent, of State and of Defense, and, therefore, of the Presidency. In security affairs, the AEC is a co-equal partner of Defense.⁷

The AEC had a controversial and sometimes turbulent history. One apparent problem was the contradiction between its tasks as promoter and regulator of atomic energy. Secondly, the Joint Committee appears on the whole to have exacerbated this problem by becoming, beginning in the early 1960's, more a defender than a watchdog of the agency. These failings are largely due to the political, economic and military orientations of the U.S. during this period and the consequent role of atomic development, but we are convinced that the role of science, and specifically scientific authority in relation to the AEC, constitutes an additional explanatory factor.

Historically, the AEC went through several phases. We exclude the war years and the Manhattan Project since the AEC was not at this point established, but note that, since the AEC in many respects

was a continuation of the Manhattan Project in peacetime under a different title, some of its practices were probably inherited from this period, especially its often noted "obsession" with secrecy. The AEC's first five years of existence saw it on the defensive against powerful anti-communist political leaders who felt it was not moving fast enough in the development of atomic weaponry under its first Chairman David Lillienthal. This period culminated with the decision by the government to build the hydrogen bomb, overriding the majority of the AEC's Commissioners and its General Advisory Committee (GAC), and with the controversy surrounding the withdrawal of security clearance for Manhattan Project hero and former GAC Chairman, Dr. Robert Oppenheimer.

The second period up to the late 1950's coincided fairly closely with the tenure of Lewis Strauss as AEC Chairman. It was marked by a tension between the AEC and the Joint Committee over administrative vs. congressional power (especially since the Administration and Strauss were Republican and the Congress was Democratic). Tension was exacerbated by the controversy over fallout from atmospheric testing--which resulted in considerable embarrassment of the AEC and some decline in its prestige.

The third period marked the beginning of the "love affair" between the JCAE and the AEC.⁸ It began, perhaps, when the JCAE pressured President Kennedy into appointing its staff director, James Ramey, to the Commission in 1961. In addition, scientists began to play a dominant leadership role under the chairmanship of Glenn Seaborg. Following the 1963 Atmospheric Nuclear Test-ban Treaty, the fallout controversy subsided and the AEC was able to continue its work with growing expenditures under relative peace and

tranquility. The period ended in the late 1960's with the AEC under increasing attack--accused of various unfair practices, and of continued failure to take adequate safety measures in uranium mines, in towns located near nuclear plant waste products, and in control over operations of power plants licensed by the AEC. This latter issue has expanded in the 1970's with the proliferation of "interveners"--groups prepared to contest the methods and decisions of the AEC (and its successors) in public hearings, before the courts, in referenda, etc., resulting in some reform of AEC practices under James Schlesinger and then Dixy Lee Ray. The inability of the AEC to satisfy its critics with these reforms led to its demise in 1975.

One interesting measure of AEC political strength is the Natchez and Bupp analysis which treats the agency's comparative success in winning budgetary allocation for the years 1958 to 1972.⁹ While the AEC was moderately successful in winning budgetary requests emanating from its 23 divisions, there were noticeable fluctuations during this period. However, they did show a clear downward trend. In 1958 the final total was actually higher than the General Manager's request; in 1972 the final total was cut by \$440 million from division requests and this amounted to a constant dollar figure of almost \$100 million less than the 1958 total, a trend which appears to reflect the decline in its political prestige and support.¹⁰

Having briefly outlined AEC evolution by drawing attention to major changes or controversies which marked each period, we now proceed to describe in greater detail the main events surrounding the more significant criticisms and issues. We then turn to consideration of some AEC methods used when faced with opposition, for these were themselves often subject to some of the heaviest criticism.

The most serious controversy, and the one to shake public confidence in the AEC most, probably has been the issue of weapons testing. In its first period, the AEC leadership had been fearful of nuclear proliferation and was reluctant to spur rapid development of nuclear weapons. This resulted in controversy over the hydrogen bomb and in the Oppenheimer case where the AEC was overruled. Yet even during this period problems occurred which presaged more serious challenges. In 1946 a miscalculated nuclear detonation forced evacuation of the Bikini atoll in the Pacific for 20 years.

By 1954, the AEC under Strauss became the major champion of rapid atomic weapons testing and development, and a source of discouragement for moves toward nuclear disarmament.¹² In that same year, a nuclear accident exposed several hundred Marshall Island natives, 23 Japanese fishermen, and 28 American servicemen to great doses of radiation--causing serious illness.¹³ Tests continued in the Pacific but were gradually shifted in the next eight years to the Nevada desert Mercury test site at Yucca Flats. This change briefly dampened criticism as the AEC assured Americans that the radiation emitted was harmless, due to its dispersal all over the globe. When soon challenged on this point, AEC spokesmen tended to ridicule any opposite contention and then play down its implications as long as possible.

This challenge began around 1956 when the veil of secrecy was lifted from information on radioactive fallout, and was led by non-AEC scientists such as Ralph Lapp. By the early 1960's the public was becoming aroused about dangers involved in the Nevada tests, for good reasons were emerging to believe AEC spokesmen had been dangerously

mistaken in their assurances. For example, the AEC was found to have been misinformed on the dangers of two of the most radioactive substances produced by the bombs, strontium 90 and iodine 131. Still, it maintained a reassuring position, and, as we shall see, delayed the publication and tried to attack the credibility of the Knapp report which demonstrated the potentially lethal effects of iodine 131.¹⁴ The awakened fear of fallout dangers provided much of the motivation behind the 1963 Atmospheric Nuclear Test-ban Treaty. President Johnson expressed the new attitude when he said in 1964:

The deadly products of atomic explosions were poisoning our soil and our food and the milk our children drank and the air we all breathe.... Radioactive poisons were beginning to threaten the safety of people throughout the world. They were a growing menace to the health of every unborn child.¹⁵

With the beginning of underground testing, the fallout controversy subsided. Still there have been certain questions surrounding underground testing. One concerns the fact that in Nevada in 1961 and again in 1963 "venting" occurred; that is the underground explosions released plutonium, in one case, exposing workers at a nearby asphalt plant to large doses of radiation.¹⁶ More recently controversy has surrounded the testing in the Northern Pacific island of Amchitka resulting in the "Pitzer case" discussed below.

The AEC never quite recovered its prestige during this period, but it did win an almost unshakeable ally in the Joint Committee on Atomic Energy. Future controversies throughout the 1960's would almost invariably find the JCAE firmly on the AEC's side, defending it from outside questioning rather than, as it had to some extent in the fallout controversy, acting as a forum for such questioning to take place.

A second area of controversy concerns the exposure of American uranium miners to cancer-producing radon (radium emanation) gas particles, and the consequent question of whether the AEC acted responsibly in allowing miners to take such risks. It is now clear that the miners' general case is beyond rebuttal. As Charles C. Johnson, Jr., the Administrator of the Consumer Protection and Environmental Health Agency of the U.S. Public Health Service said in 1969, "of the six thousand men who have been uranium miners, an estimated six hundred to eleven hundred will die of lung cancer within the next twenty years because of radiation exposure on the job."¹⁷ The miners, it appears, were not told of the mortal danger of radon particles in the mines; the mine owners were not directly informed by the AEC. They produced the uranium as requested by their sole client, the AEC. Some AEC scientists must certainly have known of the danger, because the results of German studies which confirmed the lethal nature of particles in uranium mines were published and well documented; and they should have known as well that the Czechs had solved the problem by ventilation at three percent additional cost. In response to workers' complaints, the AEC in 1959 pronounced that the link between uranium mining and cancer was not proven and that further study was needed. When soon challenged on this statement's validity, the Commission took the questionable position that regulation of safety standards within the mines was outside its domain, and was a responsibility of the states' departments of health.¹⁸ However, the AEC appears to have been particularly negligent in supplying the states with information on which to base safety regulations. During this entire controversy, the JCAE firmly supported the AEC, even to the extent that some of its members attacked scientists and union

representatives who disputed the AEC position. The result was a delay in governmental action to bring in new safety regulations for uranium mines. These regulations were not enacted until July 1, 1971, though by that time their impact could only have been quite limited because little uranium has been mined in the U.S. since the mid-1960's as the AEC had by then stockpiled many years' uranium requirements.

The AEC has been frequently criticized for its record in occupational health generally. The criticism here is often based on speculation rather than fact, partly because the AEC has been unwilling to engage in or fund research into the long-term health of those individuals suspected to have been subjected to radiation in one form or another.¹⁹ One of the few cases to come to light is that of

Edward J. Gleason. Roger Rappoport Reports:

On January 4, 1963, at the request of the AEC, the jug was dispatched to the agency's Brookhaven Laboratory for disposal. The jug, capped with a loosely-fitted rubber stopper and packed in a wooden crate, arrived for transfer to Brookhaven at the Eazor Express Corporation Trucking Terminal in Jersey City on January 8, 1963. It was Gleason, a dock worker for Eazor, who noticed that the box was leaking. He was used to handling leaky shipments, so he simply tilted the box onto a handcart and took it to the loading dock. When the box continued to leak, forming a puddle, Gleason turned it over....

A.E.C. officials finally discovered the leak and became alarmed. The shipment, containing dangerous radioactive material, had not been transported properly. Retracing the route of the shipment, the A.E.C. spent \$25,000 decontaminating trucks, truck trailers and terminals. Details of the cleanup made all the newspapers, and ten days after handling the shipment, Gleason read about it in the Newark News. He telephoned the A.E.C....

The A.E.C. promptly interviewed Gleason. He said that at no time did he see a radiation warning label on the box and he had no knowledge that it contained radioactive material....

Three years after the accident Gleason developed cancer

and in 1968 doctors were forced to amputate his left hand, arm and part of his shoulder yet his condition worsened steadily. Gleason filed suit in the Newark Federal Court of Judge Anthony T. Augelli, asking for 2 million dollars' damages....

In December 1970 Judge Augelli dismissed the case on a motion from the defendants.²⁰

Rappoport suggests that the AEC's carelessness about radiation can be traced to the Manhattan project where similar carelessness resulted in the deaths of at least two scientists, Harry Daghljan and Louis Slotin.

A more current controversy concerns wastes produced in the various stages of atomic power production. The subject is highly complex, involving various types of radioactive wastes and their leakage into the ground, air and water. There are many examples, such as the case of the "tailings" under the town of Grand Junction, Colorado which is located adjacent to the AEC-licensed Climax Uranium Company. In 1970 it was discovered and made public by the Colorado Department of Health that the people of the town were living directly atop great piles of radium tailings, creating a cancer risk especially great for children. The radioactive wastes had been left in piles near the mill and were used in many forms of construction in the town including "children's sandboxes."²¹ As in the case of the uranium miners, the AEC discounted the fears expressed, claiming there was no evidence of danger to the townspeople. It appears though to have blocked attempts to conduct testing necessary to provide the information.²² Nevertheless, Colorado State authorities finally did carry out the necessary research and discovered dangerous levels of radon gas in thousands of houses.

Questions were also raised when the AEC announced it had

discovered the solution to radioactive wastes. It planned to bury them deep within a salt bed below the ground near Lyons, Kansas. The idea was eventually scuttled because unexpected technical difficulties arose in relation to possible seepage, but here too the record shows that the AEC responded to outside challenges by attempting to play down the problems and by questioning the competence of critical Kansas Geological Survey members.²³ One outside expert succinctly stated the seriousness of the nuclear waste problem in 1975. David Dinsmore Comey, Environmental Director of Business and Professional People for the Public Interest, said: "We would have to have fail-safe storage for lethal radioactive waste--with no social instability or earthquakes--for the next 5,000,000 years."²⁴

Similar questions have been raised continually about the safety of the hundreds of reactors the AEC has licensed to be built in the U.S. Since the late 1960's, groups of "interveners" have challenged specific license applications to the AEC on many grounds, including the safety of particular reactors and procedures used by the AEC as both promoter and regulator of atomic energy. These interventions raised such serious questions about the safety of nuclear power installations that Congress and the President finally agreed the AEC's dual role was untenable. In addition, we have seen indications in the past year of a general slowing of the development of atomic power.²⁵

Perhaps the incident which set off this apprehension more than any other, was a fire on May 11, 1969, in the Rocky Flats plant outside Denver--operated by Dow Chemical for the AEC. The fire caused many millions of dollars in damage, and aroused considerable anxiety, particularly in Denver. The AEC at first maintained there was no danger to health whatsoever, but could not refute findings based on

soil samples by local scientists that the leaked radiation could in the long run cause up to 2,000 cancer deaths in the Denver area. The AEC did admit one year later that the surrounding public had been quite fortunate because the fire could have easily caused a massive leak of lethal radiation.²⁶ Furthermore, Dr. Edward Martell's independent study of one soil in the Rocky Flats-Denver area showed that some of the soil's plutonium had been there prior to the 1969 fire. It then was discovered that Dow-AEC had been burying wastes in the late 1950's on the last site itself in total contravention of safety standards.²⁷ Rappoport angrily concluded: "It seems that Denver needs an atmospheric-test-ban treaty of its own with Rocky Flats."²⁸

The issue of reactor safety is a complex one and remains highly controversial. In 1974 nuclear plants experienced 1,400 "abnormal occurrences," including four the government considered serious and one, at a reactor in Zion, Illinois, in which a small amount of radioactive material escaped. Another release of this kind was reported in January, 1975 at a Maryland plant.²⁹ The most serious danger seems to be a loss of coolant accident, and it appears that the back-up protection, the "emergency core cooling system," is to be regarded as suspect. This system apparently failed to operate when tested in 1974, and repeated warnings to this effect came from some of the AEC researchers involved.³⁰ The AEC reaction, (as in the case of Dr. Morris Rosen, discussed below) is said to have been to punish and thereby attempt to silence such criticism. Hence suspicion and fears remained. The AEC gave assurances that the chances of the back-up system ever being required at all are infinitesimal. However, many people, on what seem legitimate historical grounds, were beginning to doubt the veracity of such assurances.³¹ This doubt was underscored

in the closing months of 1974: the vulnerability of the safety system was demonstrated when discovery of cracks in the emergency cooling apparatus of one giant generator in Illinois forced the temporary shutdown of 23 others from coast to coast.³² It was underscored even more dramatically on March 22nd, 1975 when fire broke out at the Brown's Ferry nuclear plant operated by the TVA and was finally put out after seven terrible hours. It was only by good fortune that no radiation escaped since "at least seven of the plant's twelve safety systems had failed."³³ At about the same time the Reader's Digest Press published We Almost Lost Detroit by John G. Fuller recounting the steps that led to a partial melt-down of fuel, though no release of radiation, at the Fermi atomic power plant.³⁴ In early 1976, four highly publicised resignations rocked the nuclear power industry first in California and then in New York. Below are excerpts from two front page news stories:

1) SAN JOSE, Calif. - Three high-level General Electric engineers with 47 years of experience in designing and building nuclear reactors resigned yesterday because they said, the risks of nuclear power are too great....

The men said they were concerned with the adequacy of plant designs, with the threat of human error along the complex chain of the nuclear fuel cycle and with the certainty that nuclear regulation is ineffective....

Nuclear power has become a 'technological monster' and it is not clear who, if anyone is in control, one engineer said....

The three GE employees are:

. Dale Bridenbaugh, 44, a project manager responsible for systems to measure the performance of GE's reactors on special assignment to evaluate the safety of the primary containment shell that houses the reactors in 25 power plants.

Bridenbaugh has been involved with nuclear power plants since 1958.

. Richard Hubbard, 38, manager of the quality assurance section in GE's nuclear energy control and instrumentation department. His job is to evaluate control room and plant instrumentation, as well as reactor vessel parts and fuel handling and service tools. He began working on GE's nuclear program in 1964.

. Gregory Minor, 38, a manager of advanced control and instrumentation. He is responsible for the design of safety systems, control systems and control room layouts for GE.

He was working in the nuclear area for GE since 1960 and has been responsible for the design of protection, containment and other reactor control systems.³⁵

2) The Federal safety engineer for the nuclear reactors at Indian Point, resigned yesterday, charging that they were unsafe in design and construction and could cause a catastrophe killing thousands of people by radiation.

'If I had the authority, I would close down Indian Point Plant No. 2 at once-it's almost an accident waiting to happen,' the engineer, Robert D. Pollard, said in announcing his resignation as project manager for the United States Nuclear Regulatory Commission, formerly known as the Atomic Energy Commission....

Declaring that he could not, in conscience, remain silent about the perils associated with the United States nuclear-power program, Mr. Pollard said the two plants at Indian Point illustrated his concerns over safety.

'I believe that the Indian Point nuclear power station constitutes an unconscionable threat to the health and safety of the millions of people who live in the metropolitan New York area,' he said adding: 'The Indian Point plants have been badly designed and constructed and are susceptible to accidents that could cause large-scale loss of life and other radiation injuries, such as cancer and birth defects.

'The magnitude of the hazards associated with these plants has been suppressed by the Government because the release of such information might cause great public opposition to their operation.'³⁶

While the granting of licenses by the AEC to build nuclear reactors used to be a routine procedure, groups since the late 1960's challenged many such decisions on the premise that the AEC could not itself regulate in the public interest and safety an industry it

promoted. These critics charged unfair treatment before the AEC licensing boards³⁷ and the JCAE³⁸ in contrast to that of nuclear power industry representatives. Even after the reforms of the early 1970's, critics noted procedures the AEC still was placing upon itself in estimating probable safety of proposed nuclear reactors before issuing construction permits. When an application was received, staff and independent expert studies of "maximum credible accident" and "hazards analysis" were prepared and a hearing always took place. There were, however, two procedural obstacles to assurance of scientific protection of the public interest. First: even though the examiner in such proceedings had no technical training on problems of reactor safety, he was forbidden to consult the technical staff when preparing his report. Second: the five commissioners who had responsibility for the final decision were not necessarily technically trained on problems of reactor safety, but they too were forbidden to consult the technical staff in making their decision.³⁹ The AEC head was thus effectively kept doubly isolated from its scientific body.

It has been charged that one of the reasons the AEC minimized the dangers especially in the plutonium-based "breeder" reactors was the continual influence of long-time AEC head Dr. Glenn Seaborg. As discoverer of this extremely toxic element, he appears to have set the tone for a strong attachment to plutonium use and perhaps for a consequent playing down of its dangers. Looking ahead from 1970 to 1990's, in a public address, Seaborg optimistically predicted U.S. GNP would quadruple, based on the availability of greatly increased electrical energy made possible basically by use of plutonium, "a vital element of the overall economic well-being of this country."⁴⁰ He further stated:

Let's begin with the 'impossible dream'—or is it 'impossible'? It is twenty-five years hence—July 16, 1995. In the parklike setting of a research center a group of international dignitaries gathers for the dedication of a new power plant. It is the prototype of a commercial fusion reactor, the first Fusion, Electrical, Material, and Sanitation (FEMAS) complex. Built through the cooperation of the World Energy Organization (WEO), a new agency of the United Nations, to be operated by an international team of scientists and engineers, the FEMAS (or the 'Lady Femas' as it is more affectionately known to the technical set) will prove the feasibility of using fusion as a new major source of power and for a variety of other applications.⁴¹

Two other areas of AEC activity merit treatment. One is weapons.

Here again the record of the AEC since the early 1950's is clear.

Former AEC Chairman Lillienthal has argued: "Realistically, the AEC essentially is not too different from any major technical contractor to the defense department in the area of missiles, say."⁴² However, the AEC did have considerably more financial and political muscle than any entrepreneurial contractor, with a result described by Don K. Price:

The great purpose of the Atomic Energy Act was to take the atom away from the soldiers and devote it to peaceful purposes. But the result ... has just been the opposite. For more than a decade every military staff planner, as he computed the alternative advantages and costs of using different types of weapons to accomplish any particular objective, was given a powerful incentive to choose nuclear weapons because they were free. That is to say, the warheads were provided not out of the budget of the military service, as were conventional weapons, but by the Atomic Energy Commission.⁴³

The other area, separate and perhaps less important but no less revealing, might be called "nuclear gadgetry." Some of these "gadgets" have succeeded. Most have failed, but few have justified the financial investment or the claims of their exponents. "Plowshare"—the use of atomic explosives for peaceful purposes—has been suggested as a means of digging harbors and canals, and freeing up natural gas deposits deep underground. To this day, however, no project has been found worth

the expense and risk involved. This is not for lack of trying on the AFC's part, but rather because no willing customer could be found.

There was a much heralded attempt to sell the idea of using atomic explosives to excavate a new canal through Central America. Many millions of dollars went into planning this and other such projects. Among projects fitting this description were: Project Chariot to dig a harbor in Northern Alaska; Project Carryall, to blast a mountain pass in California; the Tennessee Tombigbee Water Project to connect two rivers; and the Cape Kerauden Project for Northwestern Alaska. The problem was that rather than a public need being articulated and then the required technology being prepared and modified to meet it, the opposite occurred. The technology was developed at some, often great, expense, and then outlets for its use were sought and researched-- again at some expense.⁴⁴ Planned because they were technically feasible, all were scrapped as dangerous and expensive.⁴⁵

There was also a group of projects for nuclear propelled flying vehicles, all failures. For instance, "Pluto" was to have been a flying nuclear reactor designed to deliver atomic warheads at low altitude and high speed. Pluto was cancelled in 1964 after several years and hundreds of millions spent; with the signing of the 1963 Test Ban Treaty, it could never have been tested even if it could be built.⁴⁶ The ANP nuclear aircraft project has cost over \$1,500,000,000 yet remains stalled due to insurmountable technical difficulties in finding a material dense enough to shield its pilot from radiation, but not too heavy to allow flight--an obstacle predicted from the outset.⁴⁷ Even if such material were available, justification for flying something so dangerous would be hard to find. The nuclear rocket NERVA project cost a great deal in research and was making little

headway when in 1971 the National Aeronautics and Space Administration decided it was not interested. This however did not stop the project since it had many Congressional friends as well as the support of the AEC.⁴⁸ While Pluto was cancelled, these two projects apparently were still going on in the early 1970's (though it is difficult to be certain, since a common practice is to change the names of such projects at given intervals).

There have been many other planned and much publicized devices such as nuclear powered merchant ships, nuclear batteries, nuclear pacemakers and irradiated foods. In almost every case it was shown that the potential benefits of the gadgets could not possibly justify the costs and risks.⁴⁹ Nevertheless such projects were continued with the spending of vast sums of money due to active support from the AEC and the Joint Committee.

These, then, are some of the major controversies and criticisms surrounding actions of the Atomic Energy Commission. We make no claim to having provided any kind of total picture for our goal is not to describe and document the AEC's history; we are interested in discovering and describing certain characteristics of a key science related institution. Practically every observer (including AEC Chairmen from David Lillienthal to James Schlesinger who was appointed in 1971),⁵⁰ agrees that for all its achievements, there have clearly been problems and failings. We have emphasized some cases where the actions of the AEC have been questionable, and which we shall try to explain. Let us briefly note the kinds of actions for which the AEC has been criticized.

In the most global sense, first of all, the atom has not lived up to its advance billing. As David Lillienthal noted as early as

1963, the AEC failed to become an independent civilian agency, it soon no longer required its tight veil of secrecy since the atomic secret was "out," and (providing in 1975 just 7% of all U.S. electrical energy) it never created the promised "new world" of atomic plenty:

In sum, the three chief reasons for the establishment of the AEC in the late 'forties are no longer wholly valid in the world of the sixties.... However painful it may be to the pride and future hopes that have grown up within the AEC and the major industrial interests which depend upon it, the public is entitled to insist that the AEC be geared to the realities of today and not to the exploded promises of 1946.

The reality is that the Atom has not justified the separate and unique status which Congress understandably assigned it in 1946.

The atom has not been the single necessary weapon. The atom has not revolutionized industrial society. The atom has not produced revolutionary advances in medicine or industry.

The peaceful atom has not ushered in a 'New World' but has rather become a part, quite a minor part, of the old one.

And yet the official sponsor and trustee of the atom in this country, the Atomic Energy Commission, has hardly changed at all.⁵¹

Given these developments, the central problem might be described as the AEC's having failed to adjust. The AEC has been accused of using whatever means seemed necessary to promote development of atomic power with the following results:

- 1) A failure adequately to control nuclear weapons.
- 2) A huge investment into unnecessary nuclear technologies.
- 3) An assumption that its own organizational integrity and continuity as well as that of its clients were important goals per se and therefore to be defended from external criticism, no matter how justified that criticism.
- 4) A far too great emphasis on the promotion rather than regulation of atomic energy.

5) Most important, a failure to take fully into account dangers from fallout, particles, atomic wastes, reactor accidents and environmental pollution, which has resulted in unnecessary deaths and dangers to health.

More significant, for this analysis, is that criticisms levelled at its actions are mainly against the AEC's methods in carrying them out, especially insofar as such methods violate normative imperatives of science. Let us consider some examples of these which are related to or parallel with events described above. Most criticism centers around allegedly frequent AEC efforts to distort and manipulate information so as to reflect positively its own position, that of its clients, and that of atomic energy in general. Since these practices appear to have characterized the AEC at least from the mid-1950's to the early 1970's, we do not treat them chronologically but draw upon examples from throughout this period.

The most general manifestation of this problem is the manner in which the AEC made information available to the public.

The kind of information provided by the AEC to describe the tailings situation is a good example. The format of one information bulletin on the subject was that of questions and answers. The answers included these statements: "It was a reported practice to use uranium mill tailings;" "In theory, radon from this mill could diffuse into buildings;" "Technical evaluation of this situation is difficult since traces of radium exist in virtually all soils...."⁵² One effect of using such language, it seems, is to turn known facts into abstract, and hypothetical or theoretical statements; and, we suggest, to cast them in such a way as to encourage the idea that only the scientists really understand and are doing whatever must be done.

A tendency of the AEC to minimize fatalities caused by nuclear accidents has also been noted. For instance, the AEC admitted to only seven deaths from radiation causes among "workers," when Public Health Service figures listed at least 142 uranium miners known to have died from this cause.⁵³ Another example is seen in a statement from Ernest B. Tamm, director of the AEC's division of industrial participation, who told a seminar for newsmen in 1969 that nuclear reactors "have compiled a record of about 90 reactor-years of operating experience without exposure of any member of the public to harmful quantities of radiation."⁵⁴ Gene Bryerton comments:

Close examination of Tamm's statement reveals that he did not deny that accidents have occurred, or that radiation releases--both planned and unplanned--have taken place. Moreover, he restricted his remarks to commercial reactors, not relating that accidents at developmental and experimental reactors have killed some reactor personnel. Finally, if one accepts the AEC's standards on the amount of radiation which constitutes harmful quantities--which many geneticists and other authorities do not--Tamm's statement is a factual one.⁵⁵

Written information material specifically distributed by the AEC has been similarly criticized. For instance the following is an excerpt from an AEC pamphlet Nuclear Reactors distributed widely to school children and others. The main theme was "the Atom as friend and neighbor," by John F. Hogerton. According to Elizabeth Hogan and Richard Curtis, Hogerton devoted only three of forty-five pages to reactor safety and these presented a "highly idealized picture," to which they offered a rebuttal:

'In normal operation, the fission products are locked in the fuel by the fuel cladding, which is thus the plant's first line of defense against release of radioactivity!' (No mention is made of the fact that the technology of cladding metals and alloys is still rife with unknowns.) 'This material leaves the premises, so to speak, when spent (used) fuel elements

are removed from the reactor and shipped to a fuel reprocessing plant!' (No mention of the treacherous hazards of removing fuel elements from the core, or transporting them to the reprocessing facility through population centers, and of reprocessing them.) 'If any trace of the fission products escapes into the reactor coolant through defects in the fuel cladding, it is scavenged from the coolant by purification equipment, packaged and shipped to an AEC site for safe burial.' (No mention of the fact that scientists have still not learned how to capture all of the fission products released during reactor operation; no mention again of the transportation hazards; no mention of the unsolved problems of burial and storage of hot atomic wastes.)⁵⁶

A film Return to Bikini released by the AEC in 1965, illustrates this same tendency. It depicts the recovery of the Marshall Islands from nuclear bombing in the 1940's and 1950's, glossing over the forced evacuation of its residents with pictures and glowing verbal descriptions of their island paradise as visited by the scientists. Given its air of scientific thoroughness, the film's failure to mention the twenty natives who developed thyroid abnormalities, it is said, can be described only as distortion.⁵⁷

Other methods used by the AEC and its clients to sell nuclear power to the public have come under criticism. Take for instance a 1964 television program entitled "Man and the Atom" aired on National Educational Television and produced in close cooperation with the AEC. The show opened with a few testimonials from residents of Buchanan, New York near the location of consolidated Edison's Indian Point nuclear facility:

LOCAL MAN: I can look out of my windows of my house and actually see the smoke-stacks. I can hear the excess steam coming off.... Nothing can really happen to it the way they have it set up and built over there. The structure itself, and the devices they have for any kind of ... warning signals and all coming up; [show] that there's no reason for worry....

LOCAL WOMAN: I never hear anyone around here ever worry about the atomic plant. I think most people feel like I do. I think they think it's a wonderful thing....⁵⁸

Buchanan had good financial cause to praise the reactor, for as Ralph Blumenthal reported in the New York Times, consolidated Edison had paid a total in annual taxes roughly equal to the village's previous budget, and more was anticipated.⁵⁹

The AEC has been accused of gross distortion and deception.⁶⁰ The official History of the U.S. AEC 1939-1946 does not cover such vital bits of history as the deaths of Los Alamos scientists Harry Daglian and Louis Slotin.⁶¹ The voluminous AEC bibliography of "Books on Atomic Energy" omits works of authors critical of its policies; for example Ralph Lapp "gets a mention for only one of his 12 titles and that is a book aimed at high school students."⁶² Another example is the case of the Grand Junction's radium tailings where the AEC, in response to mounting criticism, asserted it had sent letters of warning to nine state health authorities in 1961. None of the nine states had any record of receiving the warnings.⁶³ Similarly, the AEC was accused by Senator Frank Church and others of suppressing in rather devious ways a National Academy of Science report critical of the entire waste disposal system.⁶⁴ Critics also claimed that a recurring tactic was for the AEC to give the appearance of changing its structures when challenged but invariably leaving the basic powers unchanged.⁶⁵

It has been suggested that one reason the AEC has been able to point to a lack of documented facts to support the arguments of its critics is the agency's consistent failure to sponsor or even permit research into the effects of radiation from testing, tailings, working in uranium mines, etc. The AEC was able to do this at first because

the great bulk of the federal money for atom-related research was under its control, and, secondly, because of its right of approval over agency-related studies by the Environmental Protection Agency (EPA), formerly the Public Health Service (PHS).⁶⁶ For example, the AEC has stated there is no documented evidence of contamination to the areas around the Nevada test site. This was true because the required research was not carried out, even though a PHS report in 1953 suggested this to be the case and called for just such testing.⁶⁷ The research priorities of the AEC must be suspect when we are told that the AEC and the Office of Civil Defense spent "over the past decade [1960's] ... over 86 million dollars to tell us the good side of nuclear war." This money supported research and publication of hypothetical studies of "postattack"—what proportion of the population, the economy, etc., is likely to survive nuclear attack, with scenarios of what is to follow.⁶⁸

The AEC did initiate studies of radiation effects, but usually in rebuttal to critical studies carried on outside. Yet such tactics have backfired. The classic cases here are those of AEC scientists Harold Knapp and Arthur Tamplin. In 1960 Knapp was asked to write a report on the level of radiation exposure from U.S. atmospheric testing to counter public criticisms voiced notably by Ralph Lapp. Knapp's report did somewhat dispute Lapp's contentions on the effects of Strontium 90, but his more important conclusions centered on the theretofore unrecognized effects of Iodine 131, a particularly toxic radioactive substance emitted by atmospheric tests which enter the human organism mainly through cow's milk. The AEC agreed to publish the report only together with and preceded by a review of it by a special committee of five. This group known as the Langham Committee

approved of Knapp's findings with the exception of one member--who had been responsible for determining safety levels in the first place. The AEC nevertheless made a point of saying the committee was not unanimous.⁶⁹ One result of such a tactic was that a scientist on the committee, John Gofman, decided to re-evaluate his own role and became a highly potent critic of nuclear safety practices a few years later. In 1971, Gofman and Arthur Tamplin published Poisoned Power, documenting the dangers of atomic reactors--especially "background radiation," that is the levels of radiation normally released by nuclear reactors.

The case of Dr. Tamplin is also instructive. Tamplin, employed at the AEC's Lawrence Livermore Laboratory, was given the task of refuting Dr. Ernest Greenglass' published calculations of deaths expected to result from nuclear fallout caused by atmospheric testing. Tamplin found Greenglass' statistical calculations to have been erroneous and that a better estimate of the deaths caused would be 4,000 rather than 40,000. Tamplin afterwards stated it was pressure from the AEC to delete any reference whatsoever to deaths attributable to fallout⁷⁰ which led him to reconsider the entire question of whether the public was protected from radiation. This, in turn, gave rise to a partially successful campaign launched by Gofman and Tamplin to bring about a tightening on the part of the government of permissible radiation standards. It also led to a cutting of Tamplin's staff from 12 to one and his budget from \$700,000 to \$300,000 with the loss of access to travel funds.⁷¹

Similarly, the transfer of Dr. Morris Rosen and most of his staff from the Systems Performance Branch of the AEC in February of 1972 allegedly for criticizing AEC safety procedures⁷² seemed to substantiate

the charge by various groups that there was a general rule operating within the AEC of "Never Disagree with Established Policy."⁷³ Another example is the 1972 cancellation of a study at Oak Ridge into the dangers of a loss of coolant accident in reactor fuel rods which was already two-thirds completed, a decision which caused disappointment and anger.⁷⁴ Although the official reason given was budgetary, a private statement made by a chief AEC administrator pointed to other factors. "You won't find this in writing, but the reason he (Andrew J. Pressesky, AEC assistant safety director) gave for cancelling the study was that it raised more questions than it solved."⁷⁵

The treatment accorded by the AEC to outside scientists who served it in an advisory capacity seems to have been little different. One incident concerns a series of underground tests conducted by the AEC in recent years under Amchitka Island off Alaska. The tests were criticized as potential threats to the environment by many groups but were carried on in spite of widespread opposition--so far without significant adverse ecological result. In order to evaluate the possible dangers of its planned Amchitka explosions, the AEC in 1968 appointed a panel of outsiders headed by atomic physicist Kenneth Pitzer. Pitzer had been associated with the AEC during the early 1950's and was one of the few scientists who testified against Oppenheimer at the Gray board hearings which examined the charges against him. The report of the Pitzer Committee was nevertheless critical of the chosen sites as potential earthquakes risks. The AEC's response to the report highlighted a few of its questionable methods and practices in dealing with criticism.

First the report was classified under AEC powers of executive privilege. Then members of the AEC staff were instructed to do their

own investigations. Four unsigned staff papers were produced, all in support of the test sites. At least one of the authors of these papers had his name removed because he was unwilling to accept the editorial changes made at various levels of bureaucracy.⁷⁶ The Pitzer panel report was finally published one year after its submission, together with and preceded by the four internal reports, just prior to "Milrow," the first Amchitka firing late in 1969.

The case of Dr. W.O. Pruitt, it is charged, demonstrates how far the AEC was prepared to go to attack outside critics. Dr. Pruitt worked at the University of Alaska in the late 1950's on an AEC grant. In his ecological study of the area, Pruitt found that the planned (and later scrapped) "Project Chariot," the goal of which was to use atomic explosives to dig a harbor in northern Alaska, would be detrimental to the wildlife and environment, and thus the life of the Eskimos. Pruitt was subsequently forced to leave the University of Alaska under charges, on the one side, that his report was unsatisfactory, and on the other, that the AEC had brought pressure on the university to force him to edit his findings. Pruitt then applied for an opening at the University of Montana but the biology department's unanimous recommendation to hire him was rejected by the administration. In explaining the rejection, a Dean stated he had received negative reports on Dr. Pruitt from Dr. John Wolfe of the AEC. The decision stood--an unprecedented event at the university.⁷⁷

Dubious methods apparently used by the AEC may be summed up at this point. Lest we be misinterpreted, it should be again made clear that the above is in no sense intended as a definitive statement on any particular issue let alone on the AEC as an institution. Controversy remains and we have not allotted "equal time to the defence."

The point is neither to indict nor to set the record straight on the agency; it is merely to set out some of the points surrounding the major controversies and criticisms which help account for its decline and ultimate demise. The methods for which it has been notably taken to task are:

- 1) The use of unnecessary secrecy against potentially "damaging" information in various ways, only a few of which are described above.⁷⁸
- 2) The application of pressure bordering on intimidation on scientists both inside and outside the AEC.
- 3) The attempt to discredit critics of its policies.
- 4) The distortion of information through use of one-sided arguments and quasi-scientific language.⁷⁹
- 5) The use of "propagandistic" promotional techniques.⁸⁰
- 6) The sponsorship of research with an aim toward vindication of AEC policy and decisions.⁸¹
- 7) A continual attempt before the courts and Congress to define its regulatory mandate in the narrowest possible manner.⁸²

To the extent that AEC scientists as commissioners, advisors, and employees did engage in or condone these practices, they violated all traditional norms of science: emotional neutrality, universality, communalism, rationality, organized skepticism and individual freedom. Yet the actions or inactions of an individual cannot be separated from the wider context as well as the organizational procedures which gave rise to them. Any suggestion that an explanation may lie in the failure of individual scientists, from Seaborg down, to live up to the norms of science in the fulfillment of their roles is inadequate. Other scientists in the same situation may be expected to have acted

similarly. It would be no more valid simply to "blame" individuals in this case than to "blame" those of Milgram's subjects who obeyed. We can, however, conclude that the normative imperatives of science provide no guarantees in such situations, a confirmation of expectations raised in Chapters Three and Four.

The relationship of Milgram's experimenter to his subject was a pure but contrived and uncommon manifestation of scientific authority. The effects of the above-described practices by the AEC upon people it affected are analytically less pure but far more important in their consequences. Is the particular place of science and scientific authority a factor here? We may now seek to address this question.

The current series of disclosures into the nefarious dealings of the CIA, as well as recent attacks by Ralph Nader and others⁸³ upon practices of various regulatory commissions apart from (though including) the AEC, might lead us to suppose that the weaknesses of the AEC were not related to its specific scientific make-up, but rather are endemic to U.S. regulatory agencies. We suggest that while the AEC was by no means immune to the typical weaknesses, this is not all there is to be said. Let us consider two major critical factors associated with such agencies.

First is the charge that such agencies defend their own institutional integrity often at the cost of service to the public. We have seen this to be true of the AEC, but also trite. For our purposes, what matters is the means at its disposal to this end which is the result of the nature of its activities and the context of its operations. Second, such agencies have often been accused of confusing the interests of the public with special interests of their clients. This second criticism can not be wholly applicable to the AEC because only in the

early 1960's, with the entry of General Electric and Westinghouse into the nuclear energy business, did the AEC begin to have anything resembling an important special private-interest clientele. We suggest that the entry of these powerful special interests may, if anything, help account more for the JCAE's attitude change in the early 1960's which amounted to the Committee's abandonment of its "watchdog" role. Many critics have seen this as having encouraged the AEC to serve displaced goals.

The peculiar relationship of the AEC with the JCAE, (coupled with the AEC's extraordinarily broad mandate) have been often seen as constituting the source of these problems. Green and Rosenthal have commented:

The Joint Committee on Atomic Energy (JCAE), since its creation in 1946, has played an unusually influential role in the conduct of the nation's atomic enterprise. Although originally a part of the legislature, the Joint Committee's behavior uniquely resembles that of the higher echelons of the executive branch....

Members of the Committee itself, other congressmen, and officials of the executive branch have long recognized the JCAE as an extraordinary Congressional institution as the only joint committee with legislative powers, having broad statutory authority, and an unprecedented role in the formulation of national policy.⁸⁴

We have previously noted the AEC's unusual mandate. Sheldon Novick makes this point well and relates it to the position of the JCAE:

The AEC has broader powers than have ever been given to a single federal agency. Among other things, the Atomic Energy Act of 1954 apparently gives the AEC power (according to its own contention) to impose security restrictions on any activities, whether conducted with or without government support, which bear on atomic energy, however tangentially.

With sole authority in its area of activity, extremely broad powers, and with much of its activity cloaked by

security restrictions on information, the AEC is a singularly difficult body to influence. It is subject only to the restraints of the Joint Committee, designated by law its 'watchdog.' Yet over the years, the two bodies have drawn together until they are almost indistinguishable. There has been considerable exchange of staff between the two bodies.... Although the senior members of the Joint Committee have been active in atomic energy affairs for twenty years or more, the Commissioners generally change with each Administration, or, in most cases, even more frequently. As a result, the Joint Committee dominates the AEC; at least in terms of the civilian reactor program, the two bodies have become one.⁸⁵

This JCAE partnership with AEC, no doubt encouraged information control and probably exacerbated the problem of excessive secrecy. The fact that U.S. atomic energy was originally developed as a military weapon also helps to account for the much commented-upon AEC obsession with secrecy. Hans Morgenthau stated bluntly:

The Atomic Energy Commission has become a state within the state, surrounded by a special wall of secrecy. Even government officials who have top secret clearance, giving them access to our political and military secrets, need a special clearance to penetrate the secrets of the Atomic Energy Commission. Even then, most of them will not be able to learn all there is to be known.⁸⁶

Clearly this secrecy is closely related to the other problems of the AEC, adding an element beyond those of most other government agencies. Conceived in this manner, the failings of the AEC are those of the so-called military industrial complex⁸⁷ in especially acute form.

Yet, especially insofar as the AEC control of information is concerned, the scientific element cannot be discounted. Morgenthau argues that military secrecy complements scientific secrecy in the AEC, a combination so powerful as to threaten the possibility of democratic decision making.

This naturally secretive character of scientific knowledge, removing it from both executive understanding and democratic control, is accentuated

by the artificial barriers of secrecy with which politically and militarily relevant scientific knowledge has been surrounded. Since scientific knowledge is esoteric and gives those who possess it a monopoly of power, the perpetuation of its esoteric nature, through prohibition of its dissemination, so it is argued, will perpetuate the monopoly of power. Thus as soon as we found ourselves in the possession of the nuclear bomb, we took elaborate precautions to protect its secret....

The function of that intragovernmental secrecy, whatever the intentions of its initiators and administrators might be, cannot be the prevention of unauthorized disclosure of state secrets; for if it were, it would cast doubt upon the reliability of the great mass of government officials who, while having been cleared for top secret information in general, are precluded from access to special 'secrets.' Rather, the function of this special secrecy is political. By protecting esoteric knowledge, it protects and enhances power. As we have seen, the possession of esoteric knowledge is a potent weapon in the struggle for political power.⁸⁸

The psychological link between esoteric scientific knowledge and political/military goals was nowhere stronger than in the AEC's establishment and early-developed sense of mission. Lillienthal, looking back on the late 1940's and 1950's blames scientists in large part for this:

Scientists and technical men have played a leading part in fathering and promoting the idea that there is some single solution to the threat of nuclear war. Increasing (though belated) recognition of the basic defect of this approach has injured public confidence in scientists, and hence in science; it has shaken scientists' confidence in themselves.

Scientists have--unwittingly, perhaps, at first--been in part responsible for diluting the spirit of independent inquiry that is the very core of science, by becoming uncritical advocates and even lobbyists for various huge 'programmatic' technical enterprises, of which atomic energy and the space program are but two instances.⁸⁹

One concrete result of this combination of factors relates to the notion of limits or boundaries introduced earlier. Assumptions are

made by scientists and others alike placing upon scientists, and specifically scientific institutions such as the AEC, responsibilities for decisions they are no more qualified to make than anyone else, but thereby eliminating possible checks upon abuses found in more democratic forms of decision making. For example Harold Green has argued that any resolution of the question of safety standards would not come by creating another scientific agency or altering the JCAE to oversee the AEC. He sees the problem ultimately as lying in scientists' world-view--which makes it inappropriate for scientists to determine and enforce criteria for public safety:

In my view, the problems ... would exist even if the AEC regulatory and licensing program were completely separated and transferred to a wholly independent agency. The real vice as I see it lies in the assumption that scientists are omniscient and possess almost infinite capacity to solve problems and permanently to fix leaky faucets. Our society has permitted these experts to play God: to assess benefits, to define risks, and to determine what risks the public must assume, cheerfully, just as it pays taxes in exchange for the benefits which the experts think the public should have. What's more, [there is] the carefully nurtured myth that judgements as to nuclear safety can be soundly made ... [on] these risk-benefit questions largely behind closed doors and in the esoteric obfuscatory jargon of their disciplines.

At the present time, the issue of 'reasonable assurance of no undue risk' is treated as a scientific or engineering question to be decided by experts. Experts, however, have no special competence to deal with the human values which enter into the risk/benefit calculus. It is necessary to convert the issue of nuclear power safety into the sociopolitical question which it in fact is, so that the public can play a larger role in determining what benefits it wants and what risks it is prepared to assume to obtain these benefits.⁹⁰

A paper entitled "The Integrity of Science," prepared for the American Association for the Advancement of Science, showed that some scientists also share this apprehension:

The growing interaction between science and public

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policy requires considerable attention to the problem of distinguishing scientific problems from those issues which ought to be decided by social processes. An example of the tendency to confuse scientific evaluation with social judgment is the matter of radiation standards. Here, a scientific body, the Federal Radiation Council, is engaged in setting standards of acceptability which are basically social judgments regarding the balance between the hazards and benefits of nuclear operations. These judgments are, or ought to be, wholly vulnerable to political debate, but their appearance in the guise of a scientific decision may shield them from such scrutiny.⁹¹

These arguments are fully consonant with concerns expressed above relating to the role of scientists in setting boundaries in which to operate. They also suggest that power, influence and authority may have been inherent in both the scientific nature of the mandate given the Atomic Energy Commission and in the context of the social and cultural position of U.S. science--particularly atomic science--after World War II. The AEC was a scientific institution in that most of its top and middle level employees were scientists, and that its work was defined by the outside world as science (and technology). The AEC was no doubt understood from within some scientific circles as a political as well as a scientific institution and therefore not extensively subject to scientific norms and procedures since these might be superseded by political considerations--as was frequently the case.

Nevertheless, we know that scientists, some of very high prestige, in great numbers came to work for the AEC directly or indirectly or to serve it in advisory capacities and accepted its grants and jobs with little hesitation. The motivations were no doubt varied, some patriotic, some altruistic, some careerist, some scientific. While a growing number of scientists, mainly outsiders, were disturbed by AEC

behavior, few considered its profound implications as to the changing nature of science operating in this new context. There were two sets of norms and expectations in operation. From the vantage point of the society outside the scientific community, the AEC was seen as a scientific, perhaps the scientific institution, which promised to unlock the atom and provide the nation with the military and peacetime wonders of science--a vantage point at odds with the perspectives of most scientists.

Theoretically, it might have been possible for scientific norms to predominate as far as the procedures if not the goals of the AEC were concerned. One might have expected the AEC to be characterized by the universality and openness attributed to academic science. This was not the case as we have seen and as the Oppenheimer trial so well symbolized; and this is quite understandable in light of the sociological explanation of the norms of science--as not inherent in the individual activities and training of scientists, but rather as constituting imperatives only for the operation of the social system of science itself. Given then that scientists entered a new social system with new rewards when they joined the AEC, it should come as no surprise that the norms of science did not hold up very well.⁹² This, it would appear to us, is to be expected as well of other non-academic scientific institutions though perhaps in a less obvious or identifiable manner. As has been argued above, this is the direction in which the role of science is moving in the contemporary U.S. The unfolding roles of the AEC's successor agencies should prove quite instructive in this regard. Our preliminary review showed little change. Since these were created, criticism has seemingly intensified.

In the case of the AEC the result of scientific norm failure

taken to the extreme becomes apparent, and this is a point worth noting. Not only were scientific norms insufficient to inhibit "unscientific behavior" in an agency where scientists were predominant, but this very unscientific behavior paradoxically could be and was justified and rationalized through continued appeals to science. Features of bureaucratic modes of operation, such as a reflexive defensiveness against any form of attack on the institution, could be and were effectively rationalized on "scientific" grounds.

It seems to us clear that just as science provides a crucial explanatory element in understanding behavior in Milgram's laboratory, so it provides a similar element in understanding criticisms of the AEC. This point is hinted at in a 1972 article by M.S. Larsen:

As in the justification of the Amchitka blast, this ideology is used to legitimize irrational decisions by giving them an air of functional rationality: because scientific and technical knowledge is unevenly distributed, the technocratic elites can use their claim to expertise and, above all, their 'secret knowledge' to minimize the uncertainty and the risks involved in a political decision. The Atomic Energy Commission could use its authority to justify the nuclear blast and minimize its danger because, first, the public had no access to the information allegedly available to the AEC and the President, and, second, the public at large accepts scientific authority as the ultimate guarantee of rationality.⁹³

It is not surprising, therefore, that in most instances exposure and criticism of the AEC's methods and faults should have been initiated by scientists such as Lapp, Pauling, Templin, Gofman and Commoner, for it is they who are least subject to misperception as to the bounds of scientific knowledge. On the other hand, the attitudes of elected and appointed public officials during this period, as suggested in these brief quotations, may have made them particularly vulnerable to the kind of misperceptions often noted as central to the failings of the AEC:

In a legislature that is supposed to distrust egg-heads, a Congressman often wants his advice on a specific program undiluted by either party doctrine or the policy views of general administrators; he is so conditioned to go directly to the scientific expert whenever he can that he sometimes treats his witnesses as experts even when they are not.⁹⁴

Scientists, whether they think so or not, still have a favored status now in the emotions of public administrators. They walk into a room where lay administrators still stand in awe of imagined mystery. That myth of science, the picture of the infallible specialist in immutable fact, lingers on in the minds of the administrators.⁹⁵

In relation to the AEC, such thinking is perhaps best exemplified not by AEC scientists who must have often thought askance (though they seldom spoke out) but rather among non-scientist commissioners and leading members of the JCAE. Simplified, the logic may have gone as follows:

-Science produced the bomb.

-Science is our present hope and future salvation.

-The unlocking of the atom is today's great scientific mission waiting to be accomplished.

-The AEC is made up of scientists dedicatedly in pursuit of this great quest.

-What is good for the AEC is good for us all, for the AEC--a pure institution with this one great goal--must bear scientific fruit in the long run.

-The long run consists of many short runs and is the cumulation of them.

-Those who attack the AEC, or even question it in short run terms, are to be distrusted.

Non-scientists did best express uncritical advocacy--and this was to be expected, given that they were least acquainted with the

etiquette of science.⁹⁶ One important example of such thinking can be found in the person of James T. Ramey, former chief of staff for the JCAE and an AEC Commissioner from 1961, making him the most senior of the commissioners.

In October of 1969, a symposium with the title "Nuclear Power and the Public" held at the University of Minnesota was marked by attendance of quite a few noted experts both from within and outside the AEC, and by great public interest.⁹⁷ Along with the many scientists who addressed the assembly were also commissioner James T. Ramey and Congressman Craig Hosmer (who had left an AEC administrative job in 1960 to run for Congress and who, upon being elected, was appointed to the JCAE where he had risen to the position of ranking minority member). Both demonstrated their unshakable conviction in the need for and desirability of nuclear power. For example, Hosmer insisted:

The requirements for [nuclear energy] doubled in less than every ten years. We cannot consider nuclear power in a vacuum. They are part of the electrical energy supply problem. If there are risks and benefits in nuclear power, there are risks and benefits in conventionally generated power, and the same for going without power....

It's a matter of having to accommodate to the world in which we live. We need to produce electricity. The chances of damage to any individual in society are so low that there would be fewer people hurt by them than there would be if the same amount of electricity was being produced by a coal plant, powering pollutants into the air. But society has forced upon us many, many risks. Crossing the street is a risk. You cannot live in our society without moving around.⁹⁸

However, as Harold Green pointed out, the argument of a "need" existing for atomic energy per se, is "an accident of history--the accident of World War II, in which the atomic bomb was developed--the historical fact that Congress created the AEC, and, even more important than that, the fact that Congress created a Joint Committee on Atomic Energy."⁹⁹

Ramey's attitude toward some of the AEC's scientific critics was extremely scornful:

I would just like to comment again that Dr. Commoner is being awfully moral in relation to the Atomic Energy Commission. I should point out, though, that he is out on a limb on Iodine 131. His data are wrong and he is making false assumptions and interpretations which it would be easy to check.¹⁰⁰

Dr. Hubbert is entitled to his views. He has been trained in geology, and knows a great deal about natural resources, but he is not an expert in nuclear power.¹⁰¹

This attitude is similar to that taken frequently by some of the JCAE committeemen during hearings on some of the controversial issues discussed above.¹⁰² Metzger noted the following conversation from a discussion during a JCAE public hearing between Chairman Hollifield and Commissioner Ramey, shortly after the Minnesota symposium, which highlights the manner in which these non-scientists denied scientific credibility to critics of the AEC:

'At each of these meetings that I have gone through--the Vermont and Minnesota ones--there are some professional "stirrer-uppers" involved....'

'That is a good name, "stirrer-uppers,"' Chairman Hollifield agreed.

Commissioner Ramey continued, '... gentlemen from some paper conservation organizations that you can hardly find an address for, to persons associated with the coal industry....' The commissioner has long been convinced that 'coal dust' is involved in opposition to nuclear plants, since nuclear heat is used to generate steam rather than coal heat. Early on in the game he was right, since the United Mine Workers at first strongly opposed the construction of nuclear plants. But he sees other devils, too.

'Second,' he continued, 'there is a group of younger scientists, some of whom might be a little bit on the extremist side, who seem to always be talking on matters beyond their own professional competence. They discuss and comment on areas in which they have not performed their scientific work.'

'Well, we have a certain number of book writers too, of sensational books,' Chairman Hollifield offered.

'They are usually journalists and public-relations men,' the commissioner said.

'That is right,' Chairman Hollifield agreed, 'with no scientific background or competence.'

'None whatsoever,' Ramey answered, 'that is right.'¹⁰³

It appears thus, at least in regard to the attitudes of certain JCAE members and AEC commissioners, that the agency's actions and decisions were set in the context of the logic described above: critical challenges including those of well-known scientists attempting to use scientific standards, were dismissed as "unscientific" even by non-scientists. Two of Milgram's variations present an interesting parallel here. In one, it was the scientist-experimenter who submitted to the shocks and it was the confederate who told the subject to continue. In the second, the subject encountered two experimenter-scientists, one of whom became the victim. In both cases the scientist-victim soon pleaded for the experiment to stop--saying it was not acceptable, that no experiment should be run this way, etc. Milgram found disobedience became the rule only in the first variation. The failure to disobey in the second may be readily compared to the attitude of Ramey and Hollifield toward anti-AEC scientists.

The Milgram experiments were portrayed in Chapter Four as concretely manifesting a relationship we have termed scientific authority. The several instances where we have drawn analogies between this relationship and aspects of the activities of the AEC suggest that scientific authority may be at play here as well. Unlike the "pure" relationship found in the experiments, the relationships of the AEC to those affected by it may not be described as scientific authority; rather, this concept provides an added and necessary explanatory dimension.

To see this, let us first pose what appear to be central questions

remaining to be answered in an analysis of the controversies concerning the AEC. We have seen that the AEC, because of the sense of mission surrounding the scientific and military possibilities of the atom after World War II, was accorded great capabilities which it was able and willing to use for manipulation of information in order to enhance its own position, that of its clients, and that of atomic energy in general. The question does not concern willingness to do so, since such efforts are frequently reported by students of powerful executive agencies, but involves rather how the AEC was able to do so. The answer thus lies most directly in AEC ability to inhibit possible opposition, sources of which could be any of the following:

- 1) Opposition from within its own staff, especially from scientists employed by it where AEC actions and methods violated the norms of science;
- 2) Opposition from the Joint Committee which possessed the mandate and the powers to challenge AEC actions and decisions effectively;
- 3) Opposition from the outside involving one or a combination of:
 - a) federal, state, and local elected and appointed officials,
 - b) oppositionist scientists and other outspoken critics, and/or
 - c) aroused public opinion.

The conceptual question before us is to find the appropriate terminology to describe and explain the relationship between the AEC and these various groups and individuals. To do so let us return to the typology derived in Chapter Four and apply it here. The sum total of the types of relevant relationships (the power generic of the AEC) may be broken down into:

1) Power. The AEC had great resources enabling it to promise or deny research rewards which could seriously affect careers of U.S. atomic scientists. In effect, the resources it may have realistically been expected to use consisted essentially of the power to withhold scientific information and the funds at its disposal, especially those earmarked for scientific research. Critics have consistently charged that research funding was available to scientists who supported the AEC but not to those who criticized it.¹⁰⁴ Hence we may conclude tentatively that the AEC's utilization of power resources had the effect of inhibiting some potential opposition from scientists.

2) Influence. In its pure sense, influence can be said to describe the relationship of the AEC to the JCAE before 1961, and, until recently, little else. In the early period the members of the JCAE usually treated the AEC scientists and administrators as experts within a context understood by them as set out in their respective mandates (and on the basis of information available and, at least in principle, meaningful to the JCAE members and staff). More recently, influence may be used to describe the AEC's relationships to public decision makers at the local or state level in those instances where AEC decisions, to build reactors, dispose of wastes, etc., were faced with organized and knowledgeable local opposition. Given the esoteric nature of scientific knowledge only these instances will serve. Early examples such as those explored above where the language appears to be that of influence, that is in the form of statements of scientific fact without threat or command, turn out upon examination to be more akin to authority than influence. This is

because implicit in the early information monopoly there was a situational logic favoring AEC and the development of atomic power which was largely unquestionable by those subject to the communication. One example among many is found in the quotation from Ernest B. Tremmel above.

3) Bureaucratic Authority. Little appears to be added by drawing discrete attention to this relationship. Clearly the compliance of staff to normal administrative orders without much question was to be found in the AEC as in any other organization. Since the actual size of the AEC is relatively small as measured by the number of employees, it appears that bureaucratic authority is less singularly important than the other relationships to understanding AEC behavior.

The fact that many of the high administrative positions were occupied by scientists cannot be presumed to have directly affected relations with subordinates. We should not assume that scientists in high administrative posts necessarily served as a check against abuses. The opposite may also have been the case. For example, an analogy can be drawn between Milgram's contrived scientist-experimenter, who appears to make the morally reprehensible act of placing "scientific research" goals above the respect for the human person and his physical safety, and the failure of the AEC to take adequate precautions in relation to uranium miners, or to be sufficiently interested in tailings or fallout radiation. These faults may be explained in part by cumulative effects of specific decisions taken by individual AEC scientists to neglect such mundane problems as nuclear wastes and to concentrate on activities which were

more "scientifically sweet," to borrow a phrase from Oppenheimer,¹⁰⁵ such as developing and refining the hydrogen bomb.

4) Scientific Authority. The most fundamental manifestation of this relationship is to be seen in the original granting of power resources to the AEC especially in the areas of secrecy and control of atom-related research, and secondly in (and complemented by) the assumption by the JCAE of the position of deferential defender and, one might say, agent of the AEC. In re-defining its role in this way, the JCAE changed its position away from openness to AEC influence and toward acceptance of AEC authority. The basis for this authority lay in the link between the AEC, atomic energy, and science. In the original mandating of the AEC, military aspects weighed as heavily as the scientific as bases of its authority, but in the case of the JCAE in the early 1960's military aspects had become clearly subsidiary to the scientific. Hence the actions and assertions of a Hollifield or Hosmer may be usefully compared to those of one of Milgram's subjects. Accepting the authority of science and linking it directly with the AEC's goals and actions, just as the subject did with those of the experimenter, it is but a short step to grasp upon this authority to rationalize one's adoption of questionable actions and methods (such as we have seen even when detrimentally affecting third parties) much as did Milgram's subjects who were prepared to inflict pain upon "innocent" victims or learners. Obviously the analogy is not perfect; Ramey and the others did not follow commands of an individual scientist but approximated what they believed to be the dictates of science as related to the needs of the AEC and atomic development. Just as obviously,

we believe, the analogy in its bare essentials is valid.

After all, the "learner" too is interested in scientific achievement as shown by his initial voluntary acquiescence; the fact that he objects at a later point is, for the obedient subject, an obstacle and irritation.

This analogous relationship, though in less stark forms than any "pure" case, we suggest best serves analysis of the criticisms and issues which have arisen surrounding the AEC. The questionable methods and actions taken by the AEC may be understood within the logic of this relationship that we have termed scientific authority. When seen in this light, a question emerges which alters the perspective normally taken: how may one explain the scientists and non-scientists who challenged the AEC and JCAE? Interestingly, as Erich Fromm notes, Milgram fails to make any effort to explain why those who disobeyed, in fact, did so.¹⁰⁶ Our partial and tentative answer must be grounded in the general workings of scientific authority. We may recall that we described as a basic assumption of non-scientist AEC defenders the belief that "science is our present hope and future salvation." This can now be restated as their being predisposed--perhaps due to their backgrounds, perhaps their positions--to scientific authority; such predisposition helped make them vulnerable to accept and to defend the explicit behavior and implicit "rules" of the AEC. Generally, thus, the success of the AEC in inhibiting potential opposition may be explained in part by recourse to analysis of the workings of scientific authority. Those who disobeyed, and initiated challenges, were generally in a position to view authority as having been usurped "in the name of science."

Recent events, such as harrowing tales of nuclear near-accidents,

much publicized resignations of officials and signs of change in the JCAE, indicate that the AEC's successor agencies, the Nuclear Regulatory Commission and the Energy Research and Development Agency, will likely have to rely on the whole more on influence than on authority. This is certainly a good sign, and we will leave our direct consideration of the AEC at that. What remains in this chapter is to bring together a few of the more general considerations and conclusions relating to scientific authority which seem relevant to the context of the AEC.

Conclusion

What then does all this tell us about scientific authority? The AEC was seen to manifest scientific authority--the authority of scientists over individuals as exercised not directly over the subjects, but indirectly--through a predominantly scientific "corporate person." We noted earlier that the non-scientist tends to accept scientists' definitions as to what constitutes legitimate scientific assertions and demands. An important result, as we concluded from our reinterpretation of Milgram's experiments, is that the non-scientist not only can be made to obey demands of the scientist, but is also likely to abdicate to the scientist the moral authority of judging the rightness or wrongness of the activity, since the subject accepts that science is guided by a higher moral authority which he cannot comprehend. This is, in essence, another way of stating our initial postulation. The subject in such situations tends to give over to the agent of authority the function of delineating the context or setting the limits. Abandoning this responsibility is tantamount to the abdication of personal moral authority, for central to the maintenance of personal morality is the presumption of individual responsibility for setting limits.

In the case of the AEC, we note that the corporate person, if publicly judged to be scientific, approximates for its own goals and methods the legitimacy accorded to science, even if the real motivation of those controlling the institution lies elsewhere. In other words, the authority accorded to a scientific corporate person allows it to justify procedures and policies in the name of science, when, if one uses the traditional definition of what activities constitute a genuine search for scientific truth (including norms traditionally

attached to scientific endeavours) these procedures and policies would not qualify as science. Secondly, such a situation is tolerated, if not enhanced, by scientists who are themselves personally caught up in the institution. Particular activities planned, carried out, and defended by the AEC not only normally excluded the non-scientist from determining whether the activity was desirable or undesirable on his own moral grounds, but also offered no assurance that the scientific goals of the AEC scientists would govern the activity. The AEC was terminated due to its conflicting areas of responsibility, not because its scientific authority had been discredited.

Milgram's subjects were confronted directly with scientific authority, and most submitted totally. The environment and rationale provided ensured this possibility. It might have seemed a heartless and insensitive scientist was at work, but he still clearly held the authority of a scientist. Someone affected by and/or asked to accept an AEC decision could not even be assured that the scientists carrying out this decision had themselves ascertained its validity on scientific grounds. This, it appears to us, is a crucial factor added by the increased complexity of scientific authority in institutions, and its implications for the science and society debate should be seen as significant.

The combination of self-serving bureaucratic decisions condoned by scientists (and thus by science) is a formula for effective irresponsibility, and as such it probably most fully accounts for many of the charges made against the AEC. The analogue for the "amoral scientist" at this level consists of governmental bureaucracy in an administrative area which clearly and closely affects the well-being of the population coupled with scientific authority provided by the

importance of scientists and science in the institution. There are then two levels to the concerns raised by our analysis. First, we are skeptical regarding the ability of scientists to make correct moral decisions on scientific grounds. This is augmented by an additional consideration drawn from our discussion of the AEC. In the case of the AEC the rather limited faith which can be placed in individuals' scientific values as potentially guiding the making of moral decisions is itself placed into doubt. This is because decisions taken on other grounds may be unjustifiably rationalized as "scientific," often with full acquiescence by outstanding scientists within the institution.

This leads to an interesting and rather ominous warning--that even those of a Baconian persuasion should be wary of using scientific authority to justify technocratic decision making such as we have seen, since some of those decisions which most appear to have been made on scientific grounds were in fact made on other and often opposed grounds.

The fluoridation controversy provides an interesting illustration. In general scientists and health professionals have favored fluoridation; they were successful on the whole in municipalities where the decision was an administrative one, they usually failed when the decisions were taken via popular referenda. It appears that when the issue was defined as one for direct popular decision, a public challenge to the authority of the pro-fluoridation scientists was often able to succeed.¹⁰⁷ We may describe such a case as a successful challenge to scientific authority. It was successful, we suggest, because it changed the context of the relationship. It became no longer simply a technical issue potentially to be resolved by calculating numbers of dental caries, but a wider one involving civil rights, the interests

of the drug companies, etc. In general the scientists and health professionals who supported fluoridation refused to enter the political fray once it had developed in this manner, preferring to remain apparently disinterested observers with technical advice available to those who wished it. Hence the public political debate tended to be an unequal one, a factor which led to the rejection of fluoridation in many referenda. On the other hand, in some larger cities or where other factors ensured that the decision was taken not at a public level but through normal administrative-political channels, scientific authority usually prevailed.¹⁰⁸

Social scientists have tended to treat such public rejection as irrational, something to be explained; however, this may itself stem partly from an uncritical acceptance of scientific authority. As Harold Green noted:

Why should anyone be distressed that the public, for whatever reason, rejects technological benefits? ... Why the great hurry? ... Dental caries is a personal disease which does not spread.... There is a question of principle involved which transcends the fluoridation controversy.... Will we, a decade hence, be compelled to accept additives to the public water supplies to reduce fertility, sharpen mental processes or tranquilize the population, because the legislature so decrees?¹⁰⁹

If we attempt to explain the acceptance of fluoridation in these latter cases, we can benefit from our understanding of scientific authority. The elected and appointed officials did not question that the issue of fluoridation lay within scientific competence; in such cases the decision took place within the context of scientific authority. To use the language of influence, we may say that authority once challenged must fall back on influence (assuming there is no power to fall back on). The agent then in effect states; if you don't

follow my commands, the following will (not) happen. In the case of the fluoridation referenda, scientific authority was challenged and the influence of the scientists frequently overridden. This is similar to the minority of cases where Milgram's experimenter was challenged: Why must the experiment go on(?) asks the subject--transforming an authority relationship into (potentially) one of influence. As the context becomes more complex and formalized, the possibility and therefore likelihood of such challenges diminishes. This factor (along with others having to do with attitudes, education, etc.) begins to clarify why fluoridation tended to be rejected in certain, usually smaller, municipalities, and accepted in others.

The above should make it clear that any adequate consideration of the impact of science on society must focus not on science and scientists as traditionally regarded, but rather on the structure and functioning of institutions with built-in scientific authority. Identifying science as the problem, or indeed as the solution, then serves us little. It is not a question of science being the cause of social ills, nor merely a tool to be manipulated at will by those who hold power in society. The relationship is highly complex and the linkages are institutional but the concept of scientific authority helps us to begin to understand this connection.

This ends our admittedly only suggestive description of the workings of scientific authority. We have suggested considerations of possible merit in increasing understanding as to how scientific authority may work by comparing two cases. We are convinced that the degrees of analogous parallelism tell something valid about contemporary (U.S.) society, though problems relating to the impossibility of establishing causal relations mean we cannot prove this. We have suggested some

circumstances under which the authority of science is likely to sway people, and some potential consequences to people's being subject to scientific authority.

In the brief conclusion which follows this chapter we shall take up some of the more speculative question linking our findings herein to the more general positions in the science and society debate. However, we make no claim whatsoever to have somehow resolved the debate--merely to have helped provide some common ground from which to disagree. It is this common ground we have sought--and the concept of scientific authority, it is proposed, forms a potential starting point. As such, the impact of science upon society is conceived as being composed of concrete and in principle identifiable manifestations of several different kinds of relationships. These are limited to situations where science or scientists are directly involved. These include not only power, influence and bureaucratic authority, all of which are normally discernible as to their outer limits, but also scientific authority which is potentially much wider in scope, involving the frequent necessity for those with scientific knowledge to set limits. Such a framework thus can encompass not only the theoretical premises implicit in the supportive and oppositionist positions as set out in Chapter Two, but also the more global and deterministic position of the radicals.

It is therefore concluded that progress toward understanding the impact of science on society is possible. This can be done by overcoming some of the conceptual difficulties caused by the different theoretical premises at the base of the opposing positions. Such progress may be made through an assessment of societal compliance to

what are perceived as imperatives demanded by the authority of science.

To understand and assess the importance of this phenomenon in a given society more fully, one would view it as consisting of at least three separate though related phenomena:

- 1) The generalized acceptance of science as a good and valuable standard of right and wrong.
- 2) The existence of institutions which are publicly associated with science.
- 3) The extent to which non-scientists affected by these institutions are placed into relationships in which the legitimacy of commands which institutional representatives make upon them is defined in scientific terms.

It is, of course, the third of these phenomena in which the actual exercise of scientific authority is manifested and any such assessment ultimately rests on one's ability to discern and identify such relationships. It is here that the communications perspective offers us some important direction. Nevertheless, the first two are not to be ignored for, as we noted in relation to the AEC, it is their presence or absence which provides us with the best indication as to whether compliance is to be expected.

Our own contribution is thus small indeed when compared to the job ahead, but the above discussions of scientific authority and of the particular cases have potentially helped:

- 1) To bring together the various views on, and the various methodological approaches to, this subject within one global perspective;
- 2) To provide some common ground from which to criticize and thereby

potentially to encourage refinement and clarification of the major arguments made in this debate; and

3) To allow some possibility of usefully comparing the relative merit of these positions.

In the brief general concluding remarks which follow we will try to tie together some of the threads of the analysis provided in this work and in so doing perhaps add weight to these contentions.

Notes

1. The AEC was killed by an executive order and ceased⁴ to exist as of January 19, 1975. Two agencies were created by that order to take its place: "... the AEC's licensing and regulatory functions and commercial nuclear power programs will be taken over by the Nuclear Regulatory Commission (NRC).... Weapons programs of the AEC plus energy research will be the province of the Energy Research and Development Administration (ERDA)." "Two Agencies Replace AEC," The Gazette (January 17, 1975), p. 22.
2. Quoted in H. Peter Metzger, The Atomic Establishment (New York: Simon and Schuster, 1972), p. 26.
3. Ralph Lapp, Arms Beyond Doubt (New York: Cowles Books, 1970), p. 177.
4. See Chapter One, footnote 70.
5. See Dean Schooler, Jr., Science, Scientists and Public Policy (New York: Free Press, 1971), pp. 103-07.
6. See R.A. Dahl and R.S. Brown, Domestic Control of Atomic Energy (New York: Social Science Research Council, 1951).
7. Theodore Lowi, The End of Liberalism (New York: Norton, 1969), pp. 170-71.
8. See H.L. Neiburg, "The Eisenhower AEC and the Congress," Midwest Journal of Political Science (May, 1962), and his Nuclear Secrecy and Foreign Policy (Washington: Public Affairs Press, 1964).
9. P.B. Natchez and I.C. Bupp, "Policy and Priority in the Budgeting Process," American Political Science Review, Vol. 67, No. 3 (September, 1973), pp. 851-63.
10. No absolute comparison is to be made, however, for in the intervening years certain expenditures were removed from the AEC and others were added.
11. Natchez and Bupp, op. cit., p. 959.
12. Schooler, op. cit., p. 108.
13. Roger Rappoport, The Great American Bomb Machine (New York: Dutton, 1971), pp. 51-52.
14. See Metzger, op. cit., pp. 108-11.

15. Quoted in Barry Commoner, "Nuclear Power: Benefits and Risks," H. Foreman, ed., Nuclear Power and the Public (Minneapolis: University of Minnesota Press, 1970), p. 225.
16. Rappoport, op. cit., p. 132.
17. Quoted in Metzger, op. cit., p. 118.
18. See ibid., pp. 115-114.
19. See Rappoport, op. cit., pp. 110-15.
20. Ibid., pp. 115-17.
21. From a U.S. Public Health Service document (March 10, 1964), quoted in Metzger, op. cit., p. 175.
22. Ibid., p. 177-79.
23. See Richard S. Lewis, The Nuclear Power Rebellion (New York: Viking Press, 1972), pp. 148-71.
24. Quoted in "Pulling the Plug on A-Power," Newsweek (February 24, 1975), p. 23. See also S. Novick, The Careless Atom (Boston: Houghton-Mifflin, 1969), pp. 99-103.
25. See "Nuclear Apostates," Time (February 16, 1976), pp. 64-65.
26. Roger Rappoport, "Catch 24,400," Ramparts (May, 1970), pp. 16-21. See also H.P. Metzger, op. cit., pp. 147-50.
27. Rappoport, The Great American Bomb Machine, op. cit., pp. 45-49.
28. Ibid., p. 49.
29. "Pulling the Plug on A-Power," Newsweek, op. cit.
30. See Tom Zeman, "Nuclear Power: Lying Doesn't Make it Safe," Ramparts (August, 1974), pp. 21-25, 51-53.
31. See Gene Marine, "Moratorium: Taking the Initiative in California," Ramparts (August, 1974), pp. 29-32.
32. "Pulling the Plug on A-Power," Newsweek, op. cit.
33. "Incident at Browns Ferry," Time (October 20, 1975), pp. 113-14.
34. Cited in ibid.
35. Larry Prior (Los Angeles Times), "Nuclear Dangers Too Great, Engineers Quit," Montreal Star (February 3, 1976), pp. A1-A2.
36. "Indian Point Safety Head Charging Atom Peril Quits," New York Times (February 10, 1976), pp. 1, 14.

37. "Present AEC licensing procedures make a great pretense at licensing in a 'goldfish bowl' with opportunity for public participation. In fact, however, the entire process involves a calculated effort to present to the public a watered-down picture of minimum risks intended to allay public apprehension." Harold Green, "The Risk Benefit Calculus in Nuclear Power Licensing," Foreman, ed., op. cit., p. 138.

38. See Metzger, op. cit., pp. 249-53.

39. This has always been AEC policy and it was successful in almost every case until fairly recently. In 1969 Congress passed the National Environmental Protection Act (NEPA) which ordered all federal agencies to give priority to environmental considerations in decisions. In the momentous "Calvert Cliffs" decision, a U.S. District Court ruled that the AEC's standard argument that it had no responsibility to protect the environment from atomic installations as long as the federal radiation standards were abided, no longer stood in light of NEPA. Uncharacteristically, the AEC did not appeal the ruling. See Lewis, op. cit., pp. 259-97.

40. Quoted in Lewis, op. cit., p. 248. See also pp. 247-48 and Rappoport, "Catch 24,400," op. cit., p. 20-21.

Many journalists have begun to report on the danger of plutonium and plutonium wastes. For example:

More than half a million gallons of deadly radioactive liquid has leaked from huge storage tanks at the AEC's Hanford facility near Richland, Washington. Some of the liquid is so hot that it boils from its own radioactivity and if allowed to boil dry would melt through steel tanks. The leaks have released such deadly radionuclides as plutonium, strontium 90, cesium and others.

Other leaks have occurred at a similar facility at Savannah River, S.C. And in at least one case some of the radioactive elements have entered the water table there.

Plutonium, the most carcinogenic (cancer causing) agent known to man and quite possibly the most dangerous substance on the earth, has been buried in ordinary steel drums at the national reactor testing station near Idaho Falls, Idaho, despite stern warnings that the drums would leak.

Radioactive materials have been found in the ground water beneath the Idaho Falls facility and could pose a serious threat to water supplies for much of the Pacific northwest.

Accumulation of plutonium in a trench at the AEC

reservation near Richland has reached such a high level that a nuclear chain reaction is possible, according to an official AEC report. The AEC is now developing equipment to mine its own trenches in hopes of recovering the plutonium, which is used in the construction of atomic bombs.

L. Dye, "U.S. Faces Radioactive Waste Crisis," Montreal Star (July 14, 1973), p. B4. Another example would be C. Foley, "Making Nuclear Waste," Montreal Star (August 10, 1972), p. A9.

41. Glenn T. Seaborg, "Our Nuclear Future-1995," R. Curtis, et al., eds., Alamogordo Plus Twenty-Five (New York: Viking, 1975), p. 9.
42. David Lillienthal, Change, Hope and the Bomb (Princeton: Princeton University Press, 1963), p. 116.
43. Don K. Price, The Scientific Estate (Cambridge: Harvard University Press, 1965), pp. 225-26.
44. See Lewis, op. cit., pp. 174-81.
45. Ibid., pp. 176-77.
46. Metzger, op. cit., pp. 201-03.
47. Ibid., pp. 203-08.
48. Ibid., pp. 208-16.
49. Ibid., pp. 218-31.
50. On October 21st, 1971, at the Atomic Industrial Forum-American Nuclear Society meeting in Florida, according to Science (October 29, 1971), Schlesinger told: "the nuclear banqueteers that their cozy relationship with the AEC was at an end." While he admitted that "from its inception, the Atomic Energy Commission has fostered and protected the nuclear industry ..." this was now to be different: "The AEC's role is ... primarily to perform as a referee serving the public interest." Schlesinger simply announced that the AEC was determined now to do what it should always have been doing. Also see Richard D. Lyons, "AEC Role Shifts to Protect Public," New York Times (October 21, 1971), p. 6. The Chairman of the new Nuclear Regulatory Commission (NRC), former astronaut William Anders, has stated his role: "I will be like an umpire in a football game." Quoted in "Pulling the Plug on A-Power," Newsweek, op. cit. NRC Commissioner Victor Galinsky stated in early April, 1976, "We are still paying for what the AEC did not do in the 1960's.... We are doing safety experiments that should have been done years ago." Quoted in "Power?" Newsweek (April 12, 1976), p. 72.
51. Lillienthal, op. cit., p. 117. (Emphasis included.)
52. Quoted in ibid., p. 179, from "Questions and Answers for JCAE

Hearings" (February 2, 1970). prepared by the AEC Division of Operational Safety.

53. Rappoport, "Catch 24,400," op. cit., p. 18. In 1971, the AEC quietly reduced allowable radioactive emissions for many of its plants 100-fold, but excluded from the guidelines all new and experimental installations. See P.R. Ehrlich, A.H. Ehrlich and J.P. Holdren, Human Ecology (San Francisco: Freeman, 1973), pp. 142-43.

54. Quoted in Gene Bryerton, Nuclear Dilemma (New York: Ballantine, 1970), p. 113.

55. Ibid.

56. Richard Curtis and Elizabeth Hogan, The Perils of the Peaceful Atom (Garden City: Doubleday, 1969), p. 222.

57. Rappoport, The Great American Bomb Machine, op. cit., p. 135.

58. Curtis and Hogan, op. cit., pp. 224-25.

59. Quoted in ibid., p. 234.

60. See, for example, Gene Schrader, "Atomic Doubletalk: A Review of Matters Withheld from the Public by the Atomic Energy Commission," Center Magazine (Winter, 1971), pp. 29-52.

61. Metzger, op. cit., pp. 181-83.

62. Ibid., p. 152.

63. Ibid., pp. 29-30.

64. Rappoport, The Great American Bomb Machine, op. cit., p. 131.

65. Ibid., pp. 29-30.

66. Ibid., p. 136. See also Novick, op. cit., pp. 190-92.

67. Ibid., p. 137.

68. Ibid., p. 97.

69. Metzger, op. cit., pp. 108-11.

70. Lewis, op. cit., pp. 79-86. See also "The Radiation Effects Controversy," Bulletin of the Atomic Scientist, Vol. 26, No. 7 (September, 1970), pp. 2-12.

71. Rappoport, The Great American Bomb Machine, op. cit., p. 144.

72. Richard D. Lyons, "Nuclear Aide who Criticized A.E.C. Over Safety Shifted by Agency," New York Times (February 25, 1972), p. 42.

73. Richard D. Lyons, "Coalition Says AEC Employees are Gagged at Safety Hearings," New York Times (February 6, 1972), p. 49; "A Question of Breeding," Newsweek (May 6, 1974), p. 55.
74. See Phillip Boskey, "Nuclear Safety (III): Critics Charge Conflict of Interest," Science, Vol. 122 (September 15, 1972), pp. 970-75.
75. Quoted in ibid., p. 970.
76. Metzger, op. cit., pp. 31-36.
77. Ibid., pp. 256-57.

78. The AEC continued to come under criticism on this score even under its last administration. A good example is a meeting in the fall of 1971 at the AEC's Batelle Institute. It was called to discuss new standards of radiation and reactor safety and many top scientists were invited. Nevertheless it was a closed meeting as physicist Dr. Daniel F. Ford found out when he attempted to participate. See Anthony Ripley, "Nuclear Experts in Secret Parley," New York Times (October 1, 1972), p. 39.

The newer methods were more subtle; the intent appeared to be the same. For example, one might go back to the earliest days of nuclear reactor research--to project Sherwood in 1952:

A program instituted to control thermonuclear energy. Sherwood was classified by the AEC due to 'divided opinion' among members of the American Physical Society who were secretly polled back on June 28, 1952, on whether they thought Sherwood should be classified 'secret'. The vote actually was 88 to 1 in favor of declassifying all research dealing with the use of thermonuclear forces to generate electric current, but the AEC decided that the one negative vote indicated a 'division of opinion'. This single dissent, in the AEC's opinion, justified the secrecy ruling, even though the AEC's classification expert, who participated in the poll, voted against secrecy. Donald W. Cox, America's New Policy Makers (Philadelphia: Chilton, 1964), pp. 200-01.

The author also notes that a report by three Los Alamos scientists on the dangers of strontium 90 in 1957 was classified as "top secret" by the AEC.

79. See Lewis, op. cit., pp. 255-58; Metzger, op. cit., p. 123; Schrader, op. cit.
80. Kenneth C. Davis, "Duprocessitis in the Atomic Energy Commission," Samuel Krislov and Lloyd D. Musolf, eds., The Politics of Regulation (Boston: Houghton Mifflin, 1964), pp. 196-97.
81. See Novick, op. cit., pp. 190-95.

82. For example, Lillienthal noted the AEC practice of detonating its most spectacular tests just in time to coincide with congressional appropriations decisions. Op. cit., pp. 110-11.

83. See John C. Esposito and Larry Silverman (and the Ralph Nader Study Group on Air Pollution), Vanishing Air (New York: Grossman, 1970); Robert Fellment (and the Ralph Nader Study Group on the Interstate Commerce Commission and Transportation), The Interstate Commerce Omission: The Public Interest and the ICC (New York: Grossman, 1970); Grant McConnell, Private Power and American Democracy (New York: Knopf, 1966); Jack Anderson and Carl Kalvelage, American Government....Like It Is (Morristown, N.J.: General Learning Press, 1972); Theodore Lowi, The End of Liberalism (New York: Norton, 1969); Robert Sherrill, Why They Call It Politics, 2nd ed. (New York: Harcourt, Brace, and Jovanovich, 1974); Clark Molenhoff, Washington Cover-Up (New York: Popular Library, 1962).

84. Harold Green and Alan Rosenthal, The Government of the Atom (New York: Atherton Press, 1967), pp. viii-ix. See also Don K. Price, The Scientific Estate, op. cit., p. 224.

85. Novick, op. cit., pp. 193-94.

86. Morgenthau, Truth and Power, op. cit., pp. 193.

87. Of the many, many books in this area, the most interesting and valuable is still probably Harold L. Neiburg, In the Name of Science (Chicago: Quadrangle, 1966). See also Senator William Proxmire, Report from Wasteland: America's Military-Industrial Complex (New York: Praeger, 1970).

88. Morgenthau, op. cit., pp. 233-34.

89. Lillienthal, op. cit., p. 62.

90. Green, "The Risk Benefit Calculus in Nuclear Power Licensing," op. cit., pp. 136-38.

91. Quoted in Curtis and Hogan, op. cit., p. 243.

92. "The data have largely supported a view of the industrial laboratory as presenting a resocialization experience for academically trained scientists." Mark Abrahamson, "The Integration of Industrial Scientists," Administrative Science Quarterly, Vol. 9 (1964-1965), p. 217.

93. M.S. Larsen, "Notes on Technocracy: Some Problems of Theory, Ideology, and Power," Berkeley Journal of Sociology, Vol. XVII (1972-1973), p. 22. (Emphasis included.)

94. Don K. Price, "The Scientific Establishment," R. Gilpin and C. Wright, eds., Scientists and National Policy Making (New York: Columbia University Press, 1964), p. 28.

95. James I. McCamy, quoted in Robert C. Wood, "The Rise of an Apolitical Elite," ibid., p. 68.

96. While there has been some indication of differences between scientist and non-scientist AEC commissioners, this has been vigorously denied. For instance, in the words of non-scientist AEC commissioner, John Palfrey: "In the AEC when we examine a policy issue ... I find no discernible difference ... that can be traced to a scientist's or a non-scientist's viewpoint.... I was informed by the press that in the AEC, the scientists were lined up on one side and the two lawyers on the other. To my knowledge there has never been a decision of the commission, certainly not during my time in office, in which there was a division of opinion with scientists on one side and lawyers on the other." Quoted in Harold Orlans, Contracting for Atoms (Washington: The Brookings Institution, 1967), p. 190.

97. This is because a lively debate was going on at the time in Minnesota over the safety of the Monticello nuclear plant, being constructed forty miles from the twin cities, and whether the state had a right to set more stringent radiation standards than those of the Federal Radiation Council. (The courts ruled two years later that it did not.)

98. Transcript of discussions, Foreman, op. cit., pp. 242, 251.

99. Ibid., p. 243.

100. Ibid., p. 245.

101. Ibid., p. 254.

102. See Metzger, op. cit., pp. 137-38, 247-52.

103. Ibid., pp. 38-39.

104. Rappoport cites Dr. Robley Evans as one example. Evans, when testifying in support of the safety of AEC installations, failed consistently to mention the large research grant from the AEC on which he worked for many years. The Great American Bomb Machine, op. cit., p. 119.

105. See Green, op. cit., pp. 134-38. See also Robert Jungk, Brighter than a Thousand Suns (New York: Harcourt, Brace and World, 1958), p. 296; Nigel Calder, Technopolis (London: Macmillan, 1969), Chapter 1. Oppenheimer's reaction to the possibility of thermonuclear explosions was shared by other scientists including Hans Bethe. See Sanford A. Lakoff, "The Trial of Robert Oppenheimer," Lakoff, ed., op. cit., pp. 80-81. A similar interpretation can be given to Edward Teller's opposition to the nuclear test ban. Cecil H. Uyahara, "Scientific Advice and the Nuclear Test Ban Treaty," ibid., p. 158.

Such an argument is made by H. Peter Metzger in "Dear Sir, Your House is Built on Radioactive Uranium Wastes," New York Times Magazine (October 31, 1971), pp. 14, 59-64. Metzger supports Lillienthal's view that waste has been badly neglected by the AEC. The reason, he says, is "it is just not as glamorous as other projects that the AEC

has gotten involved in, like the abortive scheme for a billion and a half dollar nuclear airplane." (pp. 64-65)

106. Erich Fromm, The Origins of Human Destructiveness, op. cit., pp. 50-51.

107. See Bernard Mausner and Judith Mausner, "A Study of the Anti-Scientific Attitude," Scientific American, Vol. 192 (1955), pp. 35-39; H.M. Raulet, "The Health Profession and the Fluoridation Issue," Journal of Social Issues, Vol. 17 (1961).

108. "Between November 1950 and December 1966 there were 952 referenda on the fluoridation issue in the United States, and in 566 fluoridation was rejected. The fact is, as Sapolsky has pointed out, that where fluoridation has been adopted by an American community it has usually been via a different kind of decision process ... 60 percent of communities of 1,000,000+ considering the issue have adopted fluoridation--compared to only 14 percent of those between 1,000 and 2,500.... Crain and Rosenthal reviewed the variables seeming to determine the mode of decision making typically adopted in considering fluoridation. They concluded that government structures permitting public participation (more referenda) tended to adopt fluoridation rather rarely. On the other hand, those which restricted public participation and involved few referenda tended to adopt fluoridation quite frequently--by administrative decision. That is, towns with strong executives, formally less democratic, were far more likely to adopt fluoridation, as advised by the public health authorities. Sapolsky suggests that the larger cities, frequently tending to opt for fluoridating their water supplies, may most often make use of a government structure limiting direct public participation.... To summarize, almost all decisions to fluoridate water have been made by administrative order; most referenda have rejected the process." Stuart S. Blume, Toward a Political Sociology of Science (New York: Free Press, 1974), p. 265.

109. Harold P. Green, "Letters," Science, Vol. 163 (1969), p. 17.

In Conclusion

In this study we have covered a great deal of ground, and in the process have not been able to elaborate as fully as we would have liked upon some of the ideas and issues raised. Yet such was inevitable in a work of this nature. Although we hope the main portions of analysis presented here hold some value in and of themselves, this was not our main intention. Rather, we feel the merit of this work lies finally neither in the theoretical validity of our conceptualization (which admittedly is still rudimentary) nor in our conclusions as to the actual working of scientific authority, for these are highly tentative. Our primary goal was to provide intellectual links which could potentially bridge widely different opinions and approaches concerning the role of science in U.S. society and to show that the seemingly separate fields of study in this area may ultimately be reconciled. We have noted that there is no shortage of authors taking theoretical positions on the issues, and certainly no simple lack of information can account for this positional profusion. What is severely lacking is some agreed upon intellectual method whereby the theoretical issues in question may be clarified and thus potentially resolved to the extent that information is or can be made available. This work does not remedy this lack but does constitute a beginning.

We may recall that the study of science and public policy was described by one observer as "a literature in search of a field."

If we take the meaning of "science and public policy" in its widest public application, then we must now respectfully disagree with this formulation. Instead, we might suggest that our subject consists of four distinct but slightly overlapping literatures in such a search. In the chapters above we have directly or indirectly dealt with each of these.

The first literature might be described as structural-historical. It describes a transformation in the relationship of science to technology and of both to society through identifying structural and organizational changes which have taken place. Hence, it concentrates on the political and administrative institutions which have been established, their functions, legal powers, and status in relation to each other and to other branches of government. The "dean" of this school of literature is Don K. Price and it includes Robert Gilpin, J.S. Dupree, G.H. Daniels, William Nelson, James L. Penick, Daniel S. Greenberg, Carl Stover, Sanford Lakoff, Michael Reagan, Dean Schooler, and Robert C. Wood among others, most of whom are political scientists and students of public administration, or historians. To a certain extent, Chapter One consisted of a brief distillation from this literature--though the discussion of the work of Schooler in Chapter Three applies here as well, and specific references from it proved useful in Chapter Five.

A second literature is described in Chapter Three as the sociology of science. The dean of this school is Robert K. Merton and its members include such scholars as Warren Hagstrom, Bernard Barber, Norman Storer, Roger Krohn and John Ziman. A useful though somewhat separate literature is that growing out of Thomas Kuhn's

analysis of normal science and scientific revolutions. While its major thrust has been in the direction of the philosophy of science and its method, Kuhn's work has affected, and positively so, the sociology of science. The basic contribution of the literature in the sociology of science has been in the systematic analysis of the workings of the scientific community as a collectivity--as seen "internally," rather than in relation to government or to the society as a whole. It has broken down some prevailing myths, and in its analysis of cultural factors such as attitudes, norms, and values, it has helped to explain many of the social dynamics lying beneath the kinds of structural changes described in the first literature. Nevertheless, its concentration on the "internal" workings of the "social system" of science have limited its potential for contributing directly on the question of the relation of science and society.

The third body of literature takes up this question by concentrating upon the implications for the workings of science as we know it in the emerging contemporary social conditions, both structural and cultural. It seeks to determine what the role of science today is, as well as what it should be. This literature, which is the work mainly of scientists or historians of science such as Alvin Weinberg and Derek de Solla Price among many others, was described and discussed in Chapter Two, Part One.

Part Two of the chapter took up the final and widest of these literatures, namely the discussion of the implications that developments in modern science (and technology) have and will have on society, on its thinking, its activities, its future. Contributors to this discussion come from practically every area--from economists to students of religion, from philosophers to mathematicians.

In this dissertation we have braved all four of these literatures out of a conviction that ultimately they can and certainly should be reconciled wherever possible. Seen from this perspective, one major goal of our work has been to provide an intellectual link between them. We began with a brief historical discussion of the structural transformation in American science to the present, relying greatly on that first body of literature. Next, we asked ourselves the implications of these changes which took us immediately to a consideration of the third and fourth bodies of literature (going by the order above). By utilizing a comparable method of categorization in each case, we were able gradually to set the main elements of controversy in both these literatures within one framework considerably simplified to suit our limited purposes. In doing so, we identified what appeared to be the more significant doctrinal differences among the major positions as well as some of the most serious methodological obstacles which stood in the way of resolving these differences.

It was clear that one's conceptualization of science affected one's understanding of its social impact, and that therefore any contribution we were to make to this debate hinged on our clarification of how science and the activities of those termed scientists might be conceptualized in as neutral and encompassing a way as possible. In Chapter Three we thus turned to the work of the scholars whose main contributions were in this area, notably the sociologists of science. A critical appraisal of their work enabled us to build on their formulations to arrive at what for our purposes seemed to be a necessary element of any conceptualization of the workings of contemporary science—one built around the concept of scientific authority. This

concept was refined in Chapter Four, and, in Chapter Five, applied in one specific case. From this came several highly tentative conclusions less in the form of answers to the question of what the full implications of the structural changes outlined in Chapter One must be--than to the question of how one might go about usefully conceptualizing and then investigating this problem in a scholarly and not mystical manner. This allowed us largely, though not of course entirely, to avoid the extremely difficult and sticky problems raised by the concept of causality which cannot ever be satisfactorily resolved with respect to historical and social phenomena.

How, then, might all of this fit together? First, it was clear from Chapter One that the structural and institutional changes we have seen since World War Two have been accompanied by alterations in the actual practising and roles of science in the U.S.A. Given the reliance of scientific research on government funding, given the large number of scientists employed outside academic research, in industry and government and as administrators and consultants, and given the importance attached to scientific discoveries by the public and by politicians, the old conception of science as an autonomous, creative profession is no longer adequate. Instead science must be understood as including an "external" component which so importantly affects its practices that it must affect any definition.

It is the external component of the interrelationship of science with society which forms the basis of the intellectual debate on the impact of contemporary science on society and vice-versa, reviewed in Chapter Two. A long, and we hope useful, discussion identified the major positions in this debate and the methodological obstacles to its possible resolution. In very simplified terms, the problem was

that the three basic positions corresponded to three different levels of analysis. What was needed to enhance the possibilities of clarification and some degree of resolution, was a framework encompassing these three levels. From this arose our own contention that scientific authority was a concept around which such a framework could eventually be developed.

Our discussion of power, influence and authority in Chapter Four revealed that basic to authority is the idea of institutional context. Someone or something has authority when he or it is accepted as the legitimate standard of appropriate behavior in a given context, hence important clues as to the type of authority in operation always lie in the context (or contexts) of the behavior being considered. Adopting a linguistic perspective, we suggested that authority could be best distinguished from power and influence by the content of the communications transmitted from A to S and the process by which it was decoded. Further, it became clear that the process of decoding which took place where the source of the authority lay with science, as in the Milgram experiment, served to distinguish scientific authority from that normally found in formal organizations.

Finally, we turned our attention directly upon one highly important case in contemporary America to test in a preliminary way whether our analytical framework could serve to help clarify and explain. By looking in some detail at a major scientific institution and the criticisms laid at it could we find instances of relationships which exemplified scientific authority being exercised in the U.S.A. today? We did find such instances, and several conclusions from examining these did emerge.

The conclusions we tentatively reached were both substantive and methodological. From a methodological perspective, we concluded that the kind of analytical framework suggested and briefly elaborated seems to hold promise. Our understanding of scientific authority turned out to be useful, though the complexity of the empirically observable world results in manifestations of scientific authority strongly tending to be increasingly "impure" as complexity increases. Because of this, we stress the cumulative aspects of our typology.

This additive feature of the framework, we suggested, is particularly useful in allowing us to go from the more immediate levels of social activity favoured by the establishment thinkers to the intermediate levels favoured by the reformists, and potentially on to the most complex favoured by the radicals--in a logically continuous manner.

Our own application of the conceptual framework is by no means complete and thus the conclusions drawn from it are merely suggestive. Nevertheless, they do provide at least "soft" ground for giving greater credence to one position than another. More importantly, the framework does appear to give us an idea of the kind of knowledge which should be acquired to allow this debate to reach a higher level of sophistication. This would mean the participants could engage each other more directly on some common ground.

Substantively, operation of scientific authority in the case of the Milgram experiments gave us some initial reason to question the optimism of some establishment thinkers--to the extent that their confidence was based on the ability of people in the U.S. today to make the decisions necessary to utilize properly the benefits of

science and technology in specific scientist/non-scientist relationships concerning concrete tasks. This suspicion was enhanced when clear analogies appeared between the attitudes and actions of Milgram's obedient subjects and those of certain non-scientist officials in the Atomic Energy Commission and the Joint Committee on Atomic Energy. Our case study of the latter, as an example of the kind of non-academic institution in which an increasingly large and significant number of American scientists are employed and with which many more are associated, led us to question the faith of some establishment thinkers in the ability of scientists to exercise the moral wisdom required when making decisions related to their roles as social experts and problem solvers.

Our fears as to the ability of non-scientists to maintain their own moral positions (which entails, as we saw, setting limits over what behavior might be legitimately expected of them) when confronted directly with scientific authority--as evidenced by the behavior of Milgram's subjects--were enhanced in our analysis of the AEC. While generalizations from any single case must be suspect, the failings of the AEC--when viewed in the context of the functioning of scientific authority--suggest that scientific authority may very well be instrumental to the goals of institutions associated with science, whether these goals are enunciated by scientists or non-scientists. And this seems true even when those goals are determined on bureaucratic grounds which often run counter to the norms of science. Furthermore, the increasing role(s) played by scientists in governmental and corporate institutions casts doubt on the assurances derived from the assertions of the Cartesians or the analysis of the sociologists of

science that scientists can be counted upon to defend these norms under normal circumstance.

If our finding should be found to hold generally, then effective limitations upon scientific authority may lie at present with the self-restraint of officially authoritative scientists, or potentially with the appearance (such as we have seen among critics of the AEC) of other scientists ready and able to provide and sustain challenges. Furthermore, as more and more corporate entities take upon themselves the aura of control by experts trained in science and as their tasks are increasingly defined in apparently scientific terms, then there may be a tendency for the limits of this authority to be almost automatically extended.

Morally and philosophically the implications of the above, if true, serve perhaps to confirm some of the fears expressed by the technophobes treated in Chapter Two. Certainly, they tend to make us somewhat skeptical with regard to the views of the reformers. While they lend some support to the oppositionists' claim that the present relationship of science to society is not serving the long-term needs of society, as well as underlining their call for the social control of science and the exercise of social responsibility by scientists, they also call into question the oppositionist belief that all this is possible without significant social transformation. There is certainly some ground for optimism as indicated by the appearance of scientists and others who have intervened effectively against dangerous AEC decisions and by the minority of Milgram's subjects who disobeyed, but on the whole the evidence seems to point, if anywhere, in the opposite direction.

It has become more apparent that the burden lies mainly with the reformers to show how to "constitutionalize" science and technology and how to bring about a resocialization of scientists so others may justifiably rest assured that scientists are regularly practising social responsibility. Perhaps the demise of the AEC and the present pattern of interventions show some progress in such a direction. Still, in the absence of any clear demonstration of this there would lie an initial indirect affirmation for the radicals' more extreme views. In short, the onus remains on the reformers. Perhaps they could use the above framework to help shed more light on the workings of large scientific institutions generally. Possibly they could alter these in concrete cases where public harm can be demonstrated. Conceivably this will be a natural social and political consequence of properly educating the public and awakening the scientific community (or relevant communities) to its current collective social and scientific responsibilities. In this way the reformers might show that the new kinds of relationships between science and society which they, and we, see as necessary are also possible--even coming to be in evidence. We cannot push these conclusions further in favor of either the radical or the reformist opposition.

The pessimism and revolutionary ardour which alternatively grip the radicals can neither be directly supported nor refuted in this work. We have not developed thus far the intellectual tools which potentially could permit directly assessing scientific authority at the societal level. However, as evidence from the manifestations of specific cases of scientific authority is reported and thereby serves to strengthen or weaken the two major alternative positions, an

extreme stand--such as the technological determinism of Marcuse--should become correspondingly more or less attractive. This should at least slightly reduce the element of "mysticism" now attached to such positions.

Some may regard these assertions as still unsubstantiated. We would not contest this--except to say that we believe they are as accurate a summation as can be provided at this point in the debate. Others may suggest, conversely, that we have simply stated the obvious--that we have spent many thousands of words coming to conclusions which any intelligent observer can perceive for himself. We would not directly contest this objection either; for one thing we would be less than truthful if we suggested that our conclusions have fundamentally altered our initial preconceptions into this question.

However, obvious as our conclusions may appear to be, we suggest that the exercise of arriving at them was by no means in vain. The debate on and around this important question has not as yet pointed to any such obvious common ground on which there is general agreement, and it is just this which stands in the way of significant progress. If our work helps to make obvious--and provides a method for doing so--some elements of this debate which should be obvious, then our enterprise will have borne fruit.

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