ETHICS, POLITICS AND THE LIMITS OF CLOSURE IN SCIENCE*

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I. Introduction: The Role of Values in the Closure of Scientific Disputes

Three levels of value systems operative within the scientific community and in the relation of science to society can be distinguished. The first of these is the value system intrinsic to scientific research. Whether one emphasizes the inculcation and expression of these values in individuals ("the seeking after truth") or among communities ("the norms of science"), they define important behavioral obligations for the scientist. Honesty, dedication, impartiality and flexibility are among the attributes which are said to define a healthy scientific ethos. A second level of values which influences the attitudes and behavior of scientists includes personal ethical and moral beliefs, ideals and perceptions belonging to the scientist as human being and citizen. Scientists come from a range of socioeconomic backgrounds and geopolitical settings, they differ widely in the set of what Michael Polanyi calls "intellectual passions", yet they tend to share at least a basic agreement on fundamental norms associated with freedom of inquiry, criticism and communication, and the protection of the scientific community from outside interference. The final level of the value structure is more tentative and less widely shared since it refers to a social context--the relationship of science to society and the polity--which is extrinsic to the conduct of inquiry, and which has only recently attained considerable significance as the products of scientific research have increasingly permeated the outside world. These values, collected under the heading "the social responsibility of the scientist", provide a dynamic tie between the scientist as seeker after truth and as member of society, and serve as a mechanism for the incorporation of personal, social and political values into the matrix of a scientist's professional career. A scientist's social responsibility is influenced by his background and perceptions; it is brought to bear upon issues arising at the social and political frontiers of his own and related disciplines.

Traditionally, only the first of the above value systems has been regarded as appropriate for study by those who study science. Historians of science have been engaged in the rational reconstruction of scientific progress through the succession of validated interpretations of nature gained through application of the scientific method. Philosophers of science have focused on the methods whereby truths are attained, communicated and accepted. Sociologists of science have emphasized the communities within which scientists work, the parameters of which are defined by bodies of scientific knowledge and techniques, the stability of which is preserved by adherence to community norms. The second level of values, while a proper subject of study by ethical, moral and political philosophers, has been regarded as extraneous to scientific research itself, or else a fundamental given and therefore non-problematic. Increasingly, however, sociologists of science have brought to light aspects of the dark ~~out~~underside of science--resistance to innovation, the weight of established dogma, the competition for rewards, the headlong rush to publish, fraud and plagiary--which call into question the accepted model of scientific discovery and progress. As a consequence, scientific change is increasingly regarded not as something unique, but rather akin to other forms of social and political change, governed by conflict, power and influence as well as criticism and consensus.

There are two general locales where conflicts involving science take place: the scientific community itself (the internal context of science); and the interaction of scientists with society, economy and polity (the external context of science). The dimensions of internal and external conflict, the processes through which disputes arise and are resolved, will be distinguished in separate sections of this paper. The separation is not merely an analytical construct but touches on the essence of the difference between internal and external conflict, as captured in the concept of "closure". Within the internal context of science, closure operates as a conflict minimizing and moderating device, to maintain in as pure a form as possible the central values of objectivity, impartiality and consensus against the onslaught of self-interest, celebrity and dogma. On the other hand, in the external context of science, the dramatic growth in size and scope of science and its increasing mutual interpenetration with economy and polity have acted to expand the boundaries of conflict, to open science to influence by extra-scientific forces and institutions, while simultaneously promoting the involvement of scientists in conflicts related to the application of science to society. Many of the most intense conflicts in contemporary science have dealt not with competing scientific theories or interpretations of natural phenomena, but with the application of science to policy or the framing of policy for science. By nature such conflicts are "open", involving a range of participants motivated by values beyond the advancement of scientific research *per se*. The authority to resolve these conflicts has rested not with scientists alone, but with those who control the political and economic resources of society.

II. Closure in the Internal Context of Science

According to Joseph Ben-David, the dominant model in the contemporary sociology of science is one of science as a system of social interaction, a network of communications and social relationships founded upon shared norms.[1] There are two major defining characteristics in this model of science. First, science forms an autonomous, independent, closed community, free of non-scientific cares and concerns and insulated from external social influences.[2] Second, the scientific community manifests a high level of shared consensus on the fundamental components of scientific knowledge and practice--the sources of data, the criteria for their evaluation and interpretation, the questions to be asked, the range of answers which might be found acceptable.[3] As a consequence, scientists alone control the direction and pace of scientific change--the generation of new knowledge and its rejection or incorporation into the body of accepted scientific truth. The processes whereby scientific change takes place, the ways in which conflict among theories are managed, are controlled and legitimized by the consensus on fundamentals which permeates the scientific community.

The concept of "closure", defined as "the act or state of closing; the state of being closed", is basic to this pattern of conflict resolution in science. What sets science apart from other social activities, even other intellectual pursuits, is its enclosed nature, its confinement to restricted lines of inquiry pursued in clearly defined ways within precisely delimited communities. Whether in folklore or in reconstructed history (as revealed in textbooks), the march of science is characterized by a general absence of conflict. Conflict which touches the roots of established knowledge or values tends to be regarded as illegitimate, and cloture is invoked against the renegade or apostate. On the other hand, change which advances the dominant paradigm or solves the puzzles it presents (to use terms popularized by Kuhn) is, once it survives the tests required by the scientific method, readily accepted and incorporated into the body of consensual scientific knowledge.

There is, however, an anomaly in the above model. The giants of science, historical and contemporary, have been not the puzzle-solvers but the shatterers of paradigms, the pathbreakers into new fields of knowledge. Their contributions have sometimes been readily accepted or endorsed, but perhaps more often resisted and condemned. How can such opposition be explained in terms of the model of science as a community of objective, disinterested seekers after truth? What alternate model might account for the fact that, in the end, theories which demonstrate the capability to satisfactorily explain reality do in fact triumph?

Values, Conflicts and the Exercise of Authority in Science. One approach to the test of scientific theories emphasizes the role of internal authority structures. The rapid growth of modern science followed the emergence around the turn of the nineteenth century of the scientific career path--the ability of practitioners to devote their professional lives to the exclusive pursuit of knowledge about nature. Just as important was the roughly contemporaneous insulation of scientific research from "practical" concerns like economics, politics, even technology, and from the intrusion of spiritual, mystical and "pseudo-scientific" approaches owing nothing to the scientific method.

In the long-dominant internalist perspectives toward the history, philosophy and sociology of science, there are two ethical principles, integrity and independence, which serve to protect science from internal disruption and external interference. The principle of integrity operates at both individual and collective levels. The scientist fulfills his obligation to the community by total dedication to the rules which guide the pursuit of scientific knowledge, combined with a pristine adherence to the values of honesty, dedication, impartiality and criticism. At the collective level, integrity ensures the proper socialization of recruits into the "norms of science", regulates organized scientific activities according to these norms, brings violators of the norms to the bar of justice and preserves the norms from external interference or erosion. The attribute of independence defines the relationship of the scientific community to its external environment--culture, society, economy and polity. On the one hand, science must be autonomous, free of external influence or interference; there can be no political, social or religious test of truth. On the other hand, independence requires the scientist to be disinterested in rewards other than the satisfaction which comes from the advancement of knowledge, realistically including the recognition which follows such advancement. With regard to the world outside science, insofar as it does not intrude upon science, the scientist makes no value judgments. Independence, like integrity, demands some kind of central coordination, control and enforcement device to protect against internal and external threats.

Grave threats to the integrity and independence of science will engender intense and concerted response. The Nazi threat, for example, was met with global mobilization, manifested at the level of ideals by a series of ringing reaffirmations of the necessity for autonomous science,[4] and at the level of action by activities ranging from the organized effort to relocate refugee German scientists[5] to the launching of the Manhattan Project.[6]

Threats from the fringes of the scientific community itself often produce equally strong reactions, in some cases, perhaps, to the point of excess. For example, the charge has been made by an outside critic that the suppression of Velikovsky's cosmological views amounts to an abuse of power by those sworn to uphold the norms of science.[7] From another perspective, however, the rough treatment of Velikovsky was entirely within the canons of acceptable scientific behavior, an illustration of the "tacit principles" upon which science must be founded--that claims which are on the face implausible, which violate the standards of scientific methods, must be rejected out of hand, since to test them would be a culpable waste of valuable time and effort.[8] Perhaps the central issue here is not the merit of the theories themselves but the manner with which they were dealt. Were abuse, intimidation, bribery and misrepresentation characteristic of the Velikovsky Affair? If so, are they found only in isolated cases such as this, or are they endemic to science?

Robert Merton and his students have over the years detected numerous qualifications to the expected permeation of the norms of science. Elitism in science can be seen in the operation of the "Matthew effect" and in the ascriptive components of scientific rewards which play favorites with certain classes and schools.[9] Priority disputes are so frequent as to amount to the rule rather than the exception.[10] Fear of anticipation and the resultant rush to publish raise serious doubts about the supposed disinterestedness and objectivity of scientists.[11] Given the recent spate of insider accounts of what scientists are "really" like, can anything be left of the norms of science but empty rhetoric? If scientists as scientists are just like everyone else, then must not all claims to uniqueness for the scientific community disappear?[12]

Mertonians have attempted to salvage the scientific ethos by fine-tuning--thus, reward-seeking becomes the engine which drives the ship of science. Yet many of the more serious aberrations cannot easily be explained away. Among those receiving the most detailed attention are resistance to innovation and scientific fraud ("trimming", "cooking" and plagiary). A catalogue of recent charges of fraud touches the established, even eminent, as well as the young and ambitious.

1. The attempt by scientists to advance themselves within the reward structure of science through fraudulent means: the Summerlin affair.[13]
2. Unconscious "finagling" which results from the self-assurance that one is in possession of the truth: Mendel's "too good to be true" statistics or Samuel G. Morton's racial classification by skull capacity.[14]
3. The attempt to preserve a theory by conscious manipulation of the data: Sir Cyril Burt's matched pairs of twins IQ data.[15]
4. The effort of a revolutionary to overcome opposition to his views by manufacturing supporting data: the accepted judgment of Kammerer's midwife toads.
5. A charge leveled by an establishment within science against the views of revolutionaries which do not square with the dominant paradigm: Koestler's interpretation of the Kammerer incident.[16]

It is unfortunate that, in the contemporary period, the incentives and pressures which can lead to fraud are intensifying at the very time when the mechanisms relied upon to detect and expose fraud are greatly overloaded. Ultimately, the only protection may be a strong commitment to honesty and objectivity, coupled with a fear of the potentially devastating consequences of fraud to a scientist's livelihood and reputation.

Resistance to innovation has been even more extensively documented. The scientific ethos requires openness to new ideas and their objective and impersonal evaluation. The history of science, however, finds repeated counterexamples of widespread, systematic resistance to innovation, not just from such external institutions as church and state, but within the scientific community itself. The cases are legion:

> In mathematics, there is the theory of transfinite numbers; in astronomy, the heliocentric theory of Copernicus, the infinity of the universe, the theory of gravitation and the theory of relativity; in physics, the mechanics of Galileo and that of Newton, the existence of the vacuum, the complex nature of white light, velocity of light, wave theory, the kinetic theory of gases, Maxwell's formulae, electronic theory and quantum theory; in chemistry, the new chemistry of Lavoisier, the new theories of organic chemistry of Laurent and Gehrhardt, and atomic theory; in biology, the circulation of the blood, vaccination, the theory of evolution, Pasteur's ideas, etc.[17]

Despite the pain suffered by those who were right but unrecognized, an attempt has been made to place resistance in a positive light. Throughout the nearly half century it took for his theory of the adsorption of gases on solid surfaces to gain acceptance, Michael Polanyi demonstrated, by his own account, a phenomenal degree of restraint. He did not complain about the disregard or dismissal of his theory because he attached a higher value to the interest of the scientific community in keeping science uncluttered with bogus contributions than he did to his own self-interest in advancing his reputation.[18] Better, in other words, that a valid theory from time to time be denied acceptance than that a lot of false theories consume unwarranted amounts of time and money in test and rejection!

In the end, of course, Polanyi received his due. Others, however, have not been so fortunate: innovators in advance of their time may not survive to see their time come; those who accept and attempt to propound resisted innovations may find avenues of communication closed, research funds cut off, deserved rewards denied. Wegener's theory of continental drift is often cited as an example of the suppression of correct views which do not square with the biases of a dominant establishment, in this case geology. Yet, while Wegener hit upon the correct answer intuitively, his explanation was clearly inadequate from the standpoint of geological knowledge. Only much later were geophysicists able to develop a physical explanation adequate to account for continental drift; thereafter, acceptance of the theory was rapid. In the meantime, what happended to those who advocated the theory? Was their work suppressed, were they intimidated, denied access to research funds or promotions? According to John Ziman, the evidence of the case does not support such conclusions, but, on the contrary, testifies to the ability of the scientific community to accommodate intellectual controversy:

> Although the majority of the leading geologists of the time were unconvinced by Wegener's theory, and no doubt cautioned their students against it, it was not suppressed and forbidden as a "heresy". There were several set-piece public debates and conferences on the subject, and books and papers supporting Wegener's interpretation continued to be published. It is likely that some of the protagonists did not fare quite so well in their academic careers as hindsight would now consider deserving; but when, eventually, new evidence from rock magnetism vindicated Wegener's bold and imagniative hypothesis, the "old gang" were not swept into scholarl~~y~~ oblivion by the "revolutionary" supporters of the new orthodoxy of plate techtonics.[19]

Patterns of Authority in Enclosed Research Networks. The first of the major criticisms of the Mertonian norms of science, discussed above, dealt with their inability to survive in the face of intense and repeated instances of resistance to innovation. A second criticism maintains that the portrait drawn by Merton and his followers of the scientific community is overly rationalistic and positivistic, working backward from an ideal model of science to define as "scientific behavior" that which accords with the model, while explaining away all facts not conforming to epistemological presuppositions. In the view of Richard Whitley, this amounts to a "black box" model of science, with the "outputs" (scientific discovery" explained in terms of the "inputs" (norms of science), but with the contents of the black box (the actual conduct of scientific research) not subject to examination.[20] What transpires in the black box is in fact crucial to an understanding of scientific change and progress. Included are such activities as the generation, dissemination and evaluation of innovations, regulated and controlled by patterns of authority and conflict management within such scientific settings as disciplines, departments and research specialties. Only at the most diffuse and general level is science structured by a single, overarching community.

This perspective relies heavily on the work of Thomas Kuhn. For Kuhn, the primary social context of scientific research is the paradigm-sharing network, comprising researchers engaged in advancing, filling-in and solving the puzzles presented by the dominant paradigm in their field. The choice of research problem, the manner in which observation is conducted, the interpretation of findings which are produced, all conform to the dictates of the paradigm. In this model, resistance to innovation is not an aberration but an inevitable result of the eventual exhaustion of the paradigm. The appearance of a stubborn anomaly in the course of normal science creates a period of crisis. The crisis can be resolved in one of three ways: accounting for the anomaly within the paradigm; setting it aside for the time being; or shaping from it a new revolutionary paradigm. Those committed to the established paradigm will resist the revolution, often bitterly; but eventually, if its superiority is proven, the revolutionary paradigm will triumph, and a new period of normal science will commence.[21]

Although Kuhn's model possesses the advantage of focusing on the research process itself in the generation of scientific knowledge, a full understanding of the dynamics of scientific change requires an exploration of the structures of authority which operate in science to resolve conflicts among scientific theories. Among the range of authority-structures ("regimes", to apply a political concept) which might be operative in science, the two extremes can be dismissed with little comment. The scientific community is neither anarchy nor tyranny: some form of social control is necessitated by the very nature of science as an intellectual pursuit; yet nowhere does there exist the power of a single individual to prescribe dogma as truth. The requisites for the operation of democracy in science--strict equality of all members and decisions reached by majority vote--are also clearly absent. Some form of elite rule may, consequently, be operative in science, but we need to determine the scope and nature of the elite, the sources of its membership and authority, and the bases for compliance.

At the national level, the scientific community is too decentralized and particularized to permit control by a single unified elite. One possible candidate, the scientific elite of achievement and recognition comprising recipients of Nobel Prizes, possesses no structure of organization or authority. Although the National Academy of Sciences is more highly organized and integrated and operates in many ways as a political interest group and policy-making body, its influence on most issues is less than that of scientific societies in other advanced nations.[22]

There remains, however, a number of possible locales among the subdivisions of the scientific community in the United States where elite rule might be present. An obvious candidate, given the tendency toward bureaucracy in modern society, is the discipline-bounded professional society. Strong evidence has been found for the operation of Roberto Michels' "iron law of oligarchy" in the internal politics of American private associations. In his study of three of the largest and most powerful private associations in American society, those operating in business, labor and agriculture, Grant McConnell focused on a paradox of the American liberal polity: how the insulation of private associations from government control has become a central component of the pluralist defense of liberty and free choice; but how, at the same time, individual liberty faces perhaps no greater threat than from the central authority structures of private associations.[23] In perhaps the only scholarly study of its kind, Daniel Rich found that control by the leadership of scientific societies over the careers of the membership was increasing and that their ability to regulate the relations of science with society had significantly expanded since World War II, but that, nevertheless, when measured by the standards of other organizations, scientific societies were indeed "weak governments", not very effective as vehicles of professional control.[24] Despite recently escalating conflict within certain societies over the legitimacy of their taking stands on issues of the day and over charges that they are guilty of bias in the advice they provide to corporate and public agencies, their predominant role conflict probably remains between the "pure science" ideal of exclusive commitment to the advancement and diffusion of knowledge, and the professional goal of promoting the economic and social standing of their members.

More likely agencies of elite control in science are the administrations of university science departments organized along disciplinary lines. The chairmen and tenured members possess a number of important sources of authority. They direct the training and certification of recruits to science, influence the distribution of research funds and laboratory facilities, control advancement within the department and access to many of the rewards available outside the department. Power able to be used can also be abused. Numerous cases have been cited of graduate students compelled to do their dissertation research along lines they would not voluntarily have chosen, of junior professors denied promotion because of refusal to toe the department line, of domineering chairmen, monopolizing the rewards earned by their underlings;[25] reported cases of such abuses probably represent only a fraction of actual occurrences. Nevertheless, there do exist important protections against oligarchical, even dictatorial, rule in university departments. According to Ben-David, the departmental structure in the United States, in contrast to the German model of the early twentieth century, is much more democratic, or at least "pluralist". Instead of a single professor who dominates the department, its curriculum and research programs, there exists a multiplicity of professors, many possessing tenure, pursuing independent programs of research and training their own students, under only loose central guidance and control.[26] Second, the primary research affiliation of scientists has shifted with the explosion and specialization of scientific knowledge, from the discipline of training and affiliation, still the basis of departmental structure, to the specialized research network, comprising scientists having similar interests and concerns from a variety of academic departments, often ranging across disciplinary lines. These research networks are analogous to Kuhn's "paradigm-sharing communities" and Price's "invisible colleges".[27] To fully comprehend the process of closure in science, one must define the operative regime of these specialized research networks and examine the processes whereby authoritative decisions are made and conflicts resolved.

The Dynamics of "Opening" in Science. Recent work by Nicholas Mullins and his colleagues has significantly advanced our knowledge of the structure of specialized communities in science and their role in scientific change and progress. They are communities in a true sociopolitical sense. A congruence on major values among the members is prerequisite to their growth and development. The members are in constant contact with each other, in writing and in person. They share common goals, symbols and perceptions of external opportunities and threats. Internal disputes are resolved by legitimate processes, never through resort to "violence".[28] For Griffith and Mullins, communities are found among groups of researchers attempting a radical reconceptualization of their field. Their study of six such communities (the phage group, Skinnerian behaviorists, ethnomethodologists, the Copenhagen school in quantum mecahnics, Goettingen algebraists and audition researchers) revealed certain common features: a clear and definitive program statement, an intellectual and an organizational leader (sometimes the same person), a geographical center, and a high degree of commitment, coherence and consensus among the members. The groups differed in their methods of bringing about change in the broader scientific community. The first three on the list were revolutionary outgroups, assaulting a dominant establishment. The last three possessed elite scientific status: their leaders were clearly members of the establishment; their work was regarded from the beginning as of central importance and, once completed, was readily able to gain acceptance. In all cases, access to the group was quite open, outlets for position and publication were readily available, cases of suppression, intimidation and other naked uses of power to prevent dissemination of ideas were rare.[29]

Nevertheless, conflict did occur within the "home" disciplines of all the above groups over the accuracy and relevance of their contributions; it remains quite intense in some of them. Clearly, therefore, empirical verifiability cannot be the exclusive test for the acceptance or rejection of group-based scientific paradigms. Michael Mulkay has investigated such other, more social factors as the degree of cognitive consensus which exists in a field, the extent to which new ideas depart from the cognitive consensus, and the prestige and sponsorship of the innovator. Utilizing the historical context of the reception of Louis Pasteur's ideas, Mulkay distinguishes three patterns:

1. Rapid acceptance of innovation--organic crystal structure;
2. Strong resistance to innovation--organic basis of fermentation;
3. Acceptance based on social position--organic explanation of decay.[30]

Emphasizing more clearly political concepts, Marlan Blissett has transformed Polanyi's image of the "Republic of Science" into something more closely approximating a confederation. Potential regimes in scientific disciplines are defined by the permutations of two variables: number of decision-makers (many or few) and degree of theoretical consensus (high or low). Of the four possible regime types, communal polity (many decision-makers and high theoretical consensus) and democracy (many decision-makers and low theoretical consensus) were found to manifest no contemporary examples. Instead, all scientific disciplines were found to belong to one of the other two regime types, both possessing elite decision structures: competitive pluralities in disciplines (e.g., biology) where there exists frequent and open competition among paradigms; oligarchies in disciplines (e.g., physics) which possess a single dominant elite, where competition is discouraged and conformity enforced.[31] Blissett's distinction would seem to be borne out in the contrasting patterns of conflict resolution manifested by molecular biology, in the debate between the structural and informational schools, and by physics, in the dispute between quantum mechanics and its critics. However, a totally contradictory conclusion might be reached if two different conflicts were selected--e.g., receptivity toward the quark theory in physics contrasted with the disposal of the challenge to Crick's central dogma of genetic inheritance in molecular biology. The single factor of greatest weight in accounting for the various ways in which these conflicts were handled is not the governing regime of the discipline as a whole which Blissett emphasizes, but the structure and operation of paradigm-sharing research networks within the discipline. During revolutionary change these networks are highly integrated and tightly defined--"enclosed" would be the best use of the concept of closure in this context. But periods of revolutionary change are inevitably short-lived: the paradigms are either defeated and the group withers away, or they triumph and the group becomes the governing elite of a new period of normal science. Many of the most insightful students of contemporary science have found in the concept of "openness" among research networks in science, the key to long-term intellectual growth and change. These include: Crane's "diffusion of innovations" within and across invisible colleges; Holton's opening up of "new areas of ignorance"; Ben-David's "crossfertilization" among specialties; and Mulkay's "migration" of workers from established to emerging paradigms.[32]

Most scientific disciplines would seem, therefore, during normal times, to be characterized by a form of limited competitive pluralism. Competing paradigms coexist relatively peacefully within a rather strictly enclosed research network, as long at least as each produces knowledge and insights and none challenges underlying principles and assumptions. Despite the emphasis by Stent on the schism between the "structural" and "informational" schools of molecular biology, Olby's magisterial research has established the extent to which each contributed to the Watson-Crick model. In the sense that Watson and Crick belonged to both schools, "their model destroyed the separation".[33]

Nevertheless, the conflicts surrounding the challenges to quantum mechanics and the Crick central dogma illustrate the limits of pluralism in science, as well as the continued significance of closure as a mechanism for limiting conflict in those cases where the conflict touches the central theoretical core of a paradigm, where the opposing sides fail to share a common perception of the nature of reality (or the reality of nature). The result of such conflict is rarely a revolutionary paradigm shift, that is, the total conversion or defeat of the side adhering to the established paradigm. The resources possessed by the establishment, including control of journals, research funds and the means of education and training are able to retard the expansion of conflict, prevent the repeated and open challenge to authority, delay the struggle to gain converts. Neither David Bohm in physics nor Barry Commoner in biology have gained much attention, to say nothing of support, within the confines of the parameters they have struggled to overturn. Their efforts have, in fact, probably had a negative effect on the scientific reputations they otherwise would merit. In Commoner's case, at least, another dimension of the scientific conscience has come into play: the extra-scientific "social responsibility" which Commoner has asserted so long and hard. In ecobiology, unlike quantum mechanics, external reality impinges significantly upon the perceptions of a scientist and the research strategies he chooses to follow. We proceed, then, to an exploration of the overall relationship of the scientific community with society, which, in contemporary America, has placed an increasing strain upon social closure in science.

III. Closure in the External Context of Science

The justification for the autonomy of the "Republic of Science" within the broader liberal polity traditionally rested on the unique position ascribed to science among other social institutions. Unlike business, labor or agriculture, science sought no influence over social, economic and political decisions and consequently had little fear of government or corporate interference. The relationship was not one of total separation but of mutual benefit through maximum independence. Here too, however, as in the internal setting of science, the reality of interaction was far more complex than the myth. The products which often result from scientific research have long been regarded as useful to the state, a fact which Hunter Dupree documented historically for the United States.[34] Scientific research was supported, not only because its products were regarded as "useful" but also because of the belief that science was virtuous and deserving of support, that its interests were equivalent to the national interest. Society in general had much to learn from science: a system of values, an orientation toward reality, a faith in the inevitability of progress. The liberal credo supportive of free enterprise, itself influenced by the application of the scientific method to practical affairs, in turn strongly justified the autonomy of science. A good example of this mutual benefit relationship was Darwinian biology. Applied to the social realm, its principles reinforced free enterprise and laisser faire. In its pure form, it gained rapid and widespread acceptance, even within organized religion. As Charles Rosenberg states:

> There never was a pervasive and genuinely divisive discontinuity between scientific and religious imperatives in the minds of most educated Americans; the remarkable thing about Darwinism...is not the conflict it inspired, but--considering its implications--the lack of conflict. The comparatively rapid acceptance of Darwinism by the scientific community meant its acceptance within a relatively short time by most articulate Americans.[35]

Yet, as Rosenberg also points out, scientific research is equally subject to influence by social values:

> ...Just as scientific vocabulary and metaphors can serve as ideological building blocks in the creation of a usable world view, scientists have... incorporated social perceptions into the formal texture of their work.[36]

This mutual influence process between science and society, which extends well into the past, has, of course, intensified during the contemporary period. No longer limited to cultural norms and social perceptions, science now encompasses matters of money and power. This has caused a strain in the delicate accommodation upon which the autonomy of science traditionally depended. While government support presents increased opportunities for facilities, equipment and personnel, it carries with it the potential for economic dependence and the influence which money buys. On the other hand, as government grows ever more dependent on scientific knowledge, the potential for influence by scientists over the policy process correspondingly increases.

Don K. Price claims to have found a means for preserving mutual independence in the continued viability of the principles and mechanisms of American pluralism. The old Madisonian model of checks and balances in a territorially open republic has been transformed, he asserts, into a new model more appropriate to a post-industrial nation. Three branches of government have been replaced by four estates: political, administrative, professional and scientific. The balance between the internal autonomy of pure science and the control by the polity over the implementation of science is preserved by the following principle: the closer an estate gets to "truth" the more freedom and autonomy it is permitted; but the closer it gets to "power" the more it is required to submit to the test of political responsibility.[37] This defense of the applicability of pluralism to the science-policy nexus has been attacked both by those who fear the growing influence of government over science and by those who perceive the danger of government dependence on scientific expertise.

Social Direction of Scientific Research. Price's early depiction of the monetary relationship between science and government as "federalism by contact", where each party benefits while retaining its autonomy and independence,[38] is regarded by some as no more accurate a depiction of the true relationship of science to government than is the original U.S. Constitution to the reality of contemporary power distribution between central government and states. In this view, the dependence of scientific training and research on massive amounts of governmental funding has eroded the autonomous control of the scientific community and given to government the power to actually direct scientific change and its societal applications.

David Noble has traced the process in the private sector as far back as the late nineteenth century. At first concentrated in the electrical and chemical industries, and later extended throughout industrial American, technological progress came to be controlled by giant corporations through their power to define the agenda of research, to organize productive processes and to shape the form of technical education. The pattern was broadened to cover basic and applied science as they were "technologized" and increasingly centered in corporate research laboratories. Both science and technology have ever after been instruments of aggressive, expansionary capitalism.[39] For Marlan Blissett, the coming transition from the bureaucratic to the post industrial state will signal, not the centrality of technoscientific knowledge forecast by Daniel Bell, but instead the final loss of internal autonomy by science (training, research agendas, reward structure) and its subordination to societal "complexes". Science will be motivated no longer by its own internal values, but will be transformed into an instrument for the implementation of the projects or programs developed by the complexes it serves.[40] One need not subscribe totally to this vision of instrumental, technologized science, to recognize the dangers posed by mission-orientation and economic control to the continued viability of the cental expressed scientific values of integrity and independence.

Control over the direction of scientific research and its applications is as much social and cultural as political and economic. Despite the supposed autonomy and value-freedom of science, scientists share the values and goals, ideals and perceptions of their fellow human beings and citizens. As Norman Storer has pointed out, even though science is international, scientists are themselves dominantly national. Before a scientist is made, he is born into a particular nation and indelibly stamped with that nation's values.[41] Beliefs, ideologies, class interests, intellectual concerns, all are incorporated into the conduct of a scientist's work. The desire to promote national interests, to be of use to society is rooted deeply in the social history of science. Rosenberg has shown how the perceived need for immediate practical utility caused turn-of-the-century geneticists and nutritionists to eagerly formulate "solutions" to social problems which, in the long run, seriously harmed both their disciplines and society at large.[42] A similar pattern may be found in the more recent activities of nuclear physicists, behavioral psychologists, even molecular biologists.

Social utility has been elevated by some contemporary students of science and public policy into a concrete standard by which to measure the value of scientific research. For example, Jean-Jacques Salomon concludes that there is no longer any real distinction between pure science and applied science, that the values of pure science "are becoming more and more dissolved in the functions it fulfills in achieved techniques", and that consequently, there is no escape for pure science from "the criteria of utility which make reseach into an institution linked with the production system..."[43] Alvin Weinberg has employed social utility as a device to decide among levels of public support for various scientific disciplines. In order to merit support, a discipline must satisfy certain externally defined criteria: the development of socially useful technologies; the expansion of knowledge in related disciplines; and the promotion of highly valued social goals. Based on these criteria, Weinberg concluded fifteen years ago, molecular biology merited maximum support, nuclear power heavy investment, since it fell short only in its contribution to knowledge in other fields, but behavior research, high energy physics and space exploration little or no support.[44] With the hindsight of history, Weinberg's hierarchy stands as an object lesson in the variability of such criteria as "highly valued social goals". Just as important as what Weinberg said, of course, is who he was: his position in the nuclear power establishment gave him a decisive role in the definition of social goals as applied to scientific choice in at least that one field; in turn, his own ranking of social goals was unavoidably influenced by the position he held.

The Political Interests and Influence of Scientists. We come then to the other side of the mutual influence relationship: the potential control by scientists, through their knowledge and expertise, of economic and political processes. Price's justification of pluralism as a device to prevent domination by a scientific elite has been faulted here on two grounds: its failure to provide a mechanism for citizen involvement in the making of science policy; and its inability to guarantee that elected and appointed public officials will retain ultimate control over those who possess scientific and technical expertise.[45] Despite the periodic warnings of the impending domination by a "scientocracy" or a "technological priesthood", the greatest threat of abuse of authority by science in American society may rest instead with its accommodation to a dynamically transformed pluralism. Theodore Lowi labels this new form of pluralism "interest group liberalism", which he defines as the division of public authority among a plurality of functionally-based elites, each within its own sphere unchecked by public officials and unresponsive to any values but the pursuit of its own self-interest.[46]

Daniel Greenberg has located an example of such a sphere of authority, where a particular scientific elite has created its own political machine, in what he calls "high energy politics". From World War II until the early 1960's politically astute nuclear physicists were able to exploit their own expertise and the fear in Congress of an "accelerator gap" with the Russians to secure increasingly more powerful and more expensive particle accelerators, built at government expense.[47] Lowi and his students picked up where Greenberg left off, describing the influence exerted by the nuclear physics establishment to secure federal commitment to the world's largest accelerator, eventually constructed in (rather, over) Weston, Illinois. In their view, not only was the National Accelerator Laboratory of little immediate practical utility to society, its construction was actually violative of the emerging national commitment to the economic opening-up of the suburbs. The obliteration of the village of Weston eliminated a lower-middle class, racially integrated community in the interest of providing temporary facilities for the planning and construction of the laboratory.[48] From a slightly altered perspective, however, one can see in this case the continued operation of the classic model of mutual benefit relationships. The nuclear physics establishment secured its accelerator, Mayor Daley and Senator Douglas obtained a valuable and prestigious facility, and the county fathers of DuPage eliminated a socioeconomic thorn in their particular body politic. Those who lacked the influence paid the price.

In the long run, "high energy politics" fits the classic model of open-ended conflict characteristic of American politics. To the extent that divergent interests are aroused to action, the potential for domination by a single elite declines and conflict is ultimately resolved by adjustment among a range of interests. This may explain the most recent developments in the National Accelerator Laboratory controversy: the resignation of the director of the Laboratory in protest against years of serious underfunding of Congress; and the subsequent commitment of sufficient funds to more than double the accelerator's power despite (or perhaps because of) this resignation.

According to the above model, scientists behave politically as members of interest groups; they seek to advance their interests by mobilizing the resources available to them within the political arena. As is true of all social groups, the interests of scientists are defined by the values and goals they possess. At the most general level, the values of science are permeative--freedom of research and communication, the need for societal approval and support. When the values comprising this consensual ideology of science are threatened, the response of the scientific community will understandably be intense and concerted. More frequently, however, the values which scientists seek to further are not those intrinsic to scientific research but relate to the applications of the products of scientific research in particular social contexts and situations. Such values are shaped in large part in the external environment of science. Inevitably, therefore, scientists will rarely find themselves on one side of an issue: while they might agree on the "science" involved (approaches, techniques, even data), they will disagree on the interpretation of the results and their policy implications. This pattern can be found in almost all recent scientific controversies--nuclear weapons policy and arms control, nuclear power, recombinant DNA controls, desegregation of schools; the list is a long one.

Consequently, in most important areas where science and policy overlap it would be a mistake to expect the problem to be resolvable by restricting it to its "technical dimensions" alone, in the hope that all parties could thereby come to consensual agreement. This is the central defect in the proposals for a "Science Court", comprising objective, impartial scientific judges who would choose between the opposite sides in a controversy solely on the merits of the scientific evidence. There is no way to deny entrance here to extra-scientific values since they are often at the heart of the controversy itself.[49]

Since the concept of value-freedom is totally inappropriate to the complex modern relationship of science to society, is it inevitably to be replaced with particularistic values and the pursuit of self-interest? Are groups of scientists equivalent to other interest groups in a pluralistic society, engaged in the struggle for economic advancement and political influence? To some extent the answers to these questions are affirmative. But at a deeper level, the third level of the value structure of science delineated earlie~~st~~r, values come into play which cannot be attributed solely to self-interest or personal professional gain. Although it has been argued that there is no room in the pluralist model of politics for a concept of the "public interest", much of the contemporary political activity of scientists can best be interpreted as an attempt to promote and specify the utilization of science for the "common good", as perceived by scientists.

This "social responsibility" of the scientist serves as the dynamic connection between his roles as member of the scientific community and as human being and citizen. It provides a mechanism for the incorporation of personal, social and cultural values into the professional judgments he makes. It is a response to the fact that contemporary science is not detached from, but thoroughly caught up in society. Social responsibility cannot be absolute; it is instead situational, influenced by a scientist's background, beliefs and perceptions as applied in the context of a particular professional choice. The implications of the assertion of social responsibility for the stucture of the scientific community and the actions of its members are dramatic. The boundaries of both internal and external conflict in science have been rapidly expanded, straining consensus, sometimes undermining the authority of the leadership of science, and thoroughly eroding the conventional bases for closure of disputes.

Science, Society and the Expansion of Conflict. Once it becomes apparent that a particular decision will have positive, negative or problematic effects upon deeply held social values, the pressure for expanding the range of values considered and the participants who will decide among them rapidly intensifies. Those conflicts in the external dimension of science which fit this pattern will inevitably be transformed from "scientific" conflicts to "political" conflicts, and their outcomes will be determined by the weight of political forces involved. E. E. Schattschneider made the point succinctly: "The central political fact in a free society is the tremendous contagiousness of conflict".[50] This is not to say that all decisions generate conflict or that all conflicts expand their boundaries--Schattschneider employed the word "contagiousness" with precision. The negation of the expansion of conflict is the "mobilization of bias" to prevent the initiation of conflict. Scientists benefit particularly from the specialized, often remote nature of the data which they employ. Deference to expert authority has often served as an effective instrument for restricting the boundaries of conflict, by scientists in, for example, the nuclear power debate, as readily as by the executive branch in, for example, national security policy. However, these examples also illustrate the potential for eventual expansion of conflict: the erosion of "national security" and "executive privilege" as blocks against public disclosure and the assertion of Congressional authority; the "opening up" of the nuclear power decision process by the creation of a separate Nuclear Regulatory Commission.

The controversy over recombinant DNA controls is particularly instructive regarding the attempts of various groups of scientists to limit or expand the boundaries of conflict. Despite the claims that recombinant DNA researchers themselves first publicized the potential risks of their research, the case was actually characterized from the beginning by the efforts of an elite among these researchers to restrict participation. The Asilomar Conference (1975) was opened to the press only with reluctance; virologists, immunologists and other relevant experts were absent, as were known opponents of the research within the molecular biology community. The guidelines of the National Institute of Health were primarily framed by those with a direct interest in seeing the research proceed and, as written, the guidelines favored their own particular lines of research, often at the expense of those, of equivalent "risk", pursued by outsiders. Legislation providing for strict enforcement and inspection by a lay-dominated review board was intensively opposed, while national "preemption" of rDNA controls was actively encouraged as a means of precluding the proleferation of conflicts in specific localities and states. It is still too early to pronounce this strategy a success: at the national level public attention has diminished; the more stringent Congressional legislation has been sidetracked; yet the national advisory committee has been more than doubled in size, now comprising a majority of "lay" members. Whatever the outcome of the rDNA conflict, however, it is clear it will not be determined by the "power" of a scientific elite to confine or cut short the conflict. In the face of widespread, intense, persistent public concern over the physical or moral threat posed by rDNA research, such power would be very weak indeed.

The concept of "interest" is relevant to the rDNA case, as to others previously discussed. For some, self-interest has primacy--the advancement of personal careers or the research paradigm with which identified. It is such a preoccupation with self-interest which rendered the high energy physicists participating in the NAL site decision oblivious to the "displacement of the conflict" away from the social and economic integration of the Chicago suburbs. It is the reductionism inherent in paradigmatic self-interest which caused neurobiologists to push their own narrow view of the causes of schizophrenia upon those responsible for the making of social and public policy in this area, despite the often devastating results.[51]

On occasion, the pursuit of self-interest may lead the scientist to violate the internal norms of science, perhaps as much for the purpose of maximizing his influence over policy as to advance his own scientific reputation. For example, the evidence is now overwhelming that Sir Cyril Burt "cooked" his date to conform with his thesis of the genetic base of intelligence, thereby securing and advancing his influence over British educational policy. As the peculiar, and somewhat pathetic case of William Shockley illustrates, one does not need any special qualifications in the field of genetics to exploit one's scientific reputation and public access in order to propose views based more on social prejudice than scientific fact. Nor does not necessarily have to break the rules of ethical scientific behavior to advance one's self-interest upon society;

they only need to be bent a little. An example can be found in the actions of certain protagonists in the rDNA drama to "publish" data favorable to their own interest through press release and congressional testimony rather than through the regular journals.[52] Such cases as these may be indicative of a growing threat to the survival of the internal norms of science as scientists are increasingly involved in, and tempted by external social relationships. In the words of Jerome Ravetz:

> The ideology and social context of this ethical commitment (to the internal norms of science) are inherited from an earlier age; and the conditions of industrialized science present them (scientists) with problems and temptations for which their inherited "scientific ethic" is totally inadequate.[53]

The best protection against abuse, for society and for science, may be the light of public attention, the clash of values in the open air, the willingness to carry an issue into the public arena rather than to accept restriction to scientific values within a scientific context. Once conflict does break out, it is very difficult for the original participants to control. External values brought by fresh participants may play a decisive role in the outcome. The internal value structures and power relationships of previously closed organizations are opened up to the public attention, demythologized and subjected to detailed security--and in the process these organizations are forever changed. In their appeal to extra-scientific social values, in their call for the involvement of opinion leaders, policy-makers and the public, the dissenters among rDNA researchers are following a trail blazed by their predecessors in nuclear physics.

This ability to promote expansion of conflict varies with the reputation, experience and, perhaps most importantly, the celebrity of the scientist. Those who are called by Rae Goodell "the visible scientists" have by definition automatic access to public opinion. Goodell finds significance in the frequency with which visible scientists advance, not their own scientific views, but public causes relating to the social implications of science, usually in areas outside their original professional specialization, and often at the expense of their scientific reputations.[54]

Another group which must be mentioned comprises the "invisible scientists", younger and less established, who find it necessary to band together in organizations in order to advance the cause of "public interest science" through media, lobbying and proselytizing activities among their colleagues.[55] Such activities often bring down the wrath of the establishment upon their relatively unprotected heads, and always take away from the precious time needed to advance their careers through pure scientific research.

In summary, despite the increased sophistication of the techniques for expansion of conflict from science to society during the contemporary period, the process can actually be traced quite far into the past as, in one discipline after another, what was originally a purely intellectual pursuit gradually entered the forefront of public concern, with increasing relevance to lifestyle, social values and political goals: economics by the early nineteenth century; Darwinian biology toward the end of the century; human genetics early in the twentieth century; nuclear physics by mid-century; molecular and socio-biology in the present decade. In broad outline, the process by which conflict expands is as follows:

1. The boundaries of debate are widened by scientists themselves, as conflict takes the form of clashing value systems, disparate risk-benefit ratios, or contradictory conclusions drawn from common sets of data through the application of different value hierarchies.
2. Extra-scientific participants are brought into the conflict by one or the other side: religious or moral spokesmen; opinion leaders; political decision-makers; and so on.
3. The ultimate power to define the parameters of conflict, establish the operative ground rules and prescribe decisional outcomes rests with economic, political, or judicial authority, not the "scientific establishment" or the participants themselves.

Once the issue has been cast into the political arena, scientists retain important types of resources, not least of which is the continuing influence of the objective, positivistic and rationalistic mode of analysis ascribed to science by society. Policy-makers remain subject to inordinate influence by data and credentials. Much of the history of policy-making is itself the search for an objective, rational "science" of policy formulation. It is evident, however, that the search will be neverending. Ultimately, policy-makers must confront the limits of rationality as a mode of analysis. Alleged rational actors must operate in a social environment where the potential for rationality is at best narrow and transitory. Even the Supreme Court, that so-called "final arbiter of the Constitution", can be overturned by Constitutional amendment and intimidated by the weight of executive or legislative power, but it can also be swayed by changing values and their social contexts. The scientific community is certainly no less subject to the influence of alterations in the operative structure of social values.

The thrust of the argument presented here is this: that the complexity of a policy system which is leaky rather than sealed, existing in an environment comprising multiple arenas of conflict and limited sovereignty, greatly restricts the potential for attainment of full and final closure in any political dispute, including those in the societal context of science.

NOTES

[1] Joseph Ben-David, The Scientist's Role in Society (Englewood Cliffs, N.J.: Prentice-Hall, 1971).

[2] Michael Polanyi, "The Republic of Science: Its Political and Economic Theory", in Criteria for Scientific Development: Public Policy and National Goals, ed. Edward Shils (Cambridge, Mass.: MIT Press, 1968), pp 1-20.

[3] Thomas S. Kuhn, The Structure of Scientific Revolutions, 2d ed., enlarged (Chicago: Univ. of Chicago Press, 1970).

[4] It is certainly no coincidence that three of the most impassioned defenses of the integrity and independence of science were made at this time. For Michael Polanyi, the spirit of personal scientific inquiry by free and dedicated men is threatened by bureaucratic direction and control. For Karl Popper, the intense intellectual honesty realized through the critical faculty is undermined by ideological and economic forces. For Robert Merton, the "ethos of science" is endangered by extra-scientific values and political interference. See Jerome Ravetz, Scientific Knowledge and its Social Problems (London: Oxford Unv. Press, 1971), pp 311-312.

[5] Charles Weiner, "A New Site for the Seminar: The Refugees and American Physics in the Thirties", in The Intellectual Migration: Europe and America, 1930-1960, eds. Donald Fleming and Bernard Bailyn (Cambridge, Mass.: Harvard Univ. Press, 1969), pp 190-234.

[6] Martin Sherwin, _A World Destroyed: The Atomic Bomb and the Grand Alliance_ (NY: Knopf, 1975).

[7] Alfred de Grazia, _The Velikovsky Affair_ (NY: University Books, 1966).

[8] Michael Polanyi, "The Growth of Science in Society", in Shils (op.cit.), pp 187-199.

[9] "For unto every one who hath shall be given, and he shall have in abundance; but from him that hath not shall be taken away even that which he hath." See Jonathan R. Cole and Stephen Cole, _Social Stratification in Science_ (Chicago: Univ. of Chicago Press, 1973).

[10] Robert K. Merton, "Priorities in Scientific Discovery", in Merton, _The Sociology of Science_, ed. Norman Storer (Chicago: Univ. of Chicago Press, 1973), pp 286-324.

[11] Warren Hagstrom, _The Scientific Community_ (NY: Basic Books, 1965).

[12] This is the lesson which many have learned from James Watson, _The Double Helix_ (NY: Signet, 1968).

[13] Joseph Hixson, _The Patchwork Mouse_ (Garden City, New York: Doubleday, 1976).

[14] Stephen Jay Gould, "Morton's Ranking of Races by Cranial Capacity", _Science_ CC (May, 1978), pp 503-509.

[15] D. D. Dorfman, "The Cyril Burt Question: New Findings", _Science_ CCI (September, 1978), pp 1177-1186.

[16] Arthur Koestler, The Case of the Midwife Toad (NY: Vintage, 1973).

[17] Rene' Taton, Reason and Chance in Scientific Discovery (NY: Wiley, 1962), pp 147-148.

[18] Michael Polanyi, "The Potential Theory of Adsorption: Authority in Science Has its Uses and its Dangers", Science CXLI (September, 1963), pp 1010-1013.

[19] John Ziman, Reliable Knowledge: An Exploration of the Grounds for Belief in Science (NY: Cambridge Univ. Press, 1978), p. 133.

[20] Richard D. Whitley, "Black Boxism and the Sociology of Science" in The Sociology of Science, ed. P. Halmos, Sociological Review Menograph No. 18 (Keele University, 1972), pp 61-72.

[21] Kuhn, op cit.

[22] On the National Academy of Sciences, see Philip Boffey, The Brain Bank of America (NY: McGraw-Hill, 1975).

[23] Grant McConnell, Private Power and American Democracy (NY: Vintage, 1966).

[24] Daniel Rich, "Private Government and Professional Science", in Scientists and Public Affairs, ed. Albert H. Teich (Cambridge, Mass.: MIT Press, 1974), p. 30.

[25] One of the most publicized cases is the charge by H. J. Muller that T. H. Morgan attempted to block the work of younger members of his laboratory on genetic inheritance, and, after the work had gained wide acceptance, hogged all the credit, refusing to give recognition where it was due. Other students of Morgan have vehemently denied

the charge. See Harriet Zuckerman, *Scientific Elite: Nobel Laureates in the United States* (NY: The Free Press, 1977), pp 141-143.

[26] Ben-David, *op cit*.

[27] Derek J. deSolla Price, *Little Science, Big Science* (NY: Columbia Univ. Press, 1963).

[28] These attributes of political communities are elaborated by Karl Deutsch, *Political Community and the North Atlantic Area* (Princeton, N.J.: Princeton Univ. Press, 1957).

[29] Belver Griffith and Nicholas C. Mullins, "Coherent Scientific Groups in Social Change" *Science* CLXXVII (Sep. 1972), pp 959-964.

[30] Michael Mulkay, *The Social Process of Innovation* (London: Macmillan, 1972), pp 9-17.

[31] Marlan Blissett, *Politics in Science* (Boston: Little, Brown, 1972), pp 107-112.

[32] Diana Crane, *Invisible Colleges* (Chicago: Univ. of Chicago Press, 1972); Gerald Holton, *Thematic Origins of Scientific Thought* (Cambridge, Mass.: Harvard Univ. Press, 1973); Ben-David, *op cit*; Mulkay, *op cit*.

[33] Gunther Stent, "That Was the Molecular Biology That Was", *Science* CLX (Apr. 1968), pp 390-395; Robert Olby, *The Path to the Double Helix* (Seattle: Univ. of Washington Press, 1974), p. 440.

[34] A. Hunter Dupree, *Science in the Federal Government* (Cambridge, Mass.: Harvard Univ. Press, 1957).

[35] Charles E. Rosenberg, No Other Gods: On Science and American Social Thought (Baltimore: Johns Hopkins Univ. Press, 1976) p. 3.

[36] ibid, p. 19.

[37] Don K. Price, The Scientific Estate (NY: Oxford Univ. Press, 1965), pp 120-162.

[38] Price, Government and Science (NY: Oxford Univ. Press, 1962), pp 65-94.

[39] David Noble, America by Design (NY: Knopf, 1977).

[40] Blissett, op cit., pp 192-196.

[41] Norman W. Storer, "The Internationality of Science and the Nationality of Scientists", International Social Science Journal XXII (1970), pp 80-93.

[42] Rosenberg, op cit., pp 19-21.

[43] Jean-Jacques Salomon, Science and Politics (Cambridge, Mass.: MIT Press, 1973), p. 76.

[44] Alvin Weinberg, "Criteria for Scientific Choice", Minerva I (Winter, 1963), pp 159-171.

[45] Langdon Winner, Autonomous Technology (Cambridge, Mass.: MIT Press, 1977), pp 251-262.

[46] Theodore J. Lowi, The End of Liberalism (NY: Norton, 1969).

[47] Daniel S. Greenberg, The Politics of Pure Science (NY: New American Library, 1967), pp 209-269.

[48] Theodore J. Lowi, Benjamin Ginsberg, et al., Poliscide (NY: Macmillan, 1976).

[49] The most thorough critique of the Science Court proposal is Dorothy Nelkin, "Thoughts on the Proposed Science Court", Newsletter on Science, Technology and Human Values 18 (January, 1977), pp 20-31.

[50] E. E. Schattschneider, The Semi-Sovereign People (NY: Holt, Rinehart and Winston, 1960), p. 1.

[51] Hilary Rose and Stephen Rose, "Do Not Adjust Your Minds, There is a Fault in Reality', Ideology in the Neurobiological Sciences", in Social Processes of Scientific Development, ed. Richard Whitley (London: Routledge and Kegan Paul, 1974), pp 148-171.

[52] John Lear, Recombinant DNA: The Untold Story (NY: Crown, 1978).

[53] Ravetz, op cit., p. 408.

[54] Rae Goodell, The Visible Scientists (Boston: Little Brown, 1977).

[55] Joel Primack and Frank von Hippel, Advice and Dissent: Scientists in the Political Arena (NY: Basic Books, 1974).