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DEFERRING TO AUTHORITY: POPULAR SCIENCE COMMUNICATION AS

LANGUAGE OF CONTROL

by

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B.A.(Hons.), Carleton University, 1974

A THESIS SUBMITTED IN PARTIAL FULFILMENT OF THE REQUIREMENTS FOR

THE DEGREE OF MASTER OF ARTS (COMMUNICATION)

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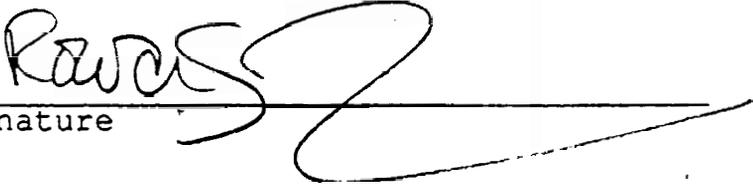
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ABSTRACT

This thesis analyses the communication of science to a nonscientific audience, and characterises it as persuasive and controlling.

The accepted purposes of popular science communication are to create or develop a scientific literacy and to inform public decisionmaking on issues involving science and technology. However, data on public attitudes and knowledge show that both ostensible purposes are frustrated: public understanding of science remains low, as does attentiveness to policy issues in which science is a component. Against this background of ignorance, nevertheless, public confidence and trust in science is high.

This thesis suggests that popular science communication better serves the purposes of interests in organized science increasingly dependent on resources allocated in the public domain, and interests that exploit the ideological potential of science in governance. It suggests that much of popular science communication is ideological, aimed at procuring the public warranting of science through creating discursive relations of trust and authority.

In a test of this interpretation, a set of exemplary science journalism texts is analysed in two methodological passes. Using first techniques from rhetoric, then critical linguistics, the characteristic modes of communication are distinguished as persuasive and controlling. The analysis suggests that popular science communications is persuasive about the trustworthiness of scientists, and controls meaning to prefer only an interpretation of scientific knowledge as objective and authoritative. This representation of science excludes a more wholesome understanding in attempts to secure deference to its authority.

To prevent the knowledge of science from being withdrawn from everyday life and used as an instrument of ideology, we must find new means of communicating it. The thesis concludes by exploring ways by which we could create more equitable relations of communication between science and a nonscientific public.

Anyone who has ever tried to present a rather abstract scientific subject in a popular manner knows the great difficulties of such an attempt. Either he succeeds in being intelligible by concealing the core of the problem and by offering the reader only superficial aspects or vague allusions, thus deceiving the reader by arousing in him the deceptive illusion of comprehension; or else he gives an expert account of the problem, but in such a fashion that the untrained reader is unable to follow the exposition and becomes discouraged from reading any further.

If these two categories are omitted from today's popular scientific literature, surprisingly little remains.

-Albert Einstein, 1948.

Foreword to Lincoln Barnett, *The Universe and Dr. Einstein* (New York: William Sloane Associates, 1948), p. 1.

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INTRODUCTION:

THE WELL-ARTICULATED SCIENCE

> "If there's anything you don't understand," urged Dr. Breed, "ask Dr. Horvath to explain it. He's very good at explaining." He turned to me. "Dr. Hoenikker used to say that any scientist who couldn't explain to an eight-year-old what he was doing was a charlatan."
> "Then I'm dumber than an eight-year-old," Miss Pefko mourned. "I don't even know what a charlatan is."
>
> -Kurt Vonnegut, Jr.,
> Cat's Cradle

The two most commonly accepted purposes of popular science communication are the creation or development of a science literacy, and the provision of information appropriate to decisionmaking on science policy issues. Into the first category fall expository works on quantum physics, brain research, microbiology, immunology, and the like. Science policy communication is more explicitly concerned with the social consequences and relations of science, and likely to have as its themes the allocation of public resources for science, risk assessment in toxic waste disposal, nuclear reactor safety and sitings, and environmental issues such as acid rain.

While a great deal of activity has been generated to these purposes -- featuring science on television and radio, in newspapers, and particularly in magazines -- there is puzzling evidence that the project of science popularization may not be what it is purported to be.

In 1986, information gathered by the Scientists' Institute for Public Information showed that 66 daily newspapers, with a combined circulation of over 11 million, carried a weekly science section, and another 81 carried one page designated for science/health/technology news.[1] *Nova, National Geographic Specials*, and *Newton's Apple*, PBS network science shows, reached audiences of 7, 10, and 4 million each broadcast.[2]

From the 1950s to present, such survey work as has been conducted on science in the media has consistently indicated a public interest in reading about science, and an expressed preference for more coverage of science-related issues.[3] A survey undertaken by the Newspaper Advertising Bureau in 1977 suggested that reader interest in the U.S. was quite strong. Science items were rated as among the most interesting of all newspaper editorial content. Respondents gave an overall rating of the editorial content of newspapers: 24 percent of all editorial content was deemed "very interesting," but 32 percent of science and technology material was "very interesting."[4]

In Canada, a 1975 study, *Media Impact: a research study on science communications* produced for the Ministry of State for Science and Technology, surveyed Canadians to discover that 80 percent of the most popular news topic preferences related to science. (Education, medicine and health, pollution, ecology and the environment, and some social issues such as urban planning and population control were construed as science-related.) More than 75 percent of Canadians wished to remain current on science news, but over half felt that the media were not doing an adequate job of providing science coverage.[5]

But although there appears to be a great outpouring of science news and information, and an ever greater demand to satisfy, there seems to be little accomplished in addressing science to the masses. Other survey data, gathered in 1979, also indicated that only seven percent of adults in the U.S.A. could meet a minimal test of scientific literacy, a statistic that suggests that very little of the science communication intended for a mass audience is understood, or has any effect in developing a capacity to understand.

On a simple 0 to 5 index of substantive science knowledge, with one point each given for a clear understanding of radiation, GNP, and DNA, and two points given for a correct understanding of the processes of scientific study, 41 percent of Americans scored zero in the 1979 survey. (Twenty-three percent scored one, and about 37 percent scored two or more.) Further, failure to attain at least a threshold level of scientific literacy

through schooling was shown to restrict both interest and ability to understand the communication attempts to develop that literacy.[6] It would appear that the explanatory promise and rationale of science popularization is scarcely fulfilled. This must be considered a doubtful purpose at any rate, given the practical barriers to understanding indicated in these surveys.

For similar reasons, the explanation and legitimation of science popularization on the grounds that awareness and capacity to critically evaluate issues involving science and technology are the necessary conditions of modern citizenship in an age shaped by science and technology is caught in contradiction.

In 1979, as part of the Science Indicators series of surveys conducted by the National Science Foundation, about 20 percent of American citizens reported a high level of interest in science or technology issues, felt well-informed about those issues, and reported a regular pattern of information consumption to maintain their currency.[7] This segment of the public is attentive to science and technology issues in public policy. Yet of these, only one-third met the minimal test of scientific literacy.

Nevertheless, because these attentives possess sufficient information to consider specific science policy issues, and are most likely to take some form of political action on them, they are the focus of efforts by science policy leaders and decisionmakers to generate a constituency for science.

But, while the expressed purposes of science popularization are apparently routinely frustrated, belief in, and support of, science continues strong among the public. The 1985 Science Indicators study shows that the public expresses a high level of confidence in the scientific community. Of 12 major social institutions (including leading corporations, media, military, and the education system) only medicine receives a greater public warrant of trust than the scientific community.[8]

A deeper explanation of the actual function of popular science communication is needed, together with a questioning of its commonly expressed purposes.

One area that first needs to be examined in the search for an explanation lies within the functional imperatives of organized science itself. According to Miller, Suchner and Voelker:

> The single most important change in the practice of science in the twentieth century has been the emergence of 'big science', as characterized by team research and large and expensive physical facilities. Individual creativity and intellect continue to be the most important elements in organized science, but an ever larger portion of scientific and technological research requires complex combinations of personnel and physical resources to exercise the basic intelligence and creativity that drives the processes of discovery and innovation.[9]

As science has become increasingly organized and supported by first, public monies and then corporate capitalization, the internal social organization has come to address, rely on, and be influenced by these relations. As Greenberg has suggested:

> ...[T]he more science, as a social practice, forms an integral part of the economic structures of the society in which it is embedded, the more the boundaries and differences between the two dissolve.[10]

As the scientific enterprise became integrated into the larger social and economic structures, during and after the Second World War, an economy of interests and discourses emerged to strengthen the points of articulation of science and society in both political and pragmatic relations. Popular science communication is one of the most important elements of these articulating relations.

For example, Canada's first National Science and Technology Policy, signed only this spring in Vancouver (1987), has as one of its six key objectives:

> To ensure that science and technology become an integral part of our culture by increasing the public's awareness of the importance of science and technology to Canada's economic and social well-being.[11]

Why is it that the public must know about science? *What* is it about science that we should understand? This thesis attempts to address these questions, and takes the following line of inquiry.

In the first chapter, something of a social history of science popularization since the Second World War is presented. The renewed interest in science popularization is attributed to an increasing awareness of the political and economic power that could be derived from a science constituency. The lessons drawn from early successes and excesses of communicating about science produced a more coherent set of shared interests within the scientific and political and economic communities, and a direction for the discourse of science in the media. In this process, the agitations for science popularization become increasingly more motivated, and the communication reflects the distortions of the interests at work: an ideology evolves.

In the second chapter, the argument for recognizing science popularization as a type of ideological discourse is expanded through recent sociological studies of science, particularly those of Thomas S. Kuhn and Michael Mulkay. The ideological potential of a realist conception of science is probed by placing in doubt its representation of science as an ideal knowledge with a special status as uncontrovertable and objective truth. The adoption and reproduction of this representation outside science in popular accounts is presented as realizing an ideological potential: alternative accounts or understandings of science are suppressed or distorted and the authoritative character of science preferred. This authority is constructed for the opportunities of social control it provides.

Some specific features of this ideology as it conditions popularizing accounts of science are operationalized in the third chapter as discursive strategies of persuasion and control. Persuasion as a discursive strategy is regarded as communication practices aimed at guiding deliberative action, or rhetoric in its classical sense. Discursive strategies of control are regarded as operations performed on linguistic features of an utterance in

communication so as to limit or strongly prefer a particular interpretation. Analytic tools from rhetoric and critical linguistics are developed for application to popular science communication in an attempt to determine if the ideological features of authority and trust are present.

Textual work begins in the fourth chapter. The reasons for selecting the text sample is outlined, and the communication situated with reference to audience and producers. An exemplary set of texts is defined and analysed, and the results presented together with an interpretation. The texts are distinguished by two broad categories, deliberative and expository. Some texts seek to guide public decisionmaking, others seek to explain. Rhetorical analysis is most appropriate to deliberative texts, critical linguistic analysis to exposition. Evidence of ideological discursive practices is presented, and an interpretation offered that the texts are constructed to suppress possible readings of scientific knowledge as contingent or uncertain, and persuade a trust in the producers as virtuous and impartial guides to truth.

The conclusion further explores the movement to establish authority discovered in the analysed texts. Strategies for questioning and negotiating authority reproduced in texts are drawn from a novel model of sociological analysis. Such an opposing struggle is recommended as a way of balancing power in communications, and in particular, in enabling a more wholesome understanding of science.

NOTES: Introduction

1   Scientists' Institute for Public Information, "U.S. Dailies with weekly science sections," and "U.S. Dailies with weekly science pages," *SIPIscope* (Autumn 1986), pp. 12-14.

2   Scientists' Institute for Public Information, "TV science shows in production," *SIPIscope* (January-February 1986), pp. 1-4.

3   Survey Research Center, Institute for Social Research, *The Public Impact of Science in the Mass Media* (Ann Arbor: University of Michigan, 1958) and Survey Research Center, Institute for Social Research, *Satellites, Science, and the Public*, (Ann Arbor: University of Michigan, 1959). Reported in Sana Siwolop, "Readership and coverage of science and technology in newspapers and magazines: report to the Council for the Advancement of Science Writing," mimeo, April 1980, p. 197.

4   Clyde Z. Nunn, "Readership and coverage of science and technology in newspapers," Journalism-Quarterly, 56(1979), pp. 27-30. Reported in Sana Siwolop, "Readership and coverage of science and technology in newspapers and magazines: report to the Council for the Advancement of Science Writing," mimeo, April 1980, p. 197.

5   Orest Dubas and Lisa Martel, *Media Impact: a research study on science communications. Volumes 1,2* (Ottawa, Ministry of State for Science and Technology, 1973 and 1975). Reported in Sana Siwolop, "Readership and coverage of science and technology in newspapers and magazines: report to the Council for the Advancement of Science Writing," mimeo, April 1980, p. 199.

6   Jon D. Miller, "Scientific literacy: a conceptual and empirical review," *Daedalus* 112 (Spring 1983), pp. 29-48.

7   National Science Board of the National Science Foundation, *Science Indicators 1980* (Washington. D.C.: US Government Printing Office, 1981).

8   National Science Board of the National Science Foundation, *Science Indicators: the 1985 report* (Washington. D.C.: US Government Printing Office, 1986), pp. 142-154.

9   Jon D. Miller, Robert W. Suchner and Allan M. Voelker, *Citizenship in an Age of Science* (New York: Pergamon, 1980), pp. 4-5.

10   David Dickson, *The New Politics of Science* (New York: Pantheon, 1984), p.24.

11   Government of Canada, "Background paper: The National Science and Technology Policy" (Ottawa: Government of Canada, 1987), p. 9.

CHAPTER ONE:

MOTIVATED COMMUNICATION

This chapter develops the argument that popular science communication is one of a set of articulating relations between science and society. It will be suggested that popular science accomplishes its part in this articulation by constituting an attentive public for science that can be controlled without being informed, allied without being involved directly in any way that might jeopardize the autonomy of the scientific community.

The development of the articulating relations of science and society since the Second World War -- and the modes of communication that helped effect, maintain, or change them -- can be located and described as varying as functions of three funding cycles for basic science. Dickson, Greenberg, and other commentators[1] have noted that the first two postwar decades (1945-1965) were a period of vigorous growth for science, dominated by federal funding. In the next decade (1966-1976), funding stagnated as the role and substantive activities of science were questioned and criticised. Since the late 1970s, support for science has once again entered a dramatic growth phase, this time spurred by corporate funding.[2]

The politics of science changed also in function with the funding basis. During the Second World War, Vannevar Bush, then president of Carnegie Institution and a former dean of engineering at the Massachusetts Institute of Technology, convinced President Franklin Roosevelt to enlist science to the war effort. Bush was also successful in convincing the administration at the same time that the most efficacious manner to do this would be to provide scientists with adequate support, and leave the scientific community to organize and disperse resources independently. The National Defense Research

Committee, superceded shortly by the Office of Scientific Research and Development, accomplished this structural arrangement.

After the war, and with the demonstration of the efficacy of science's contribution in the form of the atomic bomb, radar, and a host of other strategic technologies, both the scientific community and government, for their separate reasons, were eager to institutionalize an articulation between them. However, the political structure under which they might be carried out became a contested question. Arguing against a legislative proposal that would have established government support combined with direct legislative control, Bush and a panel of science notables produced one of the most significant ideological documents of modern science -- *Science: the endless frontier.*[3]

Part one: Colonizing the frontier, 1945 - 1965

Science: the endless frontier, delivered on July 5, 1945, to Roosevelt's successor, Harry S. Truman, laid out a set of basic principles that the leadership of the scientific community wished to have recognized in the policy contract between science and government.

Prime among these principles was that the mechanism of support, while allowing government to fulfil obligations of accountability, must "leave the internal control of policy, personnel, and the method and scope of research" to the research institutions themselves.[4] These principles were endorsed, although not without struggle and some compromise, with the establishment of the National Science Foundation in 1950.[5] Federal funding for research grew handsomely in the next decade. From 0.3 percent of GNP, or $920 million in 1946, funding grew to 0.8 percent of GNP, or $3.45 billion, in 1956.[6]

Bush's orchestrations helped dissipate the suspicion generated by a history of neglect and exploitation in the relations of science and government. The success of the Office of Scientific Research and Development and the Manhattan District of the Army Engineers (a.k.a. the Manhattan Project) in enlisting scientists through contractual relations with industrial corporations and universities, rather than setting up centralized systems of military laboratories, had exploded the simple subordination-to-utility relationship.[7] As historian Don K. Price noted, science would no longer:

> ...serve as a docile instrument toward purposes that are implicit in a
> system of automatic economic progress, or even toward purposes that are
> defined for sciences by business and political leaders. In short, we can no
> longer take it for granted that scientists will be "on tap but not on top."[8]

Science began to emerge as an "estate," as Price defined it, a community with a special function and share in government along with the professional, administrative, and

political estates. The addition of science to the traditional estates likewise entailed the beginning of a new negotiation of political relationships.

Twenty years after Bush opened the new frontier, Price could propose a "twofold principle of freedom and responsibility" in government that would enable science to obtain the support required for its own purposes, and allow more effective use of science for the practical purposes of public policy:

> The most important principle seems to be a twofold one: (1) the closer the estate is to the end of the spectrum [from truth to power] that is concerned solely with truth, the more it is entitled to freedom and self-government; and (2) the closer it gets to the exercise of power, the less it is permitted to organize itself as a corporate entity, and the more it is required to submit to the test of political responsibility, in the sense of submitting to the ultimate decision of the electorate.[9]

But the real contest in the relations of estates has been for the increasing freedom from responsibility in the process of converting truth to power. Popular science communication, as I hope to show, is one of the means by which the scientific community maintains autonomy with power. The argument that efficacy of science can only follow from its autonomy was not enough to sustain advantageous relations for the scientific community. Recent history had demonstrated that power could alter the exercise of truth, not truth the exercise of power. The new relations had to be maintained in a way that permitted some control from within science: the expansion of an ideology of science, and the creation of a constituency for science to which the other estates could be held responsible provided this control.

Border skirmishing

There were significant beginnings and growth in popular science communication during the immediate postwar period as well, keeping pace with the developments in the larger universe of discourses in science and public affairs. Offering science goods for consumption, James McGraw Jr., president of McGraw-Hill trade publishers, launched a

mass circulation science magazine, *Science Illustrated,* in 1946.[10] The glossy monthly was designed to be one where "the average citizen [could] find in his own terms a reporting or interpreting of what the scientists are doing, what they are beginning which will soon be affecting our lives." McGraw's market strategy in popularizing science was to reach an audience of "science-activated people who lead the buying parade, who are keenly alert to new ideas, who are usually the ones to buy things first." But the potential market segment, estimated at about 25 million at the time, was not attracted to the light shed by *Science Illustrated.* Four years and an investment of $5 million later, the magazine reverted to a cruder "ghee-whiz" and gadget-oriented editorial package that did not so much prepare readers to meet the future as it did equip them to remove unwanted hair while they waited for it to overtake them.

The market logic and scientific appeal were better understood by Gerard Piel and Dennis Flanagan, two editors working at *Life* magazine in the late 1940s. The two identified the same sort of window of opportunity presented by the closer articulation of big science and social and economic structures. However, they saw an opportunity to:

> ...serve the need of the scientist, the engineer, the doctor, the educator, and the intelligent layman for information concerning the progress of science, engineering, and medicine in all their branches and in their application at the social and economic level to the lives of all men....[T]he common denominator of this audience is the interested layman: the scientific professional who is a layman in departments outside his own.[11]

The strategy seemed to work for Piel and Flannagan. They bought and refurbished the venerable *Scientific American* magazine (103 years old in 1948), and had by 1951 turned it into a profitable enterprise. As Lewenstein notes, "successful 'popular science' meant disseminating scientific knowledge to a well-educated technocratic elite."[12]

Meanwhile, the scientific community sought other means of forming and extending the ideological links on which the new relations of science were engaged. Cultural strategies, as well as those based on economic efficacy were pursued. New frontiers were

opened in responding to new ideological challenges: first the anticommunist, anti-intellectual attacks of the McCarthyism, then the technological humiliation of Sputnik.

In 1951, the American Association for the Advancement of Science (AAAS), the national scientific society founded in 1848 to "further the work of scientists, to facilitate cooperation among them, to improve the effectiveness of science in the promotion of human welfare....,"[13] recognized the need to reorganize its activities to respond to changing political and social relations. The Board of Directors of the AAAS (then called the Executive Committee, meeting in Arden House at Harriman, New York) drafted a policy statement that acknowledged the new frontier principles:

> In view of the present size and complexity of science, in view of the seriousness and importance of the relation of science to society, and in view of the unique inclusiveness of the AAAS, it seems clear that the organization should devote less of its energies to the more detailed and more isolated technical aspects of science, and devote more of its energies to the broad problems that involve the whole of science, the relations of science to government, and indeed the relations of science to our society as a whole.[14]

The opportunity to try and "put science back together" that seemed to present itself to the Board of Directors in 1951 was in part the product of the privileged political position gained from the legislation enacting the National Science Foundation, in part the recognition of the strategic importance of scientific research and development expressed by the various arms of the military (particulary the Navy), and in large part, the resulting influx of resources.

But the impetus for more activity on the relations of science and government was also a result of new threats to the intellectual freedom and autonomous operations of the scientific community that came from the House Committee on UnAmerican Activities and Senator Joseph McCarthy. A number of the AAAS executive had been the subject of accusations of the committee, and the public awareness of science and scientists had been roused in a way that endangered their political and economic future. The board proposed

undertaking "as a major active interest" the long-neglected portion of the statement of purpose of the AAAS in its constitution "...to increase public understanding and appreciation of the importance and promise of the methods of science in human progress."[15]

The new policy recognized that the "diffusion among the general public of knowledge about science and its methods is a difficult, slow, and never-ending job."[16] But with the promise and the threat that the dissolving boundaries brought to science and society, came the recognition that:

> ...[I]n our modern society it is absolutely essential that science -- the results of science, the nature and importance of basic research, the methods of science, the spirit of science -- be better understood by government officials, by businessmen, and indeed by all the people.[17]

This was also the period of the investigation of media effects using emerging social science methodologies.[18] The new phase began, in part, as a corrective to ungrounded observational evidence that promoted a view of the media as simply and powerfully instrumental in shaping and changing beliefs and opinion. The experience of the Second World War had seemed to confirm that the media could be powerful -- and dangerously so under the control of authoritarian states. The era of effects research undertaken by Berelson, Lasswell, Lazarsfeld et al. Star and Hughes and others focused on the possiblities of the media for active persuasion -- and on means to mitigate or control these potentials.[19]

New agitations over the perceived power of the media provided new opportunites for joining the scientific community's interests with a broader constituency. The Commission on the Freedom of the Press, which reflected the contemporary concerns over the role and effects of the press (widely defined to include most other mass communication mediums) had added social responsibility theory to the political agenda of the media and polity.[20] The rise and apparent power of radio and film (with the example of their

contribution to propaganda in the war still vivid), coupled with a widespread awareness that the free market philosophy adopted for the operations of the press had not produced enough of the expected social benefits, had stimulated a reappraisal.

Social responsibility theory had as principle tenets: an assumption that the media serve essential functions -- especially in the maintainence of a democratic political process; an assumption that the media accept the obligation to fulfil social functions, particularly in providing information and a plurality of viewpoints; a priority on sustaining the fullest possible independence of the media consistent with the fulfilment of its obligations; and adoption of explicit standards of performance and mechanisms of adherence to them.

In practice, social responsibility theory has exercised influence on the media in two broad approaches. The first is in the establishment of public, but independent or arm's length agencies for the management of media -- largely the broadcast media -- such as the FCC (Federal Communications Commission) in the U.S.A., or the CRTC (Canadian Radio and Television Commission) in Canada. The second thrust has been the development of professional codes of conduct -- criteria for informativeness, accuracy, balance, and truthfulness -- as a means of realizing improved standards of performance, without unacceptably compromising media autonomy.[21] Social responsibility pressures, joined with renewed activity on the part of organized science, helped produce and institutionalize a new media role and site for popular science discourse -- science journalism.

The good soldiers

Science journalism had begun to be institutionalized as a profession in the 1950s -- the membership of the National Association of Science Writers (NASW) grew from the dozen founding members in 1934 to over 400 by the end of the 1950s, a particularly active decade for the science writers. The NASW had a close relationship with the AAAS; it was an affiliate member of the larger science body. The objectives of both organizations

corresponded, and the AAAS sought to use the leverage provided by the association with the NASW and current social responsibility criticism to further the new policy of promoting public appreciation. As Lewenstein points out, the policy leaders of the scientific community reasoned that making efforts to help science journalists become more "professional" through training seminars, briefings, informational encounters with scientists (and through grants of money to undertake such activities as surveys of the media that discovered a need for more science coverage) achieved these ends more effectively than any independent initiatives to communicate scientific knowledge directly. Social responsibility theory of the press was part of the social context and provided effective, and invisible, instruments further connecting the scientific community's interests and the public good.

The science writers themselves were ideologically aligned with the scientific community in the project of creating public appreciation of science. The professional code of ethics by which NASW sought to set the boundaries of membership and control the production of science writing had as a major theme a dedication to "factual accuracy" -- and an operational definition of factual accuracy that translated as those facts which scientists determined appropriate. According to Lewenstein, there were even proposals to include as part of a professional ethos a requirement that scientists read and comment on manuscripts before publication.[22]

Science writers were not complete shills in the scientific community's game, however. Following a joint meeting of the AAAS and NASW in 1956, the president of NASW, John Pfeiffer, reacted to the suggestion on the part of the AAAS that science information aimed at the general masses through television, newspapers and movies "necessarily involved a dilution and distortion...."[23] Writing in the NASW Newsletter of March 1956, Pfeiffer complained: .

[AAAS] efforts are being devoted exclusively to an audience considerably smaller than 'all the people' -- an audience of scientists and 'intelligent, responsible' laymen....Nothing is planned for the 50,000,000 or more people who obtain their science news from the newspaper science reporters, press services, and popular magazines.[24]

NASW began to commit more resources towards its own professional goals, undertaking surveys with foundation grants that documented -- using the tools then evolving for empirical 'scientific' research in communications -- a growing demand, editorial attention, and ultimately, of course, need, for more science writing.[25] In 1959, NASW established the Council for the Advancement of Science Writing, having also discovered the need to support and promote science writing more actively and "increase the quantity and quality of scientific information in the public press....[to] heighten the public's understanding and appreciation of scientific enterprises."[26]

The concern with the popular acquaintance with science in the 1950s reflects a convergence of interests in the scientific community, among decisionmakers in business and government, and within sectors of the media. The studies and policies devoted to popular science communication developed, as Chris Dornan notes, "explicitly as a systematic influence on the practice of that which it sought to investigate....descriptive findings were to provide the basis for a series of prescriptive agitations."[27]

Spy in the sky

The conjunction of interests is most clearly and dramatically marked by the launch of Sputnik by the Soviet Union in 1957, an event which jolted the American administration and science community, and precipitated an even greater mobilization of resources.

In the political arena, NASA (National Aeronautics and Space Administration) was created in 1958, the administration created a special assistant to the President for science and technology, and raised the status of the wartime-established Science Advisory

Committee from its largely ceremonial function and linked it directly to the executive office. A program of science education in the schools was begun in 1958, under the aptly titled National Defense Education Act, a reflection of the cold war ideology that placed strategic importance on science. In Dickson's account:

> The Sputnik episode was therefore a key turning point in postwar U.S. science policy from two points of view. Fears of Soviet supremecy in science and technology -- whether or not they were legitimate -- were used to justify an increased commitment of federal funds to civilian research, which rose dramatically in the years that followed. At the same time it marked the full acceptance of the scientific community in the corridors of Washington.[28]

But the launch of Sputnik by the Soviets was, perhaps most importantly, an ideological challenge to liberal-democratic social order. As Dornan aptly puts it:

> In the ongoing dress rehearsal of a Cold War, in which technological capacity is the index of power, scientific accomplishment is the criterion for victory....This was not simply a matter of surrendering a military advantage to an enemy: it represented a direct challenge to the United States' own understanding of herself and her place on the world stage. America was the exemplary democracy, whose social conditions promoted and rewarded excellence in every endeavour. The Soviet Union, by comparison, was widely understood to be a ruthless police state that perpetuated its rule through the armed suppression of its population of backward peasants. Its economy, without the benefit of the stimulation of the market, was sluggish and austere. Its science too, at all times subordinate to the dictates of political ideology, was presumably a hobbled imitation of that permitted by Western freedom of inquiry.[29]

Popular science communication became an even more critical point of articulation bewteen science and society. The perception that the Soviet system had produced a scientific triumph that was related to the mandated nature of the scientific undertakings and the equally controlled development of the human resources for science prompted a re-examination of science policy. The National Defense Education Act was the manifestation of the strategic and ideological importance attached to science education. The project of popularizing science, already begun with the initiatives of the AAAS and NASW and private and government agencies, took on a similar ideological character and conditioning in practice. It now assumed an inflated strategic importance. Greater resources were

dedicated to popular science communication, and communication also assumed a greater influence in the allocation of resources.

Ignorance of science, in light of this new -- and spectacularly demonstrated -- strategic importance was a perceived threat to democracy. Science as a generator of economic progress, and as an emerging source of political authority was more than ever implicated in the social and economic structures of North America. Continued scientific illiteracy impaired democratic governance, and rendered the polity susceptible to irrationality and manipulation. Or so the rhetoric went. As Warren Weaver, one of the principle ideologues of public awareness of science, drafter of the Arden House policy of the AAAS, and later president of that organization, put it in a talk given a little over a month after Sputnik wrenched open the window of opportunity for science popularizers:

> No longer is it an intellectual luxury to know a little about this great tool of the mind called science. It has become a simple and plain necessity that people in general have some understanding of this, one of the greatest forces that shapes our modern lives. We must realize what conditions of freedom and flexibility of support must be maintained for pure scientific research in order to assure a flow of imaginative and basic new ideas. Without some of this understanding we simply cannot be intelligent citizens of a modern free democracy, served and protected by science.[30]

It would be difficult to find a more direct expression of conjunction of the politics and ideology of science with communication. This ideological conditioning also gave media coverage of science one of its enduring metaphors, that of science as a competitive activity, in which scientists race against one another to make discoveries that will unlock vast potentials of power or wellbeing.

Imaginations of science

Nelkin observes that science coverage in the press at this time typically portrays science as a series of dramatic events in which competition plays a principal role: imagery replaces content.[31]

Science coverage had a frame, a term Nelkin borrows from Todd Gitlin: -- "a persistent pattern of cognition, interpretation and presentation, of selection, emphasis, and exclusion."[32] The frame of the late 1950s added a new element to the coverage that had, since the Second World War, covered science and technology as "breakthroughs," or "revolutions," at which the journalists and the audience were invited to marvel and awe. One of the key items in the metaphoric repertoire of science coverage became that of scientific knowledge as the most important resource of the nation. This was most obviously seen in the articles about the "crisis in science education" and the representation of science and technology as a strategic resource, threatened by being depleted as the quality of science education is allowed to deteriorate. The "the two cultures" metaphor was created at this time by C.P. Snow, and invoked on both sides of the Atlantic to suggest that the quality of society was threatened, including its essential democratic political structure, if the divisions between competencies in the sciences and the humanities were allowed to widen.[33] But as Nelkin notes, the popular treatment of these themes in science reporting did not promote any equality of relations between the two cultures:

> By stressing the gap between fuzzies and techs, ... between the needs of society and the availability of people with technical skills, these reports on science education reinforce the mystification of science. And by idealizing technical professions, they oversimplify both the meaning of science literacy and the actual role of science as a national resource.[34]

As the revitalized US President's Scientific Advisory Committee formulated it in 1959:

> A democratic citizenry today must understand science in order to have a wide and intelligent democratic participation in many national decisions....There is, therefore, no escape from the urgency of providing high-grade and plentiful adult education in science now, planned for those who are unprepared even in the fundamentals.[35]

Dornan, in his examination of the creation of the 'problem' of science and the media in the discourse of academics, decisionmakers and scientific journalists, suggests that the problem was seen as one of widespread public ignorance exacerbated by a "flawed

and opportunistic" coverage. The necessary antidote, and the strategic importance of the media, lay in a new type of science journalism that was being advocated:

> [O]ne that will concentrate its attention on processes of investigation (and therefore on academic inquiry); that will be motivated by the need to educate the laity in the contents and methods of science; that will be answerable to the scientific community for the accuracy of its coverage; and although perhaps critical of science in specific instances, will ultimately have as its aim the promotion of scientific interests and the creation of a public appreciative of science, accepting of its findings, and supportive of its efforts.[36]

Such improvements in science journalism might provide the necessary antidote to the low salience of science for the public. And as for the larger ideological project:

> [A] population sufficiently attentive to science, and adequately apprised of the rigour of its methods and the value of its investigations, would be, if not more 'rational,' then certainly more inclined to listen to 'reason.'[37]

And in returning "reason" to the public, science popularization also restored authority to science and to the political, social, and economic structures in which it was increasingly implicated.

As a reward, total federal expeditures on research and development increased by 42 percent over the 1960-1968 period, representing a growth rate of close to 5 percent a year. Support for basic science rose even more precipitously, doubling over the same years.[38]

Part two: The sorcerer's accomplice, 1966-1976

The period from the mid-1960s to mid-1970s saw science's political fortunes tumble, as cultural frames shifted and disenchantment with science and many other institutions lead to the criticism of them in fundamental ways as alienating and anti-humanistic, reducing experience to the one-dimensional. The conventional political agenda changed, and with it the resource allocations to science -- science policy shifted support to the application of knowledge in forwarding specified social goals. Funding for basic research dropped under two administrations -- Johnston and Nixon -- falling by over 10 percent between 1968 and 1971 and then staying practically constant for the next four years.

But perhaps a more significant causal factor, according to some commentators, was the change in industrial strategies on the part of the large corporations. As the payoffs from innovations seemed delayed or increasingly reduced because of market pressures for more immediate gains, emphasis shifted from pure research and development to process improvements, away from expansion of capacity to optimization of existing production. Industry-funded research declined more sharply than publically funded research: a drop of 37 percent in the period 1966-1972.

Science policy and funding was restored in a manner that linked science ever more closely with the dominant economic structures. The implicature of science and the economic structure was reinforced by the recession of 1973 and 1974. Searching for a way out, and for reasons for the poor performance of the American economy in those years, economists noted that it was the science-based industries that remained dynamic -- microelectronics, pharmaceuticals -- and that these industries exhibited a compounded growth triple that of low technology industries. Extending the analysis to factors within those industries that contributed most significantly to growth, it appeared that investment in research and

development was strongly correlated, in fact, with performance in manufactured exports, direct foreign investment, and licensing: some 30 percent of expected return on expenditures in research and development came from some form of technology transfer.

The conclusion that emerged was that research and development make fundamental contributions to capital expansion in creating new products and new production processes based on advanced scientific or technological inputs. As intellectual capital, "scientific resources and the aptitude for technological innovation,...[constitute] the major asset of industrialized nations in the new modes of international competition and interdependence," as the OECD (Organization for Economic Cooperation and Development) was to summarize the developments of the decade.[39] The perception of science as strategic economic capital (in addition to having strategic Cold War importance) had important consequences for both the internal organization of the scientific community, and the nature of the larger social and political relationships of science...and the nature of the discourse about them. As Dickson points out:

> Science as an essentially undirected activity, even if carried out with the promise of long-term technological spin-off, fit easily within traditional concepts of academic scholarship, requiring for example, the complete freedom of scientific communication and exchange for the growth of scientific knowledge. Science as an economic commodity, however, has very different implications. The language itself is the language of the boardroom and corporate investment planning. As "knowledge capital," research is expected to generate an appropriate return....Control of knowledge becomes a vital weapon in the corporate armory -- and thus a crucial factor in determining the form of the new politics of science."[40]

The changing cultural frames of the decade 1966-1976 brought charges that the application of "intellectual capital" primarily to technical and economic goals demonstrated a harmful social and environmental indifference. In the early to mid-1960s the consequences of the postwar economic and technological development were examined critically for the first time -- fission, elucidation of DNA, transistor develpment, chemical development -- particularly the dimensions and severity of the impact on the environment.

In 1962, Rachel Carson published *Silent Spring*, an indictment of the technological abuse of the environment resulting from indiscriminate use of pesticides, particularly DDT.[41] A year later, Barry Commoner organized the Scientist's Institute for Public Information, adding a critical voice to the hymns of public praise. These, among many other agitations, turned to a larger questioning of the philosophical underpinnings of science and science policy.[42]

Typical of the tenor of the new criticism was a paper delivered to the AAAS annual meeting in 1966 by historian Lynn White Jr., which created an enormous stir inside and outside the scientific community. White challenged some deep cultural assumptions of both science and the larger society by suggesting that there was a dangerously anthropocentric religious foundation to the ideas of human domination of nature and perpetual progress that had produced an environmental crisis (and by attribution of cause, a crisis in the public belief and warranting of science).

> [M]ore science and more technology are not going to get us out of the present crisis until we find a new religion or rethink our old one (for they are permeated with orthodox Christian arrogance toward nature)....We shall continue to have a worsening ecological crisis until we reject the Christian axiom that nature has no reason for existence save to serve man.[43]

Jerome R. Ravetz, in *Scientific knowledge and its social problems*, published in this period, likewise warned that science might be in decline and in danger of dissolution because of its seeming alienation from a changing social context.[44] Ravetz characterized science as "technocratic." While science had penetrated society in applied forms in technology, it had itself been penetrated and industrialized by the domination of capital-intensive research. The consequent loss of boundaries and unique codes of behaviour and ideals thought to govern science threatened its autonomy. Scientists were in danger of becoming simply labour resources as their production was appropriated: science was victimized by its own efficacy. Decisions on funding had become contingent on potential practical or selectively beneficial outcomes, determined by "an oligopoly of investing

agencies." The community of truth-seekers was being broken, and social pressures were turning scientists into knowledge entrepreneurs. Ravetz warned that as science was seen to be a more dubious, interested activity, the struggle to control and appropriate its product would intensify. In this period the autonomy and resource base of science was under seige from all sides.

Reenchanting science

In responding to the criticisms and the budgetary strictures that they produced in public funding in the 1970s, the science community sought both pragmatic ways to apply science to socially responsible ends, and political strategies to control such criticisms. It was in attempting to counter the critical "anti-science" movements that the discourse of science popularization became even more strongly of ideological character: "We must try and reverse the disillusionment with science of the American public," said Glenn Seaborg, president-elect of the American Association for the Advancement of Science in 1970, in an address to the annual meeting that year. "And we're going to have to make science more relevant to human problems than we have in the past."[45]

One of the means of returning and integrating social values into scientific and technological decision-making was through the introduction of regulatory control in the areas of health, environment, and safety.

Pressure for more explicit and direct intervention -- such as the actions of the municipality of Cambridge, Massachusetts in trying to prevent recombinant DNA experiments at MIT until issues of possible health hazards were resolved -- represented a political challenge that required a different strategy. The political response of the scientific community was to attempt to regain control and redefine the boundaries between science and the political process in a way that reasserted the principle -- now once again seemingly

threatened -- of intellectual freedom and autonomy from control that had been negotiated immediately after the Second World War.

One of the sites from which the discourse was mounted was provided by the technology assessment process. Technology assessment seemed to transport social and environmental values into the decision-making system, and integrate them in a way that did not perturb the dominant mechanisms controlling science and technology. As Dickson notes, technology assessment was deliberately pursued as a way of reducing political conflicts to technical terms, and favouring technocratic decision-making modes over democratic modes. Larger demands for a redistribution of power were deflected, in science, at least, by a strategy of transforming the process into the goals -- a fine tuning of technological development toward addressing social needs, rather than a re-evaluation of the desirablity of the methods used to achieve them.

> The political potential of technology assessment as opening a path to a national technology policy held both a promise and a threat to the scientific and corporate communities. The promise lay in the opportunity that, if kept under control, it presented those who believed that social problems could be solved through a consensus of scientific experts with a mechanism for injecting their solutions into the public policy field; the threat lay in the danger, as these two communities saw it, that the same mechanism could be used for imposing socially determined objectives on their activities, i.e. for public control of what many considered should be private decision-making. The scientific community ... recognized in the public questioning of science a threat to its own political autonomy unless this questioning was deflected....[16]

The establishment, in 1972, and subsequent functioning of the Office of Technology Assessment and the resolution of public concerns over the potential health hazards presented by recombinant DNA research reflected the way in which the political autonomy of scientific decision-making was maintained. The challenge to it represented by increased awareness of the broader social impacts of science and technology was contained and controlled. In both cases, the institutionalization of a processs of evaluation in the OTA and the forum presented by the recombinant DNA debate, the field of discourse was redefined in a way that restricted the political import. In ostensibly attempting to raise the

level of political discourse over contentious issues involving science and technology, the discourse was transformed from an essentially political one to a technocratic one in which the terms of the debate and their valuations were set by the scientific and corporate interests.

The instrument of truth

Press coverage of science in the 1970s was part of the larger framing of social criticism in which concerns about the social and environmental impacts of science and technology predominated. Coverage was extended from the products of research and development to social implications: the conflicts between the two cultures became a focus. Disputes over recombinant DNA research, food additives, supersonic transport (SST), and nuclear power were reported more often.

This is the frame, from the mid-1960s to the mid-1970s, in which science was feared, sometimes reviled, as the cause of environmental and social risks. Coverage shifted "from the conquests of science and technology to their consequences, from the celebration of progress to a more critical reflection about the problems brought about by technological change."[47]

The public attitude toward science near the close of this period was one of "deepening disillusionment,"[48] and scientists were concerned with the "deep mistrust of science and technology...expressed by many of our society today."[49]

> Essays and books by prominent writers with wide readership broadcast the doubts concerning the explanatory power that was thought to be resident in science itself. The counterculture essayists, with large audiences of their own, would drastically change the predominant balance favoring rational elements. Matters are only made worse by some reactions from within the scientific community, which often seem, and sometimes also *are*, self-serving. These frequently confuse the obligation to take seriously the state of public understanding of science with the short term benefits that would accrue from better public relations.[50]

But some, like Amitai Etzioni and Clyde Nunn, disagreed. (Etzioni, director of the Center for Policy Research at Columbia University, was also a key figure in institutionalized science popularization: onetime member of the editorial board of *Science* magazine, the National Science Foundation Information Council, and the AAAS Committee on the Public Understanding of Science.) In a review of survey and poll data Etzioni and Nunn set out to explore:

> ...[H]ow widely institutions embodying rationality are accepted, or at least tolerated....the status of science not only as a social institution in its own right, but also as an indicator of the changing status of "rationality," one of the foundations of modernity in our society.[51]

Gauged from the data gathered in a number of surveys undertaken from 1957 to 1973, science gained in public confidence relative to other institutions. In the spring of 1973, science ranked second only to medicine in eliciting "great confidence." The general disaffection from authority and loss of confidence of the period affected science, but to a lesser relative degree. They suggested that as science:

> ...seems to command more confidence than many other institutions, particularly government and education, science cannot hope to gain legitimation from public officials and educational leaders. Indeed, government and education may, in future years, turn to scientists for legitimation, since only those who are trusted can lend trust.[52]

They also suggested that confidence was correlated with education and that in any strategy to obtain public support, scientists might "recommend a large-scale campaign of education."[53] The education and income correlations performed on the data suggested to Etzioni and Nunn that the major source of distrust for science was "not a massive monolithic counterculture group."made of educated middle and upper-middle classes, but "lower-status, less-educated groups, and people who live in less affluent parts of America."[54] The alienation from rationality and its institutions was strongest among the usual malcontents: the poor, the disadvantaged, the poorly educated.

A number of factors combine, however, to minimize the likelihood that science will come under severe and damaging attack from this quarter. The kinds of people we have been discussing tend to be politically inactive and unsophisticated, and to command limited political resources. Although, when they are aroused politically, they often tend to gravitate toward extremist movements and charismatic demagogues, their overall record of political participation in the United States is weak and sporatic. They are less likely than better educated, wealthier, more tolerant urbanites to engage in such traditional political acts as voting, writing letters to their elected representatives, campaigning, and contributing time or money to a political campaign.[55]

Etzioni and Nunn went on to point out that a small erosion of faith in science among the "politically active social strata" would be of greater consquence than "massive disaffection among the geographically, politically, and economically peripheral members of society."[56] Given this stratification and its significance for the public warranting of science, a major focused campaign "to inform and educate the public would yield more understanding and support than such campaigns usually yield."[57]

It is an ironic contradiction, but perhaps an inevitable consequence of the increasingly ideological character of the public discourse of science at this time. The purpose widely argued and accepted for science popularization was to enable and enlarge democratic participation, promoting more responsible modern citizenship in an age of science. Given something of an experience of what the realization of that purpose might involve, the scientific community turned it into a baldly instrumental strategy, and began more systematic efforts to communicate with control.

The scientific community came to realize the double-edged importance of public awareness of science -- and especially the desirablity of inculcating a favourable attitude -- through the crises of the 1970s. In order to deflect future crises, and evolve a strategy, this community of interests began to examine the nature of "normal" public awareness, and build and strengthen the public awareness of science as originally conceived in the 1950s -- but with a new precision. New communications research begun in 1979, based on a model initially developed in the 1950s, moved popular science communication further

away from enlightening a public with science information and toward targetting a

constituency with ideology.

Part three: Attending to science, 1976-1986

In an earlier attempt to reconcile problems in the area of foreign policy immediately after the Second World War, Gabriel Almond had developed an analysis of political process that acknowledged that the public remained uninterested in non-salient policy issues unless provoked by some dramatic turn of events. During normal periods, only a relatively small, self-selected segment of the public monitored and generally attended to such issues. This "attentive" public was characterized as having a high level of interest coupled with a functional level of knowledge of a low-salience issue.

Science and science policy toward the end of the turbulent 1960s and the mid-1970s were construed similarly as having a history of low salience under normal circumstances. Now the lessons learned in the crisis of the Second World War about the strategic importance of a polity aware (if not informed) of foreign policy were applied to science. Jon D. Miller cautioned:

> The emergence of numerous science policy issues on the national political
> agenda reflects the growing importance of organized science to the welfare
> of society. Yet, only a minority of the citizenry has sufficient interest and
> knowledge to debate and resolve the issues. Inevitably, the issues are
> defined and resolved outside the electoral process. Policies that will affect
> all Americans are determined by a relatively narrow segement of the
> citizenry.[58]

Much of this concern with the "threat" low salience posed, of course, sprang from a dismay over the perceived erosion of resources for science. With the 1970s, the 12-year "golden age" of science -- following the launch of Sputnik, and until the manned moon landing of 1969, when the growth rate of federal funding for research and development was substantial -- came to an end in a plateau that was to last until the mid-1980s.[59]

The Almond model provided the scientific community with a useful framework for understanding the formulation of public policy in low salience areas, and the beginnings of a communications strategy for science.

The model, a pyramid-like stratified classification of actors in the political process, rests on a large base of uninterested general public and becomes increasingly specialized in interest and political behaviour. The next tier is the attentive audience, followed by nongovernmental policy leaders, and at the top, political decisionmakers. According to Miller, its principal exponent in the area of science policy, this model does not "mitigate the basic tenets of a democratic society, but rather reflects the differential ability of individuals in our society to devote the necessary resources, especially time, to become and remain informed and active in numerous policy areas."[60]

Individuals, faced with the pressures of competing and attractive demands on time and resources, choose to allocate them to a few limited areas. This behaviour is termed, interest specialization.

Already limiting interest and behaviour in interest specialization, individuals further restrict their political horizon through issue specialization. The threshold of information necessary for informed or knowledgeable participation in many issues -- and especially those with scientific and technological content, it is argued -- has risen such that it is increasingly difficult to attend to more than a few issues at any time.

Science communication enters this model when one looks more closely at the actors in the various strata. Communication of scientific knowledge, or rather communication about the scientific process of generating knowledge, is one of the principal items of the science policy agenda for scientists -- as represented by the members of the "nongovernmental policy leaders" stratum.

Miller and coworkers surveyed leaders of science policy in 1981: it is apparent that this *policy* leadership was also coextensive with leadership in the scientific community *per se*[61]:

> [The leadership group was]...broadly representative of the major disciplines and professions within the scientific community, predominantly from universities but with significant representation from both the for-profit sector and from independent research centers...The policy leaders have a strong organizational base within the scientific community.[62]

(Interestingly, Miller and Prewitt included science journalists for national distribution broadcast or print organizations among this group -- a tacit acknowledgment of the role of popularization in the politics of science.)

The policy leadership group reported a "high level of persuasive activity" in engaging in attempts to influence both colleagues within the scientific community and outside of it.[63]

This group rated the inadequate public understanding of science as a major problem of science policy, second only to the level of funding for basic scientific research, and ahead of such items as the obsolescence of scientific instrumentation, the quality of precollegiate science education, the availability of training and research opportunities for young scientists, incentives for industrial research, and the application of new scientific knowledge toward end products and use.[64]

But a feature of the political process that this model was constructed to describe is how the policy process works, and can be regarded in any way as democratic, when there is limited participation by "the public." Why then should a concern with the public awareness of science feature so highly on the agenda of the policy leadership of the science community?

In the activity of science policymaking modelled, most decisions are taken at the top levels of the participant pyramid -- the decisionmakers and policy leaders acting

independently of the policy public. These elites usually only have recourse to the larger public under three general circumstances. When there are "serious differences among science policy leaders," appeals are made to the attentive and interested publics for support of contested policy positions -- nuclear power generation is an example. When the policy demands made by policy leaders are rejected by decisionmakers, attentives, principally, are enlisted in attempts to leverage decisionmakers. Finally, when issues become politicized as election issues or as referenda topics through special mobilization of opinion and support, the public participates -- examples being issues such as flouridation, food irridation, food additive safety, and the like.

> ...[I]t would appear that the stratified model for the formulation of science policy provides an accurate description for normal times. The policy agenda formulated by the leadership group provides the framework within which science policy develops. When disagreements arise among science policy leaders themselves or between the policy leaders and decisionmakers, efforts may be made to mobilize the attentive public for science policy.[65]

Unwelcome attentions

I have suggested that the funding cycles identified by Dickson, Greenberg, and Miller himself, and the history of the rise and fall of the growth rates that produce those cycles are sources of "disagreement" between decisionmakers and the science policyleaders. There is also an ongoing "disagreement" in cultural values between the scientific community and society at large that periodically assumes dramatic proportions -- the period of the mid-1960s to mid-1970s was one, and there are signs that we may be entering another (the renewed questioning of nuclear energy policy in the fallout from Chernoybl, a similar disillusionment with the space programme following the shuttle disaster, and the increasing disillusionment with science in its complicity in the "Star Wars" lap of the arms race.)

According to the series of biannual surveys conducted for the National Science Foundation, science and its principal audience and constituency have been of similar minds

on most of the items of the science policy agenda in the two broad areas of concern,

acquisition of resources and preservation of independence.[66]

The basic objectives of the scientific community have been simple and
straightforward, according to Miller: "independence of inquiry and adequate support to
sustain the scientific enterprise."[67]

The attentive public supports the policy leaders' emphasis on the need for
increased funding for scientific research and development, but was interested primarily in
the utilization of science and technology in the resolution of problems or the improvement
of well-being particularly in the fields of health, and education:

> The attentive public appeared to view support for scientific research as one
> of several desirable governmental activities, but refused to accord it special
> status -- including an exemption from federal budget reductions.[68]

Although both the attentive public and the policy leaders rejected the idea that
there actually is a growing trend of public distrust in science -- and the attitudinal survey
work seems to confirm that science continues to enjoy strong support and high
expectations -- both groups expressed a concern about the potential for public distrust of
science.

The policy leaders gave a relatively low estimate of the public's knowledge of
science, suggesting that only about 14 percent of the public understood the meaning of
scientific study, or specific concepts such as radiation (estimated public understanding: 22
percent), or DNA (estimated public understanding: 9 percent). The public believe
themselves to be better informed than the estimates given by the policy leaders, but when
tests of scientific literacy were applied, proved to have greatly overestimated their
understanding -- only half of the respondents in a 1981 survey who estimated that they
themselves understood the process of scientific study could provide a minimally acceptable
definition. Revising these self-estimates of the level of public understanding, Miller

calculated that the public estimate of understanding was closer to that of the policy leaders, placing it at 11 percent.[69]

In fact, Miller susequently developed and applied "an index of scientific literary that incorporated three dimensions: an understanding of the purposes and processes of scientific investigation, a minimal command of basic scientific constructs like radiation or DNA, and an awareness of some current policy issues," to discover than only 7 percent of the total adult population of the U.S.A. could be deemed scientifically literate. Perhaps more disturbingly, when correlated with the other measures of attentiveness, Miller noted that only about one-third of the attentive public for science policy could meet the minimum qualifications of scientific literacy.[70]

On another critical area of preservation of independence, the attentive public returned unsettling evidence of deviation from the science policy agenda. Whereas the policyleaders appeared to be not uncomfortable with the degree and extent of government regulation of scientific and technological activities, the attentive, interested and nonattentive publics:

> ...all indicated a willingness to restrain various scientific activities, especially research concerning new life forms and increased control of gender determination. While the propensity of a substantial portion of the public to restrain scientific research has not become a widespread political problem for the scientific community, it remains a potential source of exploitation and difficulty.[71]

Perhaps even more troubling for the authority of science, is the contradiction between the public and policy leaders' attitudes concerning creationism (the claim of various religious groups that the Biblical version of creation should have equal status in science classes with the evolutionary theory). Almost unanimously rejected by the scientific community, creationist theory was supported by fully two-thirds of the *attentive* public. As Miller notes:

The low level of scientific literacy among the attentive public is a serious barrier to communicating sophisticated information about science policy, or to effectively mobilizing the attentive public to work for specific policy outcomes. The propensity of the attentive, interested and nonattentive publics to restrain scientific investigation in selected areas [and] to support the creationist position in regard to science education illustrates the potential threats to the independence of the scientific community that may result from low levels of scientific literacy.[72]

The attempts to mobilize the attentive public on specific issues have been rare, the accounts given of the efforts made to establish the Atomic Energy Commission as an exclusively civilian-controlled organization, and the establishment of the National Science Foundation are generally the only instances in which the attentive constituency has been explicitly called upon. In addition, Miller points out that the policy leadership of science is both unpracticed in mobilization -- having been able to obtain sufficient resources and maintain an acceptable measure of independence without recourse to any kind of broader political struggle -- and somewhat wary of the attentive public. Enlisting the support of a political constituency would mean opening the science policy process and to an extent the scientific community and its activities -- albeit in a highly controlled and limited fashion -- to a broader participation.

In very general terms, for policies other than the acquisition of resources, the primary use of the attentive public for science policy is defensive, that is, to protect against the erosion or reversal of the postwar relationship between government and organized science.[73]

Motivated communication

It should be clear that the renewed interest in science popularization on the part of the scientific community, decisionmakers, and interested participants in the media and corporate sectors is the result of an increased awareness of the political and economic power that could be derived from a science constituency. This new awareness was itself the product of the positive experience in the "golden age" of science funding and harmonious discourse on science from the Second World War until about the mid-1960s. It

was spurred equally by the negative experiences of the mid-1960s to mid-1970s, which saw encroachments on both the funding and cultural authority of science.

Dickson suggests more directly that much of the recent efforts to create science literacy and popularize science are transparent efforts to restore to the scientific community the status and resources -- and particularly the political power and autonomy -- eroded by the attempt to bring social values and controls to the scientific enterprise in the decade of the mid-1960s to mid-1970s. Programs of science education in the schools and the project of increasing science communication to the attentive and interested publics have as their objective not the promotion of critical scrutiny of science or questioning the conjunction of scientific and economic power in the hands of private decisionmakers, but the legitimation of the scientific controls in social life, and the willing acquiescence to them.

In this, an ideology conditions science communication. And as Dickson forewarns:

> ...[U]nless the results of decisions made about science and its applications, as well as the procedures by which these decisions are made, are subject to continous public scrutiny, the danger remains that the democratic process will have been replaced by negotiations among the members of a relatively small, like-minded elite, justifying the consolidation of their power over important scientific and technological choices by referring to the need for an efficient response to both commercial and military threats, and thus conveniently ignoring the broader political implications of their actions.[74]

Some other means must be found to communicate about science and technology other than the ideologically controlled ways currently dominating. Not to do so will allow critical political issues to continue to move further and further away from the domain of democratic decision-making under the guise and control of popularization.

This motivated communication on science draws its ideological coloration and force from a more deeply embedded ideology within scientific practice. The next chapters examine the ideology conditioning popular science communication closer to its source, and propose some analytic tools for recovering it from popular science texts.

NOTES: Chapter One

1   David Dickson, *The New Politics of Science* (New York: Pantheon, 1984); Daniel S. Greenberg, *The politics of pure science* (New York: New American Library, 1967); Don. K. Price, *The Scientific Estate* (Cambridge, Mass.: Harvard University Press, 1965), *Science Indicators* 1970-1985 (Washington, D.C.: US Government Printing Office [for the National Science Board, National Science Foundation], 1971-1986).

2   Dickson, Ibid.; Greenberg; Ibid.; National Science Board, Ibid.

3   Vannevar Bush, *Science: the endless frontier* (Washington, D.C.: U.S. Government Printing Office, 1945; reprinted Washington, D.C.: National Science Foundation, 1960.)

4   Bush (1960), *Frontier.*

5   The policy process and the political struggle surrounding the proposals to establish this administrative structure in the National Science Foundation are, in microcosm, a history of the changing relations of science and society. See Greenberg, pp. 97-122; Dickson, pp. 25-27; Price; Daniel Kevles, "The National Science Foundation and the debate over postwar research policy," *ISIS*, Vol. 88(1977), pp. 5-26; and J. Merton England, *A Patron for Pure Science: The National Research Foundation's Formative Years, 1945-57* (Washington, D.C.: National Science Foundation, 1982).

6   The government directly mandated most of the money spent on research and development, however. In 1955, most (80 percent) of the support was for military research and development undertaken by the Department of Defense. Another 10 percent went to the Atomic Energy Commission, and two percent to the Department of Health, Education, and Welfare. The National Science Foundation administered only 0.1 percent of U.S. federal funding for research and development.

7   Don K. Price, *The Scientific Estate* (Cambridge, Mass.: Belknap Press of Harvard University Press, 1965), p. 37.

8   Price, p. 19.

9   Price, p. 137.

10   McGraw-Hill Corporate Resource Center, *Science Illustrated* file. This account is drawn from Bruce V. Lewenstein, "Public communication of science after World War II," paper presented at the Canadian Communications Association Annual Meeting. Session on Science and the Media, 20 May 1987, 22 pages, mimeo.

11   *Scientific American* archives, file cabinet 2, "Summary 1946" file, "Proposal for a monthly magazine," p. 2; "An announcement to our readers," *Scientific American*, December 1947, 177(6): 244. Quoted in Lewenstein, p. 5.

12   Lewenstein, p. 6.

[13] AAAS, Constitution, Article II, Objectives, *American Association for the Advancement of Science Handbook: Officers, Organization, Activities, AAAS 1986-1987* (Washington, D.C.: AAAS, 1986), p. 133.

[14] AAAS, "Policy Statement, Arden House," *American Association for the Advancement of Science Handbook: Officers, Organization, Activities, AAAS 1986-1987* (Washington, D.C.: AAAS, 1986), p. 127.

[15] Ibid., p. 128.

[16] Ibid., p. 128.

[17] Ibid., p. 128.

[18] Denis McQuail, *Mass Communication Theory: An Introduction* (London and Beverly Hills: Sage, 1983), pp. 176-180.

[19] Among the classic studies: Bernard Berelson, *Content Analysis in Communications Research* (Glenoe, Ill.: Free Press, 1952); Bernard Berelson, "The State of Communication Research," *Public Opinion Quarterly*, 23(1) 1959, 1-6; Harold Lasswell, "The Structure and Function of Communications in Society," in L. Bryson (ed.), *The Communication of Ideas* (New York: Harper, 1948); Harold Lasswell, D. Lerner, and I. de Sola Pool, *The Comparative Study of Symbols* (Stanford: Stanford University Press, 1952); Paul F. Lazarsfeld, *The People's Choice* (New York: Duell, Sloan and Pearce, 1944); S.A. Star and H.M. Hughes, "Report on an Education Campaign: The Cincinatti Plan for the UN," *American Journal of Sociology*, 55 (1959), 389-400.

[20] R. Hutchins, Commission on Freedom of the Press, *A Free and Responsible Press* (Chicago: University of Chicago Press, 1947); and F. Siebert, T. Peterson, and W. Schramm, *Four Theories of the Press* (Urbana, Ill., University of Illinois Press, 1956).

[21] As McQuail has summarized them:
".media should accept and fulfill certainobligations to society;
.these obligations are mainly to be met by setting high or professional standards of informativeness, truth, accuracy, objectivity and balance;
.in accepting and applying these obligations, media should be self-regulating within the framework of law and established institutions;
.the media should avoid whatever might lead to crime, violence or civil disorder or give offence to ethnic or religious minorities
.the media as a whole should be pluralist and reflect the diversity of their society, giving access to various points of view and to rights of reply;
.society and the public, following the first named principle, have a right to expect high standards of performance and intervention can be justified to secure the, or, a, public good;" pp. 91-92.

[22] Lewenstein, p. 14.

[23] AAAS Board Minutes, 3-4 March, suppl. to item 22, "Memorandum, Preliminary Draft, 20 Feb 1956, p. 3. Quoted in Lewenstein, p. 14.

[24] *NASW Newsletter*, March 1956, pp. 10-12. Quoted in Lewenstein, p. 14.

25   Hillier Kriegbaum, *Science, the News, and the Public* (New York: New York University Press, 1958); *Satellites, Science, and the Public* (Ann Arbor: Survey Research Center, Institute for Social Research, University of Michigan, 1959).

26   CASW, "Corporate papers" file, certificate of incorporation, 6 Jan. 1960; and Ibid., certificate of extension of powers, Nov. 1960. Quoted in Lewenstein, p. 15.

27   Chris Dornan, "The 'problem' of science and the media in academic discourse" (Phd. Thesis, Special Programme in Communications, McGill University, Montreal, 1987), p. 4.

28   Dickson (1984), p. 28.

29   Dornan, p.34.

30   Warren Weaver, "Science and the Citizen," *Science* 126(3285), 13 December, 1957, pp. 1225-1229. Reprinted in *Science and Imagination: selected papers of Warren Weaver* (New York: Basic Books, 1967), p. 67.

31   Dorothy Nelkin, *Selling Science: how the press covers science and technology* (New York: W.H. Freeman and Co., 1987).

32   Todd Gitlin, *The Whole World is Watching* (Berkeley: University of California Press, 1980), p. 7, quoted in Nelkin, Ibid., p. 9. Nelkin gives science coverage three frames: they fit like neat overlays on the cycles of science politics and funding characterized by Greenburg and Dickson. The present period under consideration -- the period after the Second World War until the mid-1960s -- was the age of heroic competition and breakthroughs. In the next frame, from the mid-1960s to the mid-1970s, science was feared, sometimes reviled, for placing the environment and society at risk. The enthusiasm for science and technology has returned in the 1980s, tempered somewhat as Nelkin says, by the awareness of entailed risks and the experiences of Love Canal, Three Mile Island, Chernobyl, and the Challenger explosion.

33   C.P. Snow, *The Two Cultures and the Scientific Revolution* (The Rede Lecture, 1959) (Cambridge: Cambridge University Press, 1959).The ideas had been formulated and aired in a previous form in "New Minds for the New World," *New Statesman,* 6 September 1956.

34   Nelkin, p. 24.

35   *Education for the Age of Science* (1959), quoted in Dornan, p. 36.

36   Dornan, p. 58.

37   Dornan, p. 47.

38   Dickson (1984), notes that the main recipients of public funding at this time were universities, which had steadfastly rejected public financing before the war in order to preserve the principles of autonomy. The strategic granting programs of the government tripled the expenditures of universiteis on basic research, rising from $433 million in 1960 to $1,649 million ($1.649 billion) in 1968.

39 Organization for Economic Cooperation and Development, *Technical Change and Economic Policy* (OECD: Paris, 1980), p. 20.

40 Dickson, p. 33.

41 Rachel Carson, *Silent Spring* (New York: Houghton Mifflin, 1962).

42 Other key texts that set the agenda.of technological and scientific criticism in the public domain are: Barry Commoner, *The Closing Circle* (New York: Knopf, 1972); William Leiss, *The Domination of Nature* (New York: Braziller, 1972); Jerome K. Ravetz, *Scientific Knowledge and its Social Problems* (New York: Oxford University Press, 1970); Theodore Roszak, *The Making of a Counterculture* (New York: Doubleday, 1969); Jacques Ellul, *The Technological Society* (New York: Vintage, 1967); Jurgen Habermas, *Knowledge and Human Interests* (Trans. J. Shapiro. Boston: Beacon Press, 1971), and *Toward a Rational Society* (Trans. J. Shapiro. Boston: Beacon Press, 1970); Herbert Marcuse, *One-Dimensional Man* (Boston: Beacon Press, 1964); Stuart Udall, *The Quiet Crisis* (Washington, D.C.: US Government Printing Office, 1963); Lynn White Jr., *Machina ex Deo: essays in the dynamism of Western culture* (Cambridge, Mass.: MIT, 1968)

43 Lynn White Jr., "Historical roots of our ecological crisis," *Science*, Vol. 155, 1967. Quoted in Michael Tobias (Ed.), *Deep Ecology* (San Diego, Calif.: Avant, 1984), p. 36. Reprinted in White, *Machina ex Deos*, Op. cit.

44 Jerome R. Ravetz, *Scientific knowledge and its social problems* (New York: Oxford University Press, 1970).

45 Quoted in Dickson (1984), p. 31. From "The painful struggle for relevancy," *Business Week* (January 2,1971)

46 Dickson, p. 238.

47 Ibid., p.10.

48 *Time* (April 23, 1971), p. 83.

49 Jerome B. Wiesner, "Technology is for mankind," *Technology Review* (May 1973), p. 10.

50 Gerald Holton, "Preface," in Gerald Holton and William A. Blanpied (Eds.), *Science and its Public: the changing relationship* (Dordrecht-Holland/Boston-U.S.A.: D. Reidel, 1976), xi. (Boston Studies in the Philosophy of Science, Vol. 33.)

51 Amitai Etzioni and Clyde Nunn, "The public appreciation of science in comtemporary America," in Gerald Holton and William A. Blanpied (Eds.), Ibid., p. 229.

52 Etzioni and Nunn, p. 233.

53 Etzioni and Nunn, p. 235.

54 Etzioni and Nunn, p. 236.

55 Etzioni and Nunn, p. 237.

[56] Etzioni and Nunn, p. 238.

[57] Etzioni and Nunn, p. 241.

[58] Jon D. Miller, *The American People and Science Policy: the role of public attitudes in the policy process* (New York: Pergamon Press, 1983), p. 2.

[59] At the time of Miller's study, 1983, he reported that:
Viewed in constant dollar terms, it is clear that the level of federal support for research and development generally, and for basic research in particular, has been stable for over a decade. The sharply increasing number of current dollars has provided an illusion of growing resources. The reality of stable or declining resources for science and the illusion of substantial budgetary increases have resulted in confusion in the public mind and in the policy process. (Miller (1983), p. 21.)

[60] Miller (1983), p. 25.

[61] Following the general structure of the scientific community described by Don K. Price in *The Scientific Estate* (Cambridge, MA: Belknap Press of Harvard University Press, 1965), Miller and colleague Kenneth Prewitt (published later as J.D. Miller and K. Prewitt, *A national survey of the nongovernmental leadership of American science and Technology*. A report to the National Science Foundation under NSF grant 8105662. DeKalb, IL.: Public Opinion Laboratory, 1982lr) established a survey universe selecting people considered to be science and technology policyleaders who either:
   1. were officers of national professional or scientific societies or associations;
   2. had testified before a committe of the Congress on a scientific or technological issues within the previous two years;
   3. had served on a major executive branch science or technology policy advisory committee within the previous two years;
   4. had published a book or refereed journal article on a science or technology policy matter within the previous two years;
   5. were members of the National Academy of Sciences, the National Academy of Engineering, or the Institute of Medical Sciences;
   6. had won a Nobel Prize in a scientific field or a Field Medal (mathematics);
   7. had served as officers or on the board of directors of one of the 20 top science or engineering corporations on the Fortune 500 list; or
   8. were full-time science journalists for a national distribution broadcast or print organization.

[62] Miller (1983), p. 34. About half of the policyleaders surveyed were members of the editorial boards of scientific or engineering journals (most frequently these editorial board members were from university science sectors).
   About one-quarter of the policyleader group were members of the governing boards of independent research laboratories or centres, with approximately the same proportion serving on the board of directors of for-profit corporations engaged in scientific or engineering activities.
   A large percentage (42%), of this stratum group were officers of national professional and disciplinary societies, and almost all (91%) were members of leadership organizations, such as the American Association for the Advancement of Science (AAAS).

[63] Miller (1983), p. 39.

64   Miller (1983), p. 56.

65   Miller (1983), p. 59.

66   *Science Indicators* 1970-1985, Op. cit., (Washington, D.C.: US Government
       Printing Office [for the National Science Board, National Science Foundation],
       1971-1986).

67   Miller (1983), p. 4.

68   Miller (1983), p.70.

69   Miller (1983), p. 72.

70   Jon D. Miller, "Scientific literacy: a conceptual and empirical review," *Daedalus*
       112(2), 1983, pp. 29-48.

71   Miller (1983), p.77.

72   Miller (1983), p. 78.

73   Miller (1983), p. 131.

74   Dickson, p. 314.

CHAPTER TWO:

THE CONDITIONED DISCOURSE

*[T]he conclusions of natural science are true and necessary, and the judgment
of man has nothing to do with them.*

> -Galileo Galilei (1564-1642)
> *A Dialogue Concerning the Two Chief World
> Systems.*[1]

The concern with public awareness of science, or science popularization, can be
seen to vary as a function of the political agenda of organized science, which itself varies
according to how adequately its institutional imperatives are met. In this context, science
popularization can be characterized as a component element of a discourse of legitimation -
- one which seeks to establish, maintain, or in some cases restore, the legitimacy or
authority of science in the relationship between science and the larger social order.

Popular science communication reproduces elements and concepts drawn from
science, but reproduces them in a way that transforms them as ideology.

This ideology constructs an ideal science, giving it a special status as rational,
uncontrovertable knowledge, and represents the social order constituted by the practices of
science as a meritocracy of truth.

In like fashion, the norms of "universalism, communalism, disinterestedness, and
organized skepticism"[2] that organize and bind the scientific community are reproduced in
the ideological communication of popular science to make scientists appear trustworthy
and authoritative.

Science is represented in popular accounts, therefore, as an ideal form of authorized knowledge, and likewise an ideal social organization that should be extended.

The ideological formulation of science in popular accounts is not unique to them. Science popularization draws on elements of a larger discourse of science. A dominant part of that larger discourse is a positivist representation of science as the only valid form of knowledge, an objective knowledge determined only by reality. In the following chapter, I will present a brief outline of recent work in the social analysis of science that questions that representation. I do so in order to suggest popular science communication draws its own force from the realization of an ideological potential in the positivist representation of science.

It is not my intention to contribute to the development of "antipositivist" theory of science, nor to rehearse the controversies among advocates of critical rationalism, critical theory, and the varieties of realism. It *is* my intention to use some of this work as contrasting concepts that reveal (or provide opportunites to consider) the elements of science that have been taken up and used improperly. Thus, without trying to decide the issues involved, I wish to use them as a platform for my own analysis.

The outline that follows will demonstrate, if nothing else, the hold that representations of science as a sort of realist epistemology or positivist methodology do have on any discourse about science.

Science has customarily been regarded as a special case, exempt from sociological analysis because of its uniquely rational espistemology. Although sociological analysis has been turned on science, the possibility of any social contingency influencing the form or content of scientific knowledge has consistently been refused. Social investigations of science have been limited to the social factors encouraging or constraining the autonomous activity of the production of scientific knowledge.

Thomas S. Kuhn provides a particularly useful perspective from which to examine science in search of the ideological potential realized in discourse. The concept of the paradigm can be taken up as an analytical tool and used to describe elements or processes of ideological potential in science, and their full-blown ideological simulacra in science popularization.

As Kuhn has suggested, the contingent or revolutionary nature of science is disguised or made invisible by the paradigm-directed history or authority in normal science that institutionalizes its knowledge as cumulative, progressing towards a stable truth. This institutionalization or transmission of paradigms is posited by Kuhn as the characteristic pattern of science. The transmission of the culture through written authorities in texts is itself also paradigmatic of science, establishing the knowledge-mediated relationship between the scientist and nature. The projection of these characteristics of science into the communication intended for a nonscientific audience produce an ideological effect.

Accounting for science

The first sociological accounts of science begin with Durkheim (although some would extend the tradition at least as far as Francis Bacon). Durkheim sought social origins to the basic categories of human thought and reasoning -- including such "scientific" forms as concepts of space. time. and force -- using evidence of the variation in these forms across cultures as evidence that they were, to at least some extent, socially conditioned.[3] But Durkheim drew back from a fully social explanation of science:

> From the fact that the ideas of time, space, class. cause or personality are constructed out of social elements. it is not necessary to conclude that they are devoid of all objective value. On the contrary, their social origin rather leads to the belief that they are not without foundation in the nature of things.[4]

Upon this foundation is built a theory that unites the physical and social sphere. Social concepts of time, for instance, are drawn from the regularities of social life, but

these regularities reflect the more basic and comprehensive regularities of the physical world. That is, concepts are formed in relation to processes in the physical world and projected back into it -- where they are ultimately confirmed. Durkheims' theorizing of this reciprocal relationship led him to postulate that conceptions arising out of social regularities would have some application to the sphere of physical phenomena, some "objectivity."

But there are closer approaches to the objectivity of the phenomenal world. As human societies grew and differentiated, the cohering bonds of social categorization loosened. Religious thought about the natural world was the product of smaller, more uniformly cohesive communities, and was conditioned throughout by the conceptual categories drawn from social life. But as complexity and differentiation increased, certain segments of the society were less constrained by these cohering conventions.

Refinements of conceptions and techniques of observation permited a clearer or more direct conceptual accord with the actual realities of the phenomenal world. The knowledge derived from this activity is science, and science sustains belief because it is more objectively -- rather than conventionally -- true.[5] The knowledge produced by science cannot be adequately accounted for by sociological analysis, because it is precisely that knowledge that is independent of social conditioning.

Marx, strong and weak

After Durkheim, Marx develops and furthers a social analysis of science. Marx has been interpreted in two senses on the social conditioning of science. In the strong sense, Marx is taken to mean that the content of scientific knowledge is the product of the cultural resources of class interests -- historically, bourgeois captitalist class interests -- and serves those interests in reconstructing reality to their advantage.[6] In the weak sense, Marx is interpreted to hold that although the focus or mandate of science may be socially

determined, its conceptual apparatus and substantive product are not. The weak sense is the predominant interpretation of Marx in conventional sociology of science.

The emerging capitalist economies of the seventeenth and eighteenth centuries provided a spur to the development of scientific enterprise as well as economic enterprise. The economic requirements of the ascendant bourgeois class were addressed by technological innovations, directed by an instrumental science. This same period, and same stimulus, brought together two traditions of knowledge, previously separated by social boundaries -- craft knowledge and scholarly knowledge. The fusion of empirical and rational knowledge in the service of new class interests produced a powerful syzygy. The new science produced practically effective knowledge that was used by the capitalist class directly to enhance economic production, and as science produced for capital, so capital produced more resources for science.

Under capitalism, Marx says:

...nature becomes purely an object for mankind, purely a matter of utility; ceases to be recognised as a power for itself; and the theoretical discovery of autonomous laws appears merely as a ruse so as to subjugate it under human needs, whether as an object of consumption or as a means of production.[7]

The strong interpretation of Marx pursues this analysis, treating the outcome of scientific production as a thoroughly social construct. Still, Marx is also considered to have given science something of the same special epistemological status as Durkheim -- and indeed, posited his own analysis as scientific, maintaining that it could be empirically demonstrated. Science is appropriated and used to serve interests, but the scientific culture has relations of production of knowledge that reduce bias, invasive ideology, and social conditioning. Within scientific production, knowledge claims can be legitimate, non-ideological, and provide accounts of phenomena free from the contingent character of class interests and social relations.

Outside the boundaries of scientific disciplines, however, knowledge claims advanced by scientists may be ideological, as Rose and Rose present the claim:

> ...[S]cience becomes an ideology and scientists the ideologists. How does this work? As the material world controls the limits of the interpretation of the scientist in his own work, the answer lies, as Marx and Engels saw, *outside* the precise research area, where the scientist, freed from such constraints, talks (typically in the name of science) pure ideology. In the name of science, invoking neutrality, technique and expertise, the scientist supports the ruling strata....[8]

Science and large-scale, differentiated capitalist economies developed in a reciprocally reinforcing relation, according to the accounts of both Marx and Durkheim. The production and modification of knowledge became instituted in a distinct social sub-system, its practitioners governed also by institutionalized rules in norms that maintain the activity independent of individual actors. The scientific subsystem operated autonomously, but provided the means to fulfill a requisite need -- instrumental knowledge was provided for economic, material, and social control (and in the Marxist analysis, for the particular class interests of capitalism). The development and growth of the scientific enterprise are also thereby determined to an extent by the usefulness of its knowledge, in the rewarding of additional resources to further scientific activity. But the content and process of producing certified scientific knowledge are not in any significant way socially conditioned, and necessarily cannot be, because the constitution -- and demonstrated efficacy -- of scientific knowledge abides in its "objectivity," or direct and unconstrained correspondence with the natural order.

And finally, in this enterprise, the social relations among the practitioners -- the role of the scientist -- are maintained by norms uniquely derived from production and certification of "objective" knowledge -- that is, scientific communities have special characteristics that function to reduce social contingencies, bias, intrusive ideologies, etc.

After Marx, a window

Karl Mannheim, following Marx, opened a window of opportunity for a more penetrating observation of science from a sociological perspective. Or perhaps it might be fair to say that a discontinuity in science provided a window, in relativity theory, that revolutionized perspectives of all (nonreligious) thought.

(The cultural impacts of quantum mechanics and Einstein's special and general theory of relativity are beyond the scope of this present investigation -- and are in any case still in progress. Mannheim was certainly aware of the developments in contemporary physics, and makes reference to them in *Ideology and Utopia*.

> We will have to reckon with situational determination as an inherent factor in knowledge, as well as with the theory of relationism and the theory of the changing basis of thought...we must reject the notion that there is a "sphere of truth in itself" as a disruptive and unjustifiable hypothesis. It is instructive to note that the natural sciences seem to be, in many respects, in a closely analogous situation.")

Mannheim, in attempting to locate the "truth" of ideas, while maintaining that ideas were historically relative, and much of social thought ideological, looked to a distinction between the methods and concepts of natural sciences and those of the social sciences and historical thought.

Science consisted of timeless and static concepts, valid knowledge obtained by unconditioned observation and increasingly refined measurement. Phenomena in the material sphere are invariant. Because the empirical relationships in the existential world are unchanging and universal, any knowledge claims about them must share this quality. Only detached, impartial observation and accurate measurement can provide valid knowledge. Scientific knowledge is the cumulative history of such certified observations and measurements.

Investigation of social phenomena involves interpretation of participants' meanings. Participants' meanings cannot be observed, and are not timeless and static, but change in relation to social position, interest, or perspective. Understanding of the

"objectifications of spirit," or cultural knowledge, requires a special mode of knowledge different from that appropriate to the natural world. An act of sympathetic understanding is required that reconstructs the embodied social meanings through an imaginative projection: hermeneutics. Mannheim sought to develop a relativisitic epistemology to establish equivalent validity or "objectivity" criteria for socially derived knowledge claims.

Mannheim critiqued science following Marx, but suggested that the methodology of the sciences was a product of ascendant bourgeois culture, in which qualitative, personal knowledge was denied validity in favour of universally applicable scientific knowledge claims. The origins of scientific epistemology are ideological in this sense, and continue to condition the criteria against which all subsequent knowledge claims are measured.

Mannheim looked for validation of socially determined knowledge in a relational epistemology in which objectivity could be achieved either by application of established procedures or agreed upon critieria of adequacy, or through a sort of mutual translation of divergent perspectives, each into the other's terms, and a reconciliation at a more general level.

Mannheim attempted to oppose the universal application of positivist epistemology to knowledge outside the natural sciences, and present a valid -- alternative and relational -- epistemology for the social sciences and existentially conditioned thought. In attempting to replicate something of the key validation procedures of production of knowledge in the natural sciences for the social sciences, Mannheim uncovered some of the underlying conventions of the natural science, and provided a glimpse of what might be considered their constructivist underpinnings.

The restricted view

But the principal sociological view of science, exemplified by analysts like Werner Stark, continued to explicitly exclude scientific knowledge from any social considerations. As the product of the culmulative application of a uniform perspective that ranged across cultures and societies -- a need to know to increase control over nature -- to a determinate natural world, science needed no explanation from sociology. Because physical scientists can achieve an abiding correspondence between an invariant nature and their own formulations, they are able to establish their distinctive degree of intellectual consensus. Scientific consensus is a product of the objectivity of scientific knowledge. Stark:

> The facts of society are made, and ever re-made, by us, whereas the facts of nature are not. They are *data* in a much more stringent meaning of the term.[10]

The standard view of science regards the natural world as both real and objective. The nature of the real world is not determined by the intentions, preferences, or class interests of would-be observers. The nature of the phenomenal world can be understood: science is the production of accurate knowledge about the characteristic objects, processes, and relationships of the real world. As Mulkay sums it:

> To the extent that scientific knowledge is valid, it reveals and encapsulates in its systematic statements the true character of [the natural world]....Although the natural world is, in a certain sense, undergoing continuous change and movement, there exist underlying and unchangeable uniformities. These basic empiricial regularities can be expressed as universal and permanent laws of nature, which tell us what is always and everywhere the case....Once an observational law has been discovered it applies universally and it commands universal assent. There may be some slight room for cultural variation with respect to *theoretical* speculations, for their content is not wholly determined by observational data. But the greater portion of scientific knowledge, directly rooted as it is in empirical evidence, is necessarily independent of the society or the specialized group which first made it available. The social origin of scientific knowledge is almost completely irrelevant to its content, for the latter is determined by the nature of the physical world itself.[11]

Revolt in the paradigm

Beginning with Thomas S. Kuhn, the substance of science has come under scrutiny in a new way. Kuhn set himself the problem of accounting for science in much the same way that Michel Foucault and Harold Innis set upon their investigations: How to provide a better explanation of the creation, maintenance and change of knowledge? Kuhn could not consider science as the accumulation of "truth," a continuous and unproblematic unfolding. That the truth is not uniform and unvarying is evidenced by the outmoded or discredited beliefs that the scientific enterprise has left in its wake. Characterizing science as simply a method, a set of unique methodological directives for the production of knowledge, was not adequate either. The outcomes of its methodologies are influenced by application and interpretation:

> Observation and experience can and must drastically restrict the range of admissible scientific belief, else there would be no science. But they cannot alone determine a particular body of such belief. An apparently arbitrary element, compounded of personal and historical accident, is always a formative ingredient of the beliefs espoused by a given scientific community at a given time.[12]

If, as Kuhn suggests, science did not contain an element of the arbitrary, the contingent -- susceptible to change, modification, revolutionization -- then science would not progress. Kuhn believed that science required a new history, achieved through an investigation of how scientific knowledge could be at one instance certified and objective, and then change in a radical way, but continue to be received as legitimate and true.

Current or present practice is "normal science": "research firmly based on one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice."[13] Normal science requires a paradigm, a recognized solved-problem set -- that entails and provides laws, theories, applications, instrumentation -- and supplies a "locus of commitment" and an area of unresolved puzzles on which to focus. Paradigms restrict the known, and allow empirical research and theory articulation to be productively directed. Paradigms provide the cohering transformation of individuals engaged in diverse investigations into physical

phenomena into a scientific community or profession. One of the first indicators of the acceptance of a paradigm and the initiation of the process of normal science is the struggle for its institutionalization as a code in communication (through journals, specialist societies) and in education to ensure its transmission.

Most of the work of normal science is esoteric, produced as further articulation or specification of an adopted paradigm. Paradigms are adopted in large part because of their efficacy in solving problems. Normal science, in effect, refines, tests, and focuses the limits of this efficacy. In the process, the paradigm enables an exploration and retrieval of knowledge that would be impossible to obtain in a "chaotic" undertaking. Three areas make up normal science investigation:

> 1)attempts to increase the accuracy and scope of the class of facts a paradigm identifies as significant;
> 2)establishing the predictive capability and extent of theory arising from the paradigm -- improving or finding agreement between fact and theory; and
> 3)articulation of paradigm theories to resolve the ambiguities or crudities of initial formulations that included puzzles -- such articulation brings the paradigm into closer correspondence with the physical world, and involves determining "universal constants" or "quantitative laws."

Normal science does not, therefore, produce major conceptual or phenomenal innovations. Kuhn chracterizes normal science activity as puzzle-solving. Using the exemplars of previously solved problems to guide or provide resources, the expansion of knowledge into normal science is an achievement of ingenuity or skill, like puzzle-solving. The solution is "assured" because normal science operates within a paradigm, just as a puzzle has a finite number of pieces, and is known to form an intelligible whole design:

> The existence of this strong network of commitments -- conceptual, theoretical, instrumental, and methodological -- is a principal source of the metaphor that relates normal science to puzzle-solving.[14]

Paradigms as analytical tools

Kuhn advances paradigms as prior to, and more determinate than, any set of rules for research, because they better locate and describe scientific communities and their activities. Paradigms provide a powerful explanatory tool for the historian or sociologist of science.

Scientists can identify a paradigm "without agreeing upon, or even attempting to produce, a full interpretation or rationalization of it. Lack of a standard interpretation or of an agreed reduction to rules will not prevent a paradigm from guiding research."[15] Kuhn notes that attempts to ascertain the unifying rules of scientific activity have been unsuccessful. Further, examination of scientific education -- where such rules ought to be available explicitly formulated and transmitted -- reveals the process as one of paradigm embedding. Science education proceeds by modelling, not by abstraction of rules from experience.

Normal science gets along without rules so long as the scientific community accepts the achieved problem solutions, and they function adequately. Rules only become a problem when a paradigm is in crisis, or beginning transition -- a theme Kuhn takes up in his account of change by revolution. Paradigms can show scientific communities "overlap without being coextensive," and provide a description of the diversity of scientific activity where change can selectively occur within some fields and not transform science in a wholesale fashion.

Normal science provides an adequate explanation for the steady extension and refinement of knowledge according to expectations produced by paradigm-guided research. New knowledge, discovery of fact and theory, is also accomplished using paradigms as sensitive locators of where that new knowledge might lie: "Discovery commences with the awareness of anomaly, i.e., with the recognition that nature has somehow violated the paradigm-induced expectations that govern normal science."[16]

Anomaly is explored and assimilated through adjustment of paradigm theory. Discovery is a process, requiring observation and conceptualization, fact and assimilation, in a way that revises or amends a paradigm. But even such fine tuning within a paradigm is a process negotiated with some difficulty. Normal science develops with precision, specifying fact and theory to develop stronger and stronger correspondences -- the more conventional and comprehensive a paradigm becomes, the greater the significance of any anomaly that persists becomes to its knowledge claims. There comes a point at which an anomaly, or the proliferation of anomalies, unsettles in a fundamental way the paradigm that sustains a scientific community. Kuhn refers to this stage as crisis. Crisis precipitates innovation by "loosening the rules of normal puzzle-solving in ways that ultimately permit a new paradigm to emerge."[17] (Not to push the analogy too far, it might be said that the scientific community turns up enough different pieces to begin to question whether there might not be an entire new puzzle mixed into the mass of problems.).

Attempts to resolve the crisis within the normal science paradigm will give way to more and more divergent articulations, to questioning and searching for the underlying rules or assumptions of the solved-problem set and ultimately -- in the generation of new solutions, new observations and theories permitted by the instability of the old paradigm. Throughout, the acceptance of new or divergent problems solutions has been achieved in a sustained process of negotiation with the old paradigm and the community created around it, from which it was created. When the new knowledge is certified, a revolution has occurred.

Change must proceed by revolution, because a new paradigm does not introduce additional objects or concepts, it transforms the way in which science is conducted and the characterisitics of the phenomena on which it is articulated. Two opposing paradigms -- or rather, scientific activity before and after a revolutionary change -- cannot be compared:

they are incommensurable because of the displacement of the conceptual network which structures and guides what counts as science.

Contingent beliefs

Within this formulation of the revolutionary nature of change in science, Kuhn shifts his analysis away from the cognitive functions of paradigms to their normative functions in the scientific community. If science is the practice of knowledge production of a special community, paradigms must guide practitioners. Kuhn, in examining science under transformation, investigates the constituent and structuring *beliefs* of science. Although Kuhn is usually regarded as taking a functionalist approach in his analysis, the investigation of how paradigms are accepted, maintained, opposed, and certified based on the opportunity provided by the rupture of normal science by revolution is very ethnomethodological. Revolutions are extended incongruity procedures, in which the tacit and contingent underpinnings of belief are revealed.

Revolutions in science are not fully reducible to a reinterpretation of individual and stable data: they are equivalent to the gestalt shifts of perceptual theory or the changing of epistemes. The new knowledge generated or progress in problem solving is in large part due to the successful exploitation of perceptual possibilities made available by this paradigm shift. Interpretive enterprise can only elaborate the congruous or incongrouous material offered in a paradigm, not change or transform it. When a paradigm shifts, all its resources become indexed to a different universe.

The revolutionary nature of science is disguised or made invisible by the paradigm-directed history or authority in normal science that institutionalizes its knowledge as cumulative, progressing linearly toward truth. The instutionalization or transmission of paradigms is described by Kuhn as the characteristic pattern of science. The transmission of the culture through written authorities in texts is itself paradigmatic of science,

establishing the "knowledge-mediated relationship between the scientist and nature" and becoming "pedagogic vehicles for the perpetuation of normal science."[18]

Verification or evaluation and certification problems of scientific knowledge are thus suppressed by the authoritive nature of the mode of transmission of scientific culture. Serious questioning, or attempts to reveal *how* what is done accounts for the reliability of the knowledge obtained by a method arise only during times of crisis and revolution.[19] In those times, the criteria or normative rules that are usually tacit appear as allegience or belief is transferred from the old paradigm to the new. In this sense, paradigms may be said to introduce ideology into scientific knowledge.

Neither probablistic verification theories nor Popperian falsification tests can adequately justify acceptance or rejection of a paradigm change. Probability verification compares systems, choosing one that matches the evidence best. But the nature of paradigms is such that they are incommensurable: the nature of evidence, what constitutes evidence, is itself changed, and there can be no neutral grounds on which to compare. Normal science precludes Popperian falsification procedures for much the same reason -- normal science identifies anomalies as focii of further investigation and articulation, not as tests of truth.

The nature of the transfer between paradigms is a "conversion experience," accomplished all at once, wholesale. Because of the incommensurable nature of the two paradigms, the only possible mode is a complete quantum leap. Yet certain factors, Kuhn holds, can influence or trigger the conversion, and operate to continue the confirmation of belief in the new paradigm in its early post-revolutionary stages. It is in these factors that Kuhn finds the relations of scientific belief.

First among the factors is efficacy. New paradigms enable the solution of anomalies, the crisis-producing problems. An improved quantitative precision or accuracy

is a second factor. This capability allows more rapid progress to normalization: greater articulation of theory and fact, excluding areas where ambiguity and anomaly might penetrate. Increased predictive power is also critical to acceptance, confirming the instrumental nature of the conception of science by its practitioners. Aesthetic appeals are "sometimes decisive" in compelling belief: an elegance or simplicity of explanatory power is considered a greater indicator of efficacy and correspondence with the physical world.

Finally, decisions to accept or certify a paradigm are influenced "less on past achievement than on future promise":

> The man who embraces a new paradigm at an early stage must often do so in defiance of the evidence provided by problem-solving. He must, that is, have faith that the new paradigm will succeed with the many large problems that confront it, knowing only that the older paradigm has failed with a few. A decision of that kind can only be made on faith.[20]

Disappointing though this might appear at first, Kuhn does not mean by it that paradigms are adopted through some mix of pragmatic improvements triggered, or determined in the last instance by a "mystical aesthetic." He intends more, I suggest, that in the absense of convincing criteria or reason -- indeed, in the impossibility of establishing those criteria -- it is more the "increasing shift in the distribution of professional allegiances" that works the change, and that what creates a collective shift has some origin in the social nature of science.

Thus the organization and activity of science come to turn on an "apparently arbitrary element compounded of personal and historical accident." This element can be the paradigm, posited by Kuhn as a construct of socially contingent, consensual knowledge. Its features include an autonomous belief system, created largely by the authoritarian modes of communication in the form of cultural transmission by model rather than by processes in education, and a systematic suppression or obscuring of the dynamics of change. This movement of authority is not only a characteristic of science, but can be taken as an analytic description of ideology and the project of science popularization.

After Kuhn

Kuhn created something of a revolution himself in the relations of science and society with the publication of *The Structure of Scientific Revolutions*. A new generation of sociologists have turned analysis on the content of science, its cognitive substance and procedures, and the sustaining or changing criteria of belief.[21]

Knorr-Cetina and Mulkay split the current work in the sociology of science into two principal lines of argument, post-Kuhn: arguments following on underdetermination of theory by evidence; and arguments based on the theory-ladenness of observation.[22]

Underdetermination from evidence turns on the following principle: If two theories have exactly the same deductive observational consequences, then any experimental evidence for or against one of them is evidence of the same force for or against the other. As philosopher of science Richard N. Boyd has framed it:

> [G]iven any theory which contains non-observational terms and is consistent, it is always possible to produce alternative theories which share with the original theory exactly the same set of observational consequences, and which advance what are clearly incompatible causal explanations at the theoretical level for those observational predictions. Since these theories all have the same observational consequences [and comparable degrees of "simplicity"], and since experimental evidence for or against a scientific theory arises from the success or failure of one of its observational predictions....choice between one or the other of these theories cannot be a matter of experimental evidence. Two such theories would be equally confirmed or disconfirmed by any possible experimental evidence, and thus -- since they also offer incompatible accounts of the causal relations between theoretical entities -- it is impossible that we should have...experimental evidence for any particular account of the causal relations between unobservable entities.[23]

Current sociology of science moves into the logical vacuum created by such anti-realist arguments in science to suggest that social factors condition the choice and acceptance of scientific theory and play an important role in the production of scientific knowledge.

On the theory-ladenness of observation, argument suggests that theories are intertwined with the facts about any subject matter under observation. N. R. Hanson has given this argument its classic formulation in *Patterns of Discovery*:

> Fundamental physics is primarily a search for intelligibility -- it is philosophy of matter. Only secondarily is it a search for objects and facts (though the two endeavors are as hand in glove)....It is important to realize, however, that sorting out differences about data, evidence, observation, may require more than simply gesturing at observable objects. It may require a comprehensive reappraisal of one's subject matter....There is a sense, then, in which seeing is a "theory-laden" undertaking. Observation of x is shaped by prior knowledge of x....[24]

Observation is not the unembroidered evidence of the senses, enhanced by sophisticated instrumentation. Observation is an active process, in which the observer "creates and responds to a dynamic sequence of clues,"[25] and involves categorization according to preconceived concepts.

The very criteria of what constitutes adequate observation varies according to interpretive and social context. Observations involve auxilliary hypotheses that establish the significance of measurement, classification, and what constitutes adequate observational evidence. As Knorr-Cetina and Mulkay have framed it:

> If scientific observations are ridden with theoretical assumptions, then scientists can in principle always doubt a particular observation by challenging the auxilliary assumptions upon which it is based. As some historians and sociologists of science have documented, what counts as a proper observation is indeed at stake in many scientific arguments....What bearing the evidence is thought to have on a theory depends on its being accepted as valid information, which in turn depends on the unproblematic acceptance of the background assumptions which are constitutive of the observations.[26]

Other of the central assumptions in the taken-for-granted view of science have come under investigation as the new field of sociology of scientific knowledge opened by Kuhn has been expanded and explored.

The principle of the uniformity of nature, the unvarying reality that science discovers unproblematically, is more appropriately understood as an accounting device,

according to the constructivist analysis forwarded by Mulkay.[27] The principle is
unprovable empirically, as such proofs presuppose the principle. Factual statements are
critiqued as dependent on theoretical frameworks. The meaning of facts is in their location
within these structures of concepts and propositions, and as such, are also "theory-laden"
and subject to change as the analytic framework in which they are embedded changes.

Scientific knowledge claims are assessed for their capacity to meet the
requirements of a particular interpretive context, which generally requires consistency
with other knowledge claims and conformity with conventional standards of adequacy
appropriate to a given class of problems. Both requirements vary over time and in
interpretation:

> Although certain broad conceptions have been identified in the philosophical
> literature as common bases for accepting or rejecting claims, these
> conceptions are necessarily interpreted by scientists in terms of particular
> theoretical ideas and specific analytic repertoires....Any significant claim is
> likely to entail some revision of current criteria of adequacy, as well as
> implying that the established corpus of knowledge is inadequate in some
> way. Consequently, the assessment of such claims within a research
> specialty tends to occur relatively slowly, and is often characterized by a
> marked opposition, as members explore the implications of the claim. The
> process of assessment is, therefore, also a process of reinterpretation.[28]

Mulkay suggests therefore, that scientific knowledge is "not stable in meaning, not
independent of social context, and not certified by the application of generally agreed
procedures of interpretation."[29]

> Whether it is the nature of the things one "sees" in scientific observation,
> the proper conduct of an experiment, or the adequacy of a theoretical
> interpretation, scientific agreement appears to be open to contestation and
> modification, a process often referred to as "negotiation". Through
> contestation and modification, the meaning of scientific observations as well
> as of theoretical interpretations tends to get selectively constructed and
> reconstructed in scientific practice.[30]

No further arguments will be advanced in an attempt to warrant the sociological
interpretation of science. The work of current philosphy and sociology of science suggests,
if nothing else, that the positivist representation of science and its product, scientific

knowledge, as unconditioned by social factors is not unquestionable. The strong -- and near exclusive -- preference given the positivist interpretation of science in popularizing accounts coupled with the supression of any contradictions or alternatives to this interpretation can, I suggest, be regarded as ideological, and as part of the struggle to maintain a cultural authority as a requisite to enjoying power over resources.

The next step in this investigation will attempt to operationalize a number of concepts with which an analysis of science popularizing texts might be able to discover such ideological conditioning.

NOTES: Chapter two

1   Galileo Galilei, *A Dialogue Concerning the Two Chief World Systems*, Ed. Giorgio de Santillana (Chicago: University of Chicago Press, 1953), p. 63.

2   Robert K. Merton, "The normative structure of science," *The Sociology of Science: theoretical and empirical investigations*, Edited and with and Introduction by Norman W. Storer (Chicago and London: University of Chicago Press, 1973), p. 271

3   Emile Durkheim, *The Elementary Forms of the Religious Life* (London: Allen and Unwin, 1915), pp. 12-13.

4   Durkheim, p. 19.

5   Durkheim, p. 437.

6   This account is derived from B. Hessen, "The social and economic roots of Newton's Principia," *in* N.I. Bukharin et al. (Eds.), *Science at the Crossroads* (2nd Edition, 1971)(London: Frank Cass, 1931), cited in Michael Mulkay, *Science and the Sociology of Knowledge* (London: George Allen and Unwin, 1979), pp. 7-8.

7   Karl Marx, *Grundrisse* (Harmondsworth: Penguin, 1973), p. 410.

8   Hilary Rose and Steven Rose (Eds.), *The Political Economy of Science* (London: Macmillan, 1976), pp. 8-9.

9   Karl Mannheim, *Ideology and Utopia* (New York: Harcourt Brace and World, 1936), p. 305.

10   Werner Stark, *The Sociology of Knowledge* (London: Routledge and Kegan Paul, 1958), p. 165.

11   Michael Mulkay, *Science and the Sociology of Knowledge* (London: George Allen and Unwin, 1979)

12   Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago; London: University of Chicago Press, 1962), p. 4.

13   Kuhn, p. 10.

14   Kuhn, p. 42.

15   Kuhn, p. 44.

16   Kuhn, p. 52,53.

17   Kuhn, p. 80

18   Kuhn, p. 136.

[19] The parallels with Harold Innis' theories of monopolies of knowledge first appropriating knowledge, then solidifying, then coming under attack from innovations arising at the margins, is striking.

[20] Kuhn, p. 157.

[21] Notably, Michael Mulkay, *Science and the Sociology of Knowledge* (London: George Allen and Unwin, 1979); Barry Barnes, *Scientific Knowledge and Sociological Theory* (London and Boston: Routledge and Kegan Paul, 1974); Karin Knorr-Cetina, *The Manufacture of Knowledge: an essay on the constructivist and contextual nature of science* (Oxford; New York: Pergamon Press, 1981); Karin Knorr-Cetina and Michael Mulkay (Eds.), *Science Observed: perspectives on the social study of science* (London; Beverly Hills; New Delhi: Sage, 1983).

[22] Karin Knorr-Cetina and Michael Mulkay, "Introduction: Emerging principles in social studies of science," in Karin Knorr-Cetina and Michael Mulkay (Eds.), *Science Observed: perspectives on the social study of science* (London; Beverly Hills; New Delhi: Sage, 1983), pp. 1-17.

[23] Richard N. Boyd, "Realism, Undertermination, and a Causal Theory of Evidence," *Nous*, 7 (1973), pp. 1-12

[24] N.R. Hanson, "Observation," Chapter 1, *Patterns of Discovery* (London: Cambridge University Press, 1970), pp. 4-30.

[25] Michael Mulkay, *Science and the Sociology of Knowledge* (London: George Allen and Unwin, 1979), p. 49.

[26] Knorr-Cetina and Mulkay, p. 4.

[27] Mulkay, pp. 27-29.

[28] Mulkay, p. 60.

[29] Mulkay, p. 60.

[30] Knorr-Cetina and Mulkay, p. 13

CHAPTER THREE:

DISCURSIVE STRATEGIES

Some circumstantial evidence is very strong, as when you find a trout in the milk.

-Henry David Thoreau,
Journal, November 11, 1854.

Science popularization in the media accomplishes its role in the discourse establishing, maintaining and restoring authority in science through two modes of communication: persuasive and controlling.

The persuasive mode of science popularization can be analysed as rhetorical in the classic sense as defined by Aristotle:

> The duty of rhetoric is to deal with such matters as we deliberate upon without arts or systems to guide us, in the hearing of persons who cannot take in at a glance a complicated argument, or follow a long chain of reasoning. The subjects of our deliberation are such as seem to present us with alternative possibilities: about things that could not have been, and cannot now or in the future be other than they are, nobody who takes them to be of this nature wastes his time in deliberation.[1]

Rhetoric is in this sense a practical activity, in that it helps complete human judgments about questions of practical choice and conduct, guiding deliberate action. Rhetoric functions to formulate the best possible arguments on either side of questions of practical import that are contested, that cannot be solved because there is no certain knowledge serving as a resource. The question of trust and authority in science, and the whole of the public understanding of science, I would suggest, fall into the domain of rhetoric in this sense. But the present rhetorical practices of science popularization, as I will attempt to demonstrate, are not virtuous.

The rhetorical practices of science popularization are akin to advertising. Science and scientists are invested with values and meanings through rhetoric to produce an extended and more powerful dimension of persuasive discourse. The rhetorical function of science popularization is to create and manipulate relations of authority and trust.

The controlling mode of communication works more directly in language to transform and shape meaning, attempting to enforce a preferred interpretation. This concept is drawn from the work of linguist M.A.K. Halliday,[2] and the analysis that grounded itself in his linguistic theories known as "critical linguistics." Halliday posited that language developed in response to specific communication needs. These needs are social, and typically involve the control of subordinate groups by dominant groups. Therefore, three related propositons inform the methodology of critical linguistics: language is ideological; discourse expresses social relations; and discourse constitutes social relations. Characteristically, those relations are ones of power and control:

> Power differential provides the underlying semantic for the systems of ideas encoded in language structure....Language variations reflect, and what is more, actively express the structured social differences which give rise to them.[3]

These two approaches will be operationalized and used to characterize a selection of popular science texts.

Part one: The knowledgeable guide

"Rhetoric is the counterpart of Dialectic."
-Aristotle, *Rhetoric*

The duty of rhetoric is "to deal with such matters as we deliberate upon without arts or systems to guide us" for an audience that has no special knowledge resource on which to draw. This proper use of rhetoric in appropriate context corresponds closely with one of the commonly accepted purposes of popular science communication.

Aristotle defined rhetorical study as the "faculty of observing in any given case the available means of persuasion."[4] He identified three divisions of rhetoric, and characterized the means of persuasion appropriate to each. Political or deliberative discourse is used by legislative assemblies to decide a future course of action. Forensic or legal discourse is used in law courts in arguments over the nature and causes of past events. Epideictic discourse, or the ceremonial oratory of display, concerns present judgments of persons or events. Epideictic discourse praises or censures in ceremonial declamations that confirm and solidify the values of its audience. The present account will not provide details of the arguments and styles of the three divisions of discourse, but instead will focus only on those elements appropriate to popular science communications.

Fahnestock has used rhetorical methodology to examine the "accommodation" of scientific fact to different audiences as it passes from scientific articles and reports to popular accounts.[5] Although she makes no rigorous interpretation of the persuasive practices of popular science communication, her schematic description provides a useful starting point.

Original scientific reports are forensic discourse, according to Fahnestock, principally concerned with establishing the validity of observations and arguing the

significance of past fact. Popular science communication, what Fahnestock calls
"accommodations," shift rhetorical genre and are "overwhelmingly epideictic; their main
purpose is to celebrate rather than validate."[6]

As Aristotle noted, rhetoric is esstentially a public, practical activity, and as such
cannot exploit special repertoires of knowledge:

> Moreover, before some audiences not even the possession of the exactest
> knowledge will make it easy for what we say to produce conviction. For
> argument based on knowledge implies instruction, and there are people
> whom one cannot instruct. Here then, we must use, as our modes of
> persuasion and argument, notions possessed by everybody....[7]

Fahnestock suggests that science accommodations have only two basic appeals to
make to everyday notions: appeals to "wonder" or "application". Appeals to wonder
correspond to deontological argument in ethics, in which something is praised or excoriated
by attaching it to a category that has a recognized value for an audience. Appeals to
application correspond to teleological arguments that claim something has value because it
leads to further benefits.

In her comparison of a number of original scientific reports with the
accommodating popular texts, Fahnestock suggests that science accommodations rely
heavily on the wonder appeal. Accommodations "emphasize the uniqueness, rarity," and
"originality of observations."[8] This is a classical feature of epideictic appeal, that "a thing
is greater when it is harder or rarer than other things." A rhetorical value is thus attached
to scientific knowledge through representing it as unique or original -- as discovery.
Although this is certainly a feature of the coverage of science in the media, I would suggest
that it is more the product of the rhetorical requirements of the media genre -- an appeal
to the topical, novel, or original as values that need to be attached to "news".

More interesting is her assertion that accommodations attach positive values by
associating more certainty to scientific data or observations than in the original accounts:

> In the space limitations of a short notice in a magazine of popularized
> science, there is no room for the qualifications a more knowledgeable
> audience would demand, qualifications that show the author's awareness of
> the criticism and refutation that an expert audience could raise against his
> inferences....When qualifications are omitted, the result is greater certainty
> for the remaining claims. These omissions once again serve the
> accommodator's epideictic purpose, for only certainty can be the subject of
> panegyric. To address the public on these subjects requires claiming their
> significance, and there is simply no way to address the public with the
> significance of findings that are so carefully hedged their reality seems
> questionable.[9]

This particular rhetorical manoeuver will be analysed in detail in a following

section, using the methodology of critical linguistics to support an interpretation that such

a transformation does more than simply assert certainty in order to praise.

In the present analysis, I will make use of rhetorical categories and styles to

examine some very specific features of popular texts to discover whether the persuasion

attaches a different set of values, and to suggest the significance of this mode of

communication in science popularization.

The rhetorical character of scientists

Robert K. Merton's landmark work on the ethos of science -- "that affectively toned

complex of values and norms which is held to be binding on the man of science"[10] -- was

written in 1942. In the context of the global war then consuming humanity, the struggle

over the ideology of science seemed of critical strategic importance.

The development of science, as Merton's earlier interrogation of history had shown,

had been spurred to its modern state from the conjunction of the "same community of

assumptions" in Protestanism, capitalism and science in the seventeenth century. It

appeared to Merton (and many of his contemporaries in the scientific, political, and

economic communities) that science was again on the hinge of history. A similarly

successful conjoining of fascism and science in a mandate of global domination was a

present and terrifying prospect. Science, too, was under attack. And in this breaching of

the boundaries of science and social order, scientists had come to "recognize their

dependence on particular types of social structure."[11]

> Three centuries ago, when the institution of science could claim little independent warrant for social-support, natural philosophers were likewise led to justify science as a means to the culturally validated ends of economic utility and the glorification of God. The pursuit of science was then no self-evident value. With the unending flow of achievement, however, the instrumental was transformed into the terminal, the means into the end. Thus fortified, the scientist came to regard himself as independent of society and to consider science as a self-validating enterprise which was in society but not of it. A frontal assult on the autonomy of science was required to convert this sanguine isolationism into realistic participation in the revolutionary conflict of cultures.[12]

But that dependence was mutual: "science is afforded opportunity for development

in a democratic order which is integrated with the ethos of science."[13] Thus the ethos of

science is an ideologically framed formulation of science, put forward at the time of crisis

for both democracy and science. As such, it provides an excellent set of operational

categories with which to conduct a rhetorical analysis of popular science communication

texts.

> The institutionalized goal of science is the extension of certified knowledge. The technical methods employed toward this end provide the relevant definition of knowledge: empirically confirmed and logically consistent statements of regularities (which are, in effect, predictions). The institutional imperatives (mores) derive from the goal and the methods. The entire structure of technical and moral norms implements the final objective. The technical norm of empirical evidence, adequate and reliable, is a prerequisite for sustained true prediction; the technical norm of logical consistency, a prerequisite for systematic and valid prediction. The mores of science possess a methodologic rationale but they are binding, not only because they are procedurally efficient, but because they are believed right and good. They are moral as well as technical prescriptions.[14]

The ethos of science thus defined has four constituent fields: universalism,

communism, disinterestedness, and organized skepticism.

Universalism: "The circumstance that scientifically verified formulations refer in

that specific sense to objective sequences and correlations militates against all efforts to

impose particularistic criteria of validity." This characteristic institutional feature of

science certifies only those truth-claims that have been "subjected to preestablished impersonal criteria," and are conformable with existing certified knowledge. "Objectivity precludes particularism," and knowledge claims are accepted or rejected independently of any personal or social attributes of their claimant -- including race, class, religion, etc.[46]

Democracy, too, includes an ethos of universalism as a dominant guiding principle -- "[h]owever inadequately it may be put into practice...."-- because impersonal criteria of accomplishment and not fixed class or economic status are characteristic of open democratic societies. Full democratization is achieved through the progressive elimination of such obstacles to the fullest possible development and exercise of socially valued capacities, just as certified scientific knowledge is achieved through the progressive elimination of any subjective, particularized elements.

Communism: Merton means, of course, "the nontechnical and extended sense of common ownership," in which knowledge, the product of scientific labour, is considered the product of social collaboration and remains a "common heritage in which the equity of the individual producer is severely limited." The reward system of science (which Merton went on to examine in some detail) exchanges recognition and esteem in degree to the significance of the knowledge contributed for the product of individual scientific labour.

Three characteristic "traits" of scientific communism are of particular interest. The first is a concern with scientific priority or discovery, that comes from the institutional emphasis on recognition and peer esteem as the "sole property right." This emphasis produces a form of competition --"competitive cooperation" -- in which the products are communalized, but the rewards accrue to individual producers. There is also an "imperative for communication of findings. Secrecy is the antithesis of this norm; full and open communication its enactment." The communal nature of the scientific enterprise provides a pressure for dissemination, and the ethos reinforces it with the incentive of recognition that can only come from publication for approval by the community at large.

The last trait is a kind of humility, "a sense of indebtedness to the common heritage and a recognition of the essentially cooperative and selectively cumulative quality of scientific achievement."[16]

Disinterestedness: Disinterestedness is supported by a complex of institutional controls on "a wide range of motives which characterize the behaviour of scientists." The prime control is a peer accountability deriving from the public and testable requirement of scientific knowledge.

Organized skepticism: Both a methodological and an institutional mandate, organized skepticism requires the suspension of judgment in order that beliefs, phenomena be scrutinized in terms of logical and empirical criteria. "The scientific investigator does not preserve the cleavage between the sacred and the profane, between that which requires uncritical respect and that which can be objectively analysed.."[17]

The ideal citizen

Yaron Ezrahi cogently observes that in the formulation reflected by Merton, science has been appropriated as a symbol of the ideal culture of politics in liberal-democratic society, exerting a cultural force as a "simpler exemplification of the principles of discourse in which open criticism and diverse positions are resolved in authoritative consensus...."[18] As John Dewey had put it in a contemporary study, *Freedom and Culture*, in 1939:

> [T]he operation of cooperative intelligence as displayed in science is a working model of the union of freedom and authority [relevant to the political, economic and moral spheres]....The very future of democracy is allied with the spread of the scientific attitude...it is the only assurance of the possibility of a public opinion intelligent enough to meet present social problems.[19]

In the empirical tradition in science, begun with the Enlightenment and continuing in dominance, truth rests on the accumulated "testimonies" of observation, rather than on

the inferences of classical science in mathematics. The experimental sciences appear as an example of a cultural enterprise in which the "construction of communally binding, intersubjective standards rests upon the coordination of individual contributions." And the communal, and disinterested norms of science identified by Merton offers, in the scientist, a model of the liberal-democratic citizen: "a public-regarding individual with an explicit moral commitment to public values:"[20]

> The evolution of scientific constructions of reality from a multiplicity of mutually corroborative and corrective individual testimonies and the certification of empirical truths through impersonal technical discourse, teamwork and comparisons, have made the empirical tradition in science a cultural model particularly relevant to the task of balancing the individual and the public realms in liberal-democratic politics.[21]

Thus, Ezrahi says, science suggests a cultural strategy for depersonalizing authority through the "free operation of a voluntary, self-regulating community which evolves universally valid standards." Originally providing a criticism of publically unaccountable authority in Church and State in the seventeenth century, science has continued to exert a normative cultural force in providing the model of authority for liberal-democratic politics. Political order rests on a "balanced symbiosis between the public and the self, and the moralization of restraint by anchoring discipline in freedom and culture."

So science entered the ideological fray with a new set of values attached by a new rhetoric. The norms of science, the moral armature of "prescriptions, proscriptions, preferences, and permissions,"[22] are the literal embodiment of science in the conduct of scientists. Legitimated in terms of institutional values, these imperatives "transmitted by precept and example and reenforced by sanctions are in varying degrees internalized by the scientist, thus fashioning his scientific conscience...."[23] And in the process, the ethos defines and legitimates an authority for liberal-democratic politics in a persuasive demonstration of virtue worthy of the term rhetoric.

In the analysis of texts of popular science communication in the following chapter, the representation of scientists will be examined for the rhetorical attachment of the values of the ethos of science. If it can be demonstrated that popular science communication makes use of this form of epideictic argument, grounds will also have been made for an interpretation of popular science communication as an ideological appeal to the values of authority.

Part two: Controlling meaning

In addition to a persuasive mode of communication that can be approached using the techniques of rhetorical analysis, popular science communication has another characteristic mode, that of controlling meaning more directly in language in order to assert a preferred interpretation. Where rhetoric may properly attempt to appeal to good reasons in argument, a language of control attempts to transform meaning, obscuring alternatives and closing off the possiblities of analysis, thus subordinating the position of the interpreting subject.

The presense of a controlling mode in popular science communication may be, in part, necessitated by the character of rhetoric. Rhetorical appeals depend on shared cultural values. The cultural values of science have become, or have been perceived to have become, increasingly alienated from the nonscientific culture. The increasing disintegration of values and alienation of reason ascribed to contemporary social life has resulted, among many other things, in the weakening of the rhetorical appeal. At any rate, the variable political and economic fortunes of organized science since the Second World War have caused the scientific community and the interests closely allied to them to search for a stronger communication programme. As a discursive strategy, control is more effective, and more predictable than persuasion.

This position is similar to the "encoding/decoding" model of communication developed by David Morley and others,[24] which suggests that the production of meaningful messages is always "problematic 'work'. The same event can be encoded in more than one way." Messages may propose and prefer certain readings, but meaning can never be completely controlled.[25] Meaning is generated in the "complex articulations of class, ideology and power, where social structures are conceived as also the social foundations of language, consciousness and meaning." An analysis of meaning must incorporate an

account of the contextual or situational social conditions that generate different cultural and ideological competencies.[26] Messages can be encoded one way and read another, according to the cultural/ideological competency and specific context·

> [T]he communicative form and structure of the encoded message can be analysed in terms of its preferred reading: the mechanisms which prefer one, dominant reading over the other readings; the means which the encoder uses to try to win the assent of the audience to his particular reading of the message. Special attention can be given...to the control exercised over meaning, and to 'points of identification' within the message which transmit the preferred reading to the audience.[27]

Morley's work has tended to concentrate on creating ethnographies of audiences, drawing the "cultural map" showing the "various cultural repertoires and symbolic resources available to differently placed subgroups" within and audience. Interpretation of meaning follows from rather exhaustive reconstructions of messages through fieldwork involving intensive and directed audience interviews.

This element of the encoding/decoding methodology, although appropriate within the theoretical scope of this thesis, is not undertaken here. The focus of the analysis in this thesis will remain with the construction of meaning in popular science communication as it is encoded in a controlling, preferred mode by producers. The vast amount of audience research done over the last 30 years by the initiating communicators -- by the National Association of Science Writers, in the *Science Indicators* project, and on behalf of the American Association for the Advancement of Science by Jon D. Miller and others -- suffices to document the dominance of the decoded preferred meaning made by the audience, as we have been arguing it.

Rather, analysis of the ideological positioning encoded in popular science communication will be forwarded by examining the patterns of relations between formal linguistic features of its texts, showing their conditioning by ideological frameworks. Based on the preceding arguments drawn from the sociology, history, and philosophy of science, the methodology of critical linguistics will be used to analyse popular science texts for

accounts of the production of scientific knowledge. If these accounts can be shown to attempt to control for a positivistic idealization of scientific knowledge, representing it as unproblematic, incontrovertable, objective -- constituted, in short, in a way that denies or conceals any social origin or contingency -- then it will be taken to be an attempt to control meaning for ideological reasons. A partial explanation of those reasons has been presented in the relationship between the funding cycles science and the salience given public awareness campaigns in effecting them. This has been supplemented with an account of ideological usefulness of a particular view of science as a model of liberal-democratic political systems, and the instrumental relation between science literacy and governance in such systems.

Language and ideology

[L]anguage, typically, is immersed in the ongoing life of a society, as the practical consciousness of that society. This consciousness is inevitably a partial and false consciousness. We can call it ideology, defining 'ideology' as a systematic body of ideas, organized from a particular point of view. Ideology is thus a subsuming category which includes sciences and metaphysics, as well as political ideologies of various kinds, without implying anything about their status and reliability as guides to reality. Language is an instrument of control as well as of communication. Linguistic forms allow significance to be conveyed and to be distorted. In this way hearers can be both manipulated and informed, preferably manipulated while they suppose they are being informed.[28]

Critical linguistic analysis was developed from the language theories of M.A.K. Halliday, who suggested that language was a social semiotic. Tracing a theoretic line through Malinowski, Firth, Jakobson and Levi-Strauss, Halliday construed the social system as a system of meaning relations. These meaning relations were realized in a multitude of ways, but most powerfully in terms of maintenance and transmission of the system through encoding in language:

The meaning potential of a language, its semantic system, is therefore seen as realizing a higher system of relations, that of the social semiotic, in just the same way as it is itself realized in the lexico-grammatical and phonological systems.

>A child who is learning his mother tongue is learning how to mean. As he builds up his own meaning potential in language, he is constructing for himself a social semiotic. Since language develops as the expression of the social semiotic it serves at the same time as the means of transmitting it , and also of constantly modifying and reshaping it, as the child takes over the culture, the received system of meanings in which he is learning to share.[29]

Halliday's concepts provided the basis for a methodology of identifying and analysing ideological discourse for an interdisciplinary group of linguists, historians, literary critics, and social scientists in an approach they termed "critical linguistics".

Following Halliday, the critical linguists took the position that language served specific social functions, and that social relationships -- the interpretation and evaluative positioning of subjects and objects, source and addressee -- in turn influenced language structure and use. But social structures determine linguistic production, and the social interaction that is encoded in language, that *is* language, is governed by rules or relations that are not consciously, immediately available to speakers as they interact.

All discourse is socially conditioned, not only the face-to-face interactions, the dialogues or speech acts of individuals. Language ability is also a product of social structure, the codification of certain styles of speech. The choice and use of styles is the realization of the social meanings speakers encode, meanings which are drawn from specific repertoires. The inequity of power relations and the stuggle to maintain them with a limited oppositional struggle are the principal social relations conditioning language (especially the public, social discourses of media, advertising, education). In this, language is both a constituting and enforcing or reproducing instrument of relations of power.

Meaning is bound up with ideology in language, and both are determined by social relations. Linguistic analysis oriented towards the social basis of language then, ought to be a sensitive and effective observational tool for examining the ideological conditioning of language to "mediate relationships of power and control."[30]

The focus of critical linguistics is not so much on language itself, or individual speech performance, but on the "social processes that make language work in communication as it does."[31] Because discourse is produced and organized in a coherent manner, and one that represents the social contingencies and purposes of the producers of that discourse, it represents a *social* phenomenon that can be analysed. The linguistic systems used by critical linguists and some of their social correlates are given below:

1)the grammar of transactivity: events, states, processes, and their correlates of power in the attribution or attenuation of agency and cause;
2)the grammar of modality: the interpersonal relations of speaker and hearer, and the correlates of relations of solidarity or command, authority, and other specific social positionings;
3)transformations: the manipulation of basic models -- substituting, deleting, combining or reordering -- and their meaning implications such as the rendering of agents or processes as objects through nominalization or passivization transformations, thereby mystifying processes or strategically disguising agency;
4)the grammar of classification: the linguistic ordering or categorization of items, and their correlates in the evaluation placed on them or their ideological positioning; and
5)coherence, order and unity of the text or discourse, correlating with a kind of political economy of the social ideology.

In positing that "the grammar of a language is its theory of reality," the critical linguists regard language as a system of related categories and processes. The most fundamental categories are models of experiential relations of object and events, the grammar of reality. There are three basic models.

In the first model, there are at least two entities, related by a process. One entity is the cause of the process, the other is effected by it. The action is understood as passing from the agent to the effected. This is the transactive model.

In the second model, one entity is related to a process. With only one entity, it is not clear whether it is an actor or an effected, and the model does not clearly distinguish causal status. This model is the nontransactive.

(Critical linguists are careful to distinguish between transactive and nontransactive models, and transitive and intransitive verb forms. Transactive and nontransactive models describe the "real" or observed nature of event and participant, action passing from an actor to effected, or action restricted to one entity. Transitive and intransitive verb forms note whether the verb may take a direct object or not, but make no existential statements, as it were.)

A third model involves not relations of action and effect, but simple attribution of qualities or equation of entities or qualities. This model, with its two subcategories of equative and attributive is the relational.

> The world is grasped through language. But in its use by a speaker language is more than that. It is a version of the world, offered to, imposed on, exacted by, someone else. The syntagmatic models offer the first classification of an event, in one or another of the meanings of the models. But no model can be offered directly from speaker to hearer. Every syntagm (the linear and structural form in which the models appear in actual language use) is classified many times over, as a whole and part by part.[32]

Among the classifications performed on these sytagms are those of tense -- indicating whether the utterance is true for the past, present or future -- or modality -- indications of degrees of certainty, probability, or authority which a speaker attaches to an utterance.

The contexts of situation in which language is used, and the ways in which situations vary, condition the selection and use of linguistic elements. "Register" is the set of general circumstances and principles that determine the range within which meanings are selected and the forms which are used for the expression of meaning. Register describes the communication instance in three aspects: who is speaking; who is taking part; and what function the language performs.

> *All* language functions in contexts of situation, and is relatable to those
> contexts. The question is not what peculiarities of vocabulary, or grammar,
> or pronunciation, can be directly accounted for by reference to the situation.
> It is *which* kinds of situational factor determine *which* kinds of selection in
> the linguistic system. The notion of register is thus a form of prediction:
> given that we know the situation, the social context of language use, we can
> predict a great deal about the language that will occur, with reasonable
> probability of being right.[33]

The social situation of language is structured as a field of significant social action, a tenor of role relationships, and a mode of symbolic organization. The selection of options in experiential systems, transitivity, classes of things, quality, quantity, time, etc., is determined by the nature of the activity, or field. The selection of interpersonal options, such as in systems of mood, modality, person, key, evaluation and comment, are determined by the role relationships, or tenor of situation. The selection of textual options, such as theme, information and voice, the cohesive patterning of reference, conjunction, etc., tend to be conditioned by the symbolic forms taken by the interaction, especially the role of the text in the meaning environment.[34]

The field (institutional setting, type of social action) of popular science communication is translation and articulation of scientific and nonspecialist knowledge. The tenor (relationship between participants) is mediated experience, exposition, instruction, information acquisition, journalism, written texts. The mode is control, legitimation. It is the mode of popular science communication that will be the focus of this analysis.

Ideologies characteristically attempt to resolve awkward facts or anomalies in the Kuhnian sense, by suppression, denial, or subsumption through reinterpretation. The process of reinterpretation, explanation, and assignment of cause in resolving these awkward facts -- which might be oppositional discourse, challenges to truth claims or legitimacy -- conditions language use in the creation of discourse. In examining a particular utterance, the critical linguists hold that an analyst can correlate linguistic

transformations with ideological transformations. Ideology cannot be simply read off from a text. But its effects, the ongoing process of its conditioning of utterance can be characterized:

> [I]f in general the original material is not recoverable by a linguistic analysis of the final product, then comparison of initial and final material is significant. It is because there are linguistic options in handling the original, that it is not recoverable. Which option is used can be explained in part as the effect of ideological determinations.[35]

A typical ideological transformation moves representation of an event having cause or agency towards a reinterpretation that locates it in a context presenting more general or less immediate causation. Attribution of agency or causality is weakened or deleted by selection or abstraction of effects, coupled with an explanation of those transformed effects by causes of higher generality or abstraction. When language is operated ideologically, meaning is dislocated from its specific context and concrete expression, and then manipulated to establish control and resolve anomaly.

George Orwell's *1984* is the archetypal presentation of language and control. In the novel, Newspeak engenders doublethink and a set of power relations in which the rules and regulations of society have become completely ideological: "totally deleted, so that they are disembodied, invisible, and hence, unassailable."[36] Power becomes purely ideological, and the mechanism of control is mystified, irrational, and strengthened.

> Anyone who can give orders without even acknowledging this in the surface of his utterance has access to an insidiously powerful form of command. For instance, someone who can say 'the door is open' and can be interpreted as saying 'close the door' has issued an imperative which has been totally deleted yet is fully effective. The person who obeys accepts the reality of a power that has not been claimed, which has been completely mystified into the form of an apparently neutral, factual observation.[37]

It is the sequencing, movement, and direction of change that makes an ideological patterning apparent to analysis. The operationalized linguistic elements of transformation that will be used to examine popular science communication are the "patterns of

categorization of participants, and the relations of transitivity (that is, the representation of causality in the process words and clauses)."[38]

The ideological conditioning at work will be located through changes involving passivization, agent deletion, rewording, nominalization and embedding. Texts will be analysed for transformations that move the representation of scientific knowledge or methodology away from any social contingency, or recast its certainty and authority in an idealized manner.

Fahnestock is helpful in providing another set of coordinates for this approach. In comparing original scientific reports and their subsequent transformations into popular accommodations, she identified appeals to certainty as an important element of the attachment of praiseworthy values to science. It is not possible to trace the popular science texts that will be used in this analysis back to scientific originals -- some have no originals, being independently generated accounts, some have too many sources in the scientific literature or to personal interviews with scientists. However, this description of scientific accommodation, together with the characterization of expository science provided by Cloitre and Shinn (which follows), suitably defines the register of popular science communication for the purposes of analysis from a critical linguistic approach. That is, the social context is epideictic, one of attaching praise or blame to science, to rendering judgment on the legitimacy of its authority and trustworthiness. The use of certain linguistic features to attempt to control the outcome of this judgment process -- or rather, to subvert the judging in favour of imposing a preferred meaning -- is an ideologically chosen strategy.

NOTES: Chapter three

1   Aristotle, *Rhetoric* (Book I, Chapter 6), translated by W. Rhys Roberts (New York: Random House, Modern Library, 1954), p. 27.

2   M.A.K. Halliday, *Language as Social Semiotic: the social interpretation of language and meaning.* London: Edward Arnold, 1978.

3   Roger Fowler, Bob Hodge, Gunther Kress, and Tony Trew, *Language and Control* (London: Boston and Henley: Routledge and Kegan Paul, 1979), p. 1.

4   Aristotle, *Rhetoric* (Book I, Chapter 1), p. 24.

5   Jeanne Fahnestock, "Accommodating science: the rhetorical life of science facts," *Written Communication*, 3(3), July 1986, pp. 275-296.

6   Fahnestock, pp. 278,279.

7   Aristotle, *Rhetoric* (Book I, Chapter 1), p. 22.

8   Fahnestock, p. 275.

9   Fahnestock, p. 283.

10   Robert K. Merton, "The Normative Structure of Science," *The Sociology of Science: theoretical and empirical investigations* (Chicago; London: University of Chicago Press, 1973), p. 269.

11   Merton, p. 267.

12   Merton, p. 268.

13   Merton, p. 269.

14   Merton, p. 270.

15   Merton, pp. 270-271.

16   Merton, pp. 274-275.

17   Merton, pp. 277-278.

18   Yaron Ezrahi, "Science and the problem of authority in democracy," *Science and Social Structure: a Festschrift for Robert K. Merton*, Thomas F. Gieryn (Ed.), Transactions of the New York Academy of Sciences: Series II, Volume 39 (New York: New York Academy of Sciences, 1980), p. 45 [pp. 43-60]

19   John Dewey, *Freedom and Culture* (New York: Putnam, 1939), pp. 148-149, cited in Ezrahi, p. 45.

20   Ezrahi, p. 50.

21   Ezrahi, p. 49.

22   Merton, p. 269.

23   Merton, p. 269.

24   David Morley, "Cultural transformations: the politics of resistance," *Language, Image, Media*, Howard Davis and Paul Watson (Eds.) Oxford: Basil Blackwell, 1983), pp. 104-117; David Morley, *The Nationwide Audience: structure and decoding*, British Film Institute Television Monograph 11 (London, BFI, 1980); and David Morley, "Reconceptualizing the media audience: towards an ethnography of audiences," Stencilled Occasional Papers, Centre for Contemporary Cultural Studies, University of Birmingham (Birmingham: CCCS, 1974) 14 pages (mimeo).

25   Morley (1983), p. 107.

26   Morley (1980), p. 20.

27   Morley (1983), p. 108.

28   Gunther Kress and Robert Hodge, *Language as Ideology* (London, Boston and Henley: Routledge and Kegan Paul, 1979), p. 6

29   M.A.K. Halliday, *Learning how to mean: explorations in the development of language* (New York: Elsevier North-Holland, 1975), p. 60.

30   Fowler et al, p. 186.

31   Fowler et al, p. 196

32   Kress and Hodge, p. 9.

33   M.A.K. Halliday, *Language as Social Semiotic: the social interpretation of language and meaning* (London: Edward Arnold, 1978), p.32.

34   Halliday (1978), pp. 142-144.

35   Fowler et al, p. 113

36   Bob Hodge and Roger Fowler, "Orwellian linguistics," *Language and Control*, Roger Fowler, Bob Hodge, Gunther Kress and Tony Trew (London, Boston and Henley: Routledge and Kegan Paul, 1979), p. 17.

37   Hodge and Fowler, p. 18.

38   Fowler et al, p. 116.

CHAPTER FOUR:

TEXTUAL ANALYSIS

Part one: sample and target

Sample description

The material selected for analysis consists of 58 texts (see Appendix A), the winners of the AAAS-Westinghouse Science Journalism Awards for newspapers of over 100,000 circulation and for general circulation magazines, in the period from 1980 to 1986.

The American Association for the Advancement of Science (AAAS) is the world's largest federation of scientific organizations, and also an association of individual scientists and of other persons interested in supporting the aims and activities of the AAAS. As such, the AAAS is also one of the leading institutions for the promotion of the interests of the scientific community in North America (together with the National Academy of Sciences and the National Science Foundation), and certainly the institutional focal point for science popularization.

The role of the AAAS, already noted in chapter one, includes as its objects efforts to:

> [F]urther the work of scientists, to facilitate cooperation among them, to improve the effectiveness of science in the promotion of human welfare, and to increase the public understanding and appreciation of the importance and promise of the methods of science in human progress. [1]

Part of the project to increase public understanding and appreciation of the methods of science has been an awards program for science journalism, beginning in 1946

and continuing in various forms to the present: "to encourage and recognize outstanding reporting on the natural sciences and their engineering and technological applications, excluding the field of medicine."[2] These articles, selected first by a screening committee made up of scientists and academics, then by a panel of judges composed of scientists, academics and senior journalists,[3] represent an exemplary set of popular science communication texts.

Most of the texts (62%, N = 36) were deliberative, that is, accommodations of scientific information strongly featured for decisionmaking. Examples of deliberative texts include those on the responsibility for acid rain problems and potential solutions, or assessments of the space shuttle program: "Space shuttle -- costs, goals, technology -- is it worth it?". A smaller proportion (38%, N = 20) were expository, that is, more strongly featured for description or explanation. Examples include articles on theoretical physics, or specific research programmes: "Space telescope holds NASA's hopes for grand discoveries in universe." (A fuller account of these categories follows in Part Three, typology of popular science texts.)

The sample contained more newspaper texts (N = 50) than magazine texts (N = 8). This is due to the brevity of most news features: in order that the awards recognize substantive accomplishment, they consider series of related articles, or a number of articles written over a year-long period to be representative of a body of work by a particular journalist. Not suprisingly, deliberative texts outweighed expository ones in the newspaper set (70%, N = 35). This reflects the topical nature of the media genre.

The proportions of deliberative and expository were reversed in the magazine set, where expository texts outweighed deliberative ones (75%, N = 6). This proportion likewise reflects the conventions of the magazine genre for more textual elaboration. A crude tabular presentation of this data is given below.

	Newspapers (%)	Magazines (%)
Deliberative	35 (70)	2 (25)
Expository	15 (30)	6 (75)
Total (N = 58)	50	8

As research materials, they are illustrative, rather than representative in the technical sense of statistical sampling. The award articles have been preselected from what is a very large universe of popular science writing by practitioners and interested participants. In an ethnomethodological sense, then, since these articles are identified for reward as certified models of popular science communication by the significant "encoding" participants, they should provide an excellent site for an investigation of how such communication is constructed, and how it is conditioned by ideology.

As to the "decoding" participants in this communciation, Miller has provided a wealth of audience research. Miller claims to have empirically described the attentive public to science, using measures developed and applied over almost thirty years of national public opinion polling and survey data, culminating in the institutionalization of measures of the public understanding of science in the biannual data celebration of the Science Indicators series, undertaken by the National Science Foundation in the U.S.A.

About 20 percent of the American public are attentive to science, and another 20 percent are "interested." The attentive public has a high level of interest in science, a functional level of knowledge in certain areas of science, and a pattern of gathering relevant information to keep their stock of knowledge current. This segment is "younger, predominantly male, better educated, and more likely to have taken a college-level science course." The interested public for science policy is characterized by a high level of interest in science and technology issues, but lacks the functional knowledge of the attentives in the various fields. That is, although the interested public expresses "a high level of interest in science and technology matters and perceives itself to be at least moderately well

informed...[they are] unable to demonstrate adequate substantive information to remain in the attentiveness classification."[4] This segment follows an information gathering pattern similar to the attentives, but less intensive.

Although Miller does not describe the attentives to the extent of what their favourite beer is, or the kind of car an attentive might drive, the picture that emerges conforms with the conventional elite of North American society: male, primarily between the ages of 17-35, college-educated, and with the perhaps more special characteristic of having been exposed at the college level to a science course.

This interest typification model, developed by Almond and elaborated by Miller, provides a useful tool of analysis and segmentation of audience for popular science communication.[5] All levels of audience on the science policy pyramid have characteristic needs -- uses, and gratifications -- for information.

Decisionmakers need information to guide judgements: empirical measures for reliability. Such information usually comes to the decisionmakers from the science policy leaders "below" them.

Policy leaders need similar empirical data on activities in the scientific communities which they lead, as well as substantive scientific material. Such information comes from disciplinary and interdisciplinary fora, such as the AAAS.

Attentives need information on the controversies and policies for which their support is sought. This specific information need can only be addressed provided there is an adequate level of scientific literacy to background or contextualize issues. The principal sources of information for attentives are usually influenced by the agency of the policy leaders in seeking participation, that is, in the specialized science magazines that are the media of policy leaders and the scientific community, or through news channels that have

been alerted to an issue by the policy leaders as part of attempts to gain support for contested positions.

The interested public has information needs similar to the attentives, but with the distinction of neither requiring, nor being capable of using, higher-order scientific information. Plausibility and social context are required by the interested audience in order to make meaning of science communication -- as distinguished from empirical data for decisionmakers, specialized and "community" information for policy leaders, and some measure of substantive scientific information, contexted by a general science literacy, for attentives. Sources for the interested public are most likely to be the media, broadly defined; radio, TV, and general circulation magazines, particularly.

Nonattentives need consumer-oriented, practical information, of a sort that has immediate application or practical use. As they are least likely to participate in policy processes, attempting to create a scientific literacy among the nonattentives is not feasible, according to Miller. The principal information sources of nonattentives are likely to be newspapers, TV, radio.

Information gathering patterns

An important component in the typology of interest and attentiveness to science and science policy issues described in chapter one is a pattern of regular information acquisition about science:

> While no single variable can completely capture the probability of continued interest and activity, the existence of a previous pattern of information acquisition on the topic or subject area is a reasonable surrogate measure. Given the cost of information acquisition in both time and resources, a pattern of regular information consumption about any specific topic or set of topics would appear to offer a reasonable probability of a continuing activity in regard to that topic or area.[6]

Survey data on information gathering activities showed that 59 percent of attentives were regular viewers of a TV science show, and 80 percent watched TV news

regularly; 75 percent of attentives were regular newspaper readers (but only 9 percent rated newspapers as a vary good source of scientific information); 50 percent of attentives read one or more science magazines, and 50 percent reported reading a general newsmagazine.

Miller and colleagues, in appying information acquisition behaviour as a variable in their examination of attentiveness to science issues, reasoned that the patterns of information acquisition showed evidence of sufficient concern over the issues to expend time and effort. Interestingly, their research lead them to judge that "reliance on print information represented the best measure of a persistence of information acquiring behaviours...."[7]

Use of television and radio were discounted as measures of attentiveness because the widespread and easy access to both mediums requires little effort on the part of a user and therefore "does not represent evidence of a sufficient interest in public affairs to expend scarce time and resources to become and remain informed."[8] The researchers dispense with the scope and quality of televised news and radio as being generally lower than print media.

In terms of the confidence placed in sources of information on science and technology, it appears from the same survey data that the audience for popular science communication, although it may make significant use of the media for information, trusts it less than wholeheartedly. Institutions, such as a Congressional Committee on Science and Technology, "a university professor," and the Environmental Protection Agency enjoyed higher levels of confidence than newsmagazines, television, or radio. Miller suggests that the trust placed in the media is not in the media per se, but rather "in the persons and organizations whose statements or actions are reported by the media."[9] This suggestion will be more fully developed in the rhetorical analysis to follow.

The attentives are the most receptive audience for science policy communications, as they are for science literacy. The attentives seek more sophisticated presentations of current science policy issues, but:

> This group will be relatively unreceptive to one-sided exhortations on policy issues. On the other hand, given some of their difficulties with basic scientific constructs, they are unlikely to be responsive to excessively technical presentations.[10]

Miller notes that increasing scientific literacy among members of the attentive public is critical -- even though the evidence that scientifically literate persons tend to feel more positive about science is not conclusive. A communications strategy for improving the understanding and appreciation of science for this audience, therefore, "should focus on the policy issue and provide intelligent discussion of the policy alternatives," rather than either attempt to create literacy or crudely advocate.[11]

(Since the barriers against increasing scientific literacy among nonattentives are even stronger, Miller suggests that the principal communications goal "in regard to this section of the population is to persuade them that it is important to be scientifically literate.")[12]

In its "purest" or archetypal form, then, popular science communication targets an attentive audience through their preferred medium of print. The awards mechanism allows further definition of the phenomenon through a kind of self-selection. The analytical tools developed from rhetoric and critical linguistics, if brought to work on this material, should be able to determine whether popular science communication has been adequately characterized.

Part two: Rhetorical analysis

The practice of science would seem to call for valor. She trades in knowledge, which is the product of doubt. And this new art of doubt has enchanted the public.
-from Galileo's final speech,
Bertolt Brecht, *Galileo* [13]

Where normal science proceeds, by extending and elaborating paradigms, the popular representations that follow trace the surfaces of certainty to marvel and celebrate. When paradigms are perturbed, scientists are thrown back on their cultural certainties in the face of changing and contradictory puzzle-solving sets. And popular science coverage then approaches even more closely the classical forms of rhetoric in attempts to present the same controversies to everyday reason. The scientific ethos, underlying and functioning in a transparent manner during normal times, becomes foregrounded both in the scientific community and its reflection in science coverage.

The norms of disinterestedness -- or rather offences against it -- seem most commonly evoked in controversial science. There are two reasons for this. The first lies in the framing of drama, conflict, spectacle, and the personification of issues given science coverage to satisfy the structural needs of the media. The second is that in the coverage of disputes, scientists are presented using arguments more directly indexed to the ethos to discredit or impugn rival theories. The scientists seldom seem to oppose fact (when the facts speak for themselves, scientists do not argue), but interpretations. And in the repertoire of discursive strategies used in scientific controversies, attributing an interested motivation is a damning criticism. But it is used with caution: to suggest that some scientists are not motivated to seek knowledge in a pure and disinterested way is to besmirch all scientists. As an accounting device, these sort of appeals to the ethos of disinterestedness are not evoked lightly.

An excellent demonstration of this confirmation by negation is presented in the award-winning articles for 1985. Both concern scientific disputes centred on nuclear war and the arms race. The magazine category award was presented to Andrew C. Revkin for "Hard facts about nuclear winter," which appeared in *Science Digest*. The award for newspapers over 100,000 circulation went to Boyce Rensberger for a series of articles in the *Washington Post* on the Strategic Defence Initiative program, a.k.a. "Star Wars."

Both articles introduce controversy in dramatic-leads, and in a manner that thematizes scientists. The possibility that smoke from a nuclear war "could prove to be more devastating than any of the other effects -- including the blast and radiation," "came as a complete suprise, stunning scientists and defense experts alike." (Revkin, p. 1)[14] The Stragic Defense Initiative has "American scientists...split into rival camps on whether the exotic concept makes technological sense," from which camps scientists are "zapping each other with hypothetical senarios, mathematical formulas and just plain nasty language." (Rensberger, p. 1)[15] Scientists become either "proponents" or "critics" of countervailing theories.

In the Rensberger article, the proponents of Star Wars "accuse their critics of "shoddy work," while the critics "denounce the proponents for 'hyena-like' behaviour." Throughout the article, the proponent and critical technical arguments are presented alternatively. The more fantastic reaches of destructive technology -- particle beam weapons, orbiting battle stations, X-ray lasers powered by nuclear explosions in space -- are consistently countered by multiple, independently targetable reentry vehicles (MIRVs), hardened missles, decoys, aiming-blinding lasers, space mines, and a variety of non-adversarial technical challenges.

The "bitterest and longest running" Star Wars technical dispute involves calculations of how many orbiting battle stations would be needed to provide a comprehensive defense against Russian missiles. Two different sets of calculations, one

posited by the Union of Concerned Scientists (UCS) and identified with Richard L. Garwin ("a physicist who worked on the hydrogen bomb and is now a defense consultant," as he is described: presenting credentials to suggest he is not simply an advocate, but an experienced scientist familiar with the field), the other with military-oriented national laboratories, but identified in the article with Robert Jastrow, NASA (National Aeronautics and Space Administration) astrophysicist.

The UCS calculations based on the deployment of the most promising weapon -- an orbiting chemical laser -- initially showed that it would require 2400 battle stations and extremely high energy inputs to operate -- figures that placed the program cost so far beyond the economic capability to realize that it appeared patently absurd. This argument is countered by proponents suggesting that the "true" number of battle stations required was closer to 90, and that " Garwin and the UCS were either bad mathematicians or trying to kill Star Wars." (Rensberger, p.7) The UCS discovers errors in its calculations, and subsequently revises its estimate downward to a battle station requirement of 800, then 300. Jastrow "pounces" on these errors "citing them as evidence that Garwin and the UCS were misleading the public." The disinterested scrutiny and self-policing ethos appears to have forced an interested scientist into revealing -- and thereby discounting as nonscientific -- motivated error. Jastrow is quoted calling the work of the UCS "the poorest that has appeared in print" and "shoddy" in contrast to "some fine work by the theoretical physicists at Los Alamos." (Rensberger, p. 7)

The article reports Garwin's response:

> Denouncing Jastrow's article as a "screen...rehashing the same demolished criticisms."Garwin added that Jastrow, who is not an expert on strategic defense technologies and who used other people's data, has "made a career of hyena-like behaviour."
>   Not all of the clashes between Jastrow and Garwin are so emotionally worded. Some are more factually verifiable. (Rensberger, p. 7)

In casting Garwin's response as "emotionally worded," and contrasting it to "more factually verifiable" differences, the article rhetorically attaches a negative valuation to a position that seems to break with the norm of disinterestedness. This manoeuver is reenforced in the article's next paragraph. Jastrow is presented critiquing the UCS report (not Garwin himself, as Garwin had been presented, attacking Jastrow in an *ad hominum* fashion) as deliberately chosing to bias the facts toward "the most pessimistic end of a range of performance possibilities."

> "All the errors and omissions," Jastrow wrote, "go in one direction only -- toward making the president's plan seem impractical, costly, and ineffective." (Rensberger, p. 7)

The article returns to Garwin's presentation of new, revised figures of battle station hardware, requirements that seem to address the criticisms of Jastrow, and return UCS figures to the levels first projected. But the argument, rhetorically at least, is lost. The new figures are countered again with a suggestion that Garwin has offended distinterestedness as the Star Wars advocates "complain that Garwin is introducing ... factors to recoup the position lost when the UCS errors were found." (Rensberger, p. 8)

The article ends in rhetorical judgement with a critique appealing directly to the norm:

> "Garwin isn't playing by the rules," said Gregory Canaven. a pro-Star Wars physicist from Los Alamos. Canaven said that without Garwin's newly introduced factors, they would be close to agreement on the number of satellites needed to counter the current Soviet missle force. (Rensberger, p.8)

The calculations would agree if disinterest operated as is normal and essential to the production of scientific knowledge, if Garwin was not motivated by non-scientific interest -- the facts would speak. The same general strategy holds in the other two articles in the series.

Personal computers

In "Computer bugs seen as fatal flaw in 'Star Wars'," controversy surrounds the technical possiblility of designing software for the supercomputer applications needed to control the defense system. Because the Star Wars system would have to respond so rapidly and be so highly effective -- perfect -- there would be no time for human intervention, no time even to "wake the president." This requirement of complete reliance on computers -- in actuality perhaps, one supercomputer -- entails a program of unprecedented complexity and reliability. The fatal flaw that critics point out is that no program of the requisite length and complexity has ever been written, that the normal programs are never error-free, and that there will never be a way of testing the Star Wars program under realistic conditions.

The argument is again presented in the article in turn-taking debate sequences between "opponents" and "advocates." The "spokesperson" for the critical argument is David L. Parnas, "one of the computer world's most respected authorities on large-scale programming." Parnas is given a measure of credibility, as it is noted that he was appointed to the Pentagon's Strategic Defense Initiative Organization (SDIO) to the advisory panel on "battle management software," and that "he supports Reagan's goal of eliminating the threat of nuclear weapons and ... has worked on military aircraft computing problems for many years." (Rensberger, p. 9)

Parnas resigns from the committee after its first meeting. His reasons are reported in quoting from his letter of resignation:

> "In March 1983," Parnas wrote in his letter of resignation, "the president asked us, as members of the scientific community, to provide the means of rendering nuclear weapons impotent and obsolete. I believe that it is our duty as scientists and engineers to reply that we have no technological magic that will accomplish that." (Rensberger, p. 9)

This would appear to be an exemplary case of a scientist guided by the norms of disinterestedness: a demonstration of high integrity, the unimpaired adherence to a higher moral imperative of science in the face of allegiance to the state and the "defense of democracy." It appears to be a difficult decision, made by a scientist represented as having no previous commitments against military research.

Parnas reportedly accompanies his resignation with eight technical papers that explain why he believes the software will not work, and by his actions "galvanized the software engineering community and set the terms of a debate that continues to rage...." (Rensberger, p. 9)

In the technical exchanges that follow, Parnas counters the arguments of the computer panel scientists with examples grounded much more closely in experience than those of the advocates: the Space shuttle, the field experiences of Vietnam, analogies to personal computers, the ethnographic "testimonies" of other experts. Advocate argument only once touches on appeals to the ethos:

> "Parnas is putting his finger on some real technical problems," said Charles
> Seitz, a computer panel member from California Insitute of Technology,
> "but these are things that SDI is researching. While Parnas is going about
> debating, we're studying these problems. The honest answer right now is
> that there is nothing that assures us it can be done or that it can't be done.
> Existing software engineering practice has never encountered a problem
> quite like this before." (Rensberger, p. 11)

This passage represents Seitz acknowledging, in a way disinterestedness would require, the potential merit of the technical arguments. But that truth is undercut by the suggestion that "going around debating" is more an interested position-taking, an excessive calling of attention to problems, rather than working to resolve them as good scientific practice would suggest. The article further portrays the panel "[f]or all its optimism" concluding that "there are limits to what software can do." Although these are positively valued aspects of the ethos, the strong moral character of Parnas carries the debate.

In concluding, the article offers in a parallel construction, a parallel positive valuation of what would seem at first an admission of defeat: "[f]or all their pessimism, most critics concede that if the government keeps spending money on SDI, someday there will be a huge computer program that SIDO calls battle management software." And in the classic close with "judgement," the debate seems decided for the critics:

> "But this software will not have the reliability that you or I would consider essential for such a system," said James J. Horning of Digital Systems Research Center in Palo Alto, Calif. "Nor will it be possible to retrofit reliability into it. The country will be faced with a cruel dilemma: deploy a system that cannot be trusted, or scrap it." (Rensberger, p. 11)

The rhetorical "judge", Horning, has not previously appeared in the article. His closure has the independence and appeal to personal, everyday reason -- "you or I would consider" -- that marks this as rhetoric. The issue remains technically ambiguous, but a preferred decision has been reached through a rhetoric centred on scientists and their ethos.

Religious wars

In the last article of this series, "H-bomb blast planned to test Star Wars idea," the disinterestedness aspect of the ethos of science is most strongly evoked and dramatized.

The article introduces a controversy: the testing and development program for a secret X-ray laser. The device entails a hydrogen bomb detonated in space that would release energy that a laser would convert into a burst of X-rays just one-millionth of a second before the device is destroyed by the blast. The aimed X-rays could destroy a hostile missle as their energy causes a surface layer of atoms to "boil off" next to the skin of a missle so rapidly that the recoil effect damages it. Controversy has arisen with the disclosure that some project physicists have discovered "major flaws" in a previous round of tests, but that the information was suppressed by others advocating the project.

The advocates "described the results in misleadingly optimistic terms" in requesting an additional $100 million for the project from the Reagan administration and congressional leaders. The actions are condemned as a breach of disinterestedness:

> "Scientific organizations such as Lawrence Livermore [National] Laboratories, the developer of the concept] have an important responsibility to provide the most evenhanded information they are capable of providing," said Ray Kidder, a senior leader of hydrogen bomb research at the lab. "That's because the people making the policy back in Washington don't have the technical expertise to evaluate these things. They have to rely on us for good information to guide them. The lab has fallen a bit short of meeting that responsibility." (Rensberger, p. 12)

The "overly zealous proponents" are Edward Teller, "a longtime Reagan friend, and his protege, Lowell Wood," suggesting in one sentence a scientifically compromised relationship between Teller and Reagan, and between Teller and Wood. Wood is described as "a zealot":

> "He believes with a religious fervor that the X-ray laser is going to save the United States from the Russians. He's absolutely convinced of it. He hates the Russians with a passion. You've got the Holy Grail zealot in Lowell Wood and right behind him is the political clout of Edward Teller. You don't want to base policy on the ravings of a zealot." (Rensberger, p. 12)

The article notes that repeated efforts to reach Teller, Wood, or other advocates of the X-ray laser were unsuccessful -- a journalistic euphemism that suggests suspicious evasion, and offends the scientific ethos of openness and communication. This interpretation is strengthed as the scientist "nominally in charge of Livermore's nuclear program" and one of its presumed advocates, is quoted offering "evenhanded" information, cautioning against premature optimism. Three-quarters of the article is taken up in the interplay of scientists and their ethics: the remainder presents a technical description of the device and the problems besetting it in the familiar turn-taking format.

Having represented the advocates of the X-ray laser as Russian-hating zealots willing to transgress scientific norms, the article adds a chilling Dr. Strangelove flavour to the rhetorical thrust against Teller and Wood. The delivery system of the X-ray laser is a

"pop-up" approach. That is, it is not installed in orbit, where it might be vulnerable to neutralizing attacks, but is envisioned as fired from submarines. The system must be deployed at the instant of a perceived attack --.presumably by the supercomputer software previously described -- in order to be properly positioned to fire at rising Russian missles. As the article notes:

> The necessity of a virtually instantaneous pop-up, critics say, would mean the United States would have to fire nuclear weapons into space without presidential authorization.(Rensberger, p. 13)

This is the liberal-democratic nightmare of nuclear war, begun without consensus.

In two of the three articles the "opponents" of Star Wars seem to carry the rhetorical debate. The struggle is ideological: on one level it is between the political nature of the mandate given science for defense research, and the autonomy of the scientific enterprise. On another, it is a struggle for the trust and authority of science. Scientists are represented in a way that preserves their moral status even while they are engaged in war research.

The debate is cast rhetorically in the terms of the ethos of science, and the rhetorical decision is encouraged by attaching positive and negative values to scientists' representing different positions. The ethical behaviour of scientists becomes the measure of science, of the truth of science. In this way popular science communication translates science -- and in the process transforms it in a direction that encourages relations of authority and trust -- into everyday reason.

Credible authority

Edward Teller makes a reappearance in Andrew C. Revkin's article on nuclear winter, "Hard facts about nuclear winter." But whereas Teller's work on the X-ray laser was impugned for having offended disinterestedness, in "Hard facts" he presents the same arguments *against* the proponents of the nuclear winter theories.

The article reports the development and debate over the theory posited by a group
of researchers that a global nuclear war could have a catastrophic impact on climate. The
pall of smoke from thousands of fires ignited by nuclear blasts, the researchers suggested,
would produce surface darkening and thermal effects sufficient to produce widespread
subfreezing land temperatures for a prolonged period. This severe 'nuclear winter' could
extinguish a major portion of the plant and animal species on the earth -- including the
humans that may have survived the initial warfare.

The theory was proposed by Richard Turco, an atmospheric scientist, and three
researchers from the NASA Ames Research Center (O. Brian Toon, Thomas Ackerman,
and James Pollack), along with astronomer Carl Sagan. The group became labelled
TTAPS, an acronym based on their last names.

Once again, the theories are presented for public understanding and decision in the
article through the rhetorical attachment of the values of the scientific ethos. Edward
Teller, presented as leading "a small but powerful cadre of critics," attacks the TTAPS
reports "arguing that the studies were inconclusive and politically motivated.":

> "The only news," Teller says, "is that Sagan has made a lot of propaganda
> about a very doubtful effect." (Revkin, p. 2)

However, this criticism is countered, seemingly overwhelmingly, by reference to
satisfaction of the univeralism imperative:

> Three congressional hearings, dozens of scientific meetings, several
> international conferences and at least four books later, *nuclear winter* has
> taken its place -- somewhere between *megaton* and *overkill* -- in the
> burgeoning lexicon of terms spawned by the study of nuclear war. After
> more than a year of scrutiny, the TTAPS study has held up, at least as a
> "first order" estimate.

The theories are then rhetorically argued against on the basis of ambiguity, that
the smoke pall could produce the opposite effect in a "nuclear summer." The argument is
again countered by a universalistic appeal, noting that the National Research Council (a

"neutral" academy) published an "exhaustive review" that gave as a best estimate the "clear possibility" of the nuclear winter hypothesis. The argument is later reversed on the critics and pointed with reference to the interest-driven generation of a contervailing interpretation:

> Mark Harwell, a Cornell biologist who recently finished a book on the subject, says, "You don't have to go to the extreme bounds of any of these ranges of uncertainties to be able to generate a nuclear winter. Actually, the converse is true. To come up with a war that does *not* generate a nuclear winter, you have to go to extremes." Commenting on the critics of TTAPS, he adds, "You can talk youself into saying, 'Gee, I could have a nuclear war that didn't lead to nuclear winter,' but it takes a lot of soft-shoe routine. I've seen people do it." (Revkin, p. 5)

Having established nuclear winter as a major new concept in the study of nuclear war, the article proceeds to another troubling question: how could a consequence of such significance have been overlooked by science? The answer lies in the presentation of an argument that seeks to show that it is again an interference in the autonomy of science, a loosening of the norm of disinterestedness that is responsible.

> John Birks, one of the authors of the *Ambio* study [which originally posited the theory of a nuclear winter], offers two answers: "Defense scientists, who are the ones funded to look at this, are not attuned to this sort of thing. Their job is to build weapons to prevent war. Their work has focused on prompt effects of single nuclear explosions. They have had no incentive to look at global effects of multiple nuclear explosions. [Neither did "independent" university scientists twig to the significance of the nuclear smoke pall.] "You don't get brownie points in academia for studying something as applied as nuclear war. You don't get promotion and tenure and things like that. So there's no incentive there either." (Revkin, p. 7)

The missing incentive is the autonomous, disinterested search for the truth that is part of the ethos necessary to produce scientific knowledge. The article elaborates on this theme by suggesting that the vagaries of funding research programs that change as government priorities change has impaired science.

In the final arguments mounted in the article, the nuclear winter theorists seem to be chastised, nevertheless, for having jeopardized scientific authority by transgressing the norms of disinterestedness. Sagan and the TTAPS group are criticized from a neutrally

located source for "having damaged his scientific credibility by discussing the political implications before the validity of the theory is confirmed." (Revkin, p. 8) The corrective is administered, in which it is clearly suggested that the power science can exert comes from the production of certified knowledge, and that evidence of having followed the ethos is the essential security of that knowledge. Robert Ayres of the Carnegie-Mellon Foundation:

> In his contribution to TTAPS critic Fred Singer's forthcoming book about nuclear winter, Ayres says, "The transparency of Sagan's motives may have detracted from the effectiveness of his argument...The nuclear hardliners in the Reagan Administration will be ultra-suspicious of any conclusions based on mathematical models that are not completely and fully tested and verified. They will point out, correctly, that it would be doubly disastrous if the U.S. leadership believed in the Sagan thesis while the Soviet leadership did not." (Revkin, p. 8)

The conditions of doubt

Perhaps one of the most striking examples of the rhetorical attachment of the ethos of science is found in the article by C. P. Gilmore, "After 63 years, why are they still testing Einstein?"[16] The article initially plays on the wonder that scientists might question the theories of the ultimate genius and icon of science:

> For, though they have formed the basis for much of the thought and exploration in physics for more than half a century, the astonishing fact is that there is still doubt about their validity. Even Einstein himself had doubts. (Gilmore, p. 58)

The rhetoric plays on both the disinterestedness of science, and the organised skepticism that challenges dogma -- even scientific dogma -- and tells of the elaborate tests of the special and general theory of relativity and that "hundreds of scientists have spent enormous amounts of time checking the results of these tests." (Gilmore, p.58)

> Together, the two theories proposed that the world is not at all as it appears to human senses, but has instead a set of astonishing properties that seem to violate common sense. (Gilmore, p. 58)

But scientists can accept even revolutionary theories, provided they can satisfy the tests of organized scepticism. Organized skepticism requires the "temporary suspension of

judgement and the detached scrutiny of beliefs in terms of empirical and logical criteria,"
and the courage to disturb the "cleavage between the sacred and the profane, between that
which requires uncritical respect and that which can be objectively analyzed."[17]

The Gilmore article is a sustained documentary of the length (widths, heights, and
other calibrations) to which scientists go in order to scrutinize Einstein's theory, purging it
of any particle of dogmatic belief. Scientists shoot lasers at the moon, signal spacecraft
250 million miles from Earth, fire atomic clocks into space, and run calculations based on
the bending of starlight around the sun during an eclipse -- performing enormously
complex, tedious and esoteric experiments.

> "Testing relativity is a tough way to make a living," says Dr. Shapiro.
> "First, the tests usually involve fantastic amounts of data, In one test I
> was involved in, we gathered no fewer than 4×10^{11} bits of data (that's
> four followed by 11 zeros). And then we had to boil all these down to
> something like one digit." (Gilmore, p. 59)

> "We have about 8000 roles of computer tape," Dr. Shapiro told one
> scientific gathering, "which, if unrolled, would reach back and forth from
> West Virginia to Massachusetts several times. And sometimes I think that
> would be easier to do than to interpret them." (Gilmore, p. 62)

But for all their elaborate attempts to falsify or empirically confirm relativity
theory, scientists appear to be actually "rooting" for Einstein, an ironic demonstration of
interest and attachment to the theory.

> "Among scientists interested in relativity, there seems to be an
> undercurrent of emotional attachment to Einstein's theory, a hope that the
> old master will turn out to be right in the end," reported a recent article in
> the journal *Science*. That may be unscientific, but there does appear to be a
> feeling that Einstein, who has been hard to prove wrong for more than half
> a century, now deserves to be considered right. (Gilmore, p. 64)

Coming as it does nearly at the conclusion of the article, this admission that
scientists are not simply rational automata, but human too, also serves a rhetorical
function to attach an everyday sense of justice to the constant inquisition of experiment.
Scientists are not presented as so detached from everyday experience that they are
alienated from the common person. Scientists are just more determined to reach the

objective truth, less willing to accept even an extremely powerful theory, until the institutional and normative imperatives of skepticism have been satisfied.

Although some may unscientifically root for Einstein, even as they try his theories in the in the court of skepticism, the ethos of disinterestedness preserves the integrity of scientists. The "rigorous policing" and "exacting scrutiny of fellow experts" that is entailed in the "public and testable character of science"[18] and the ethos of disinterestedness means that scientists produce knowledge without favour or prejudice. This self-policing and the integrity of knowledge and action that come from it are critical to the public acceptance of scientific knowledge and authority. Because the ethos prohibits it, scientific knowledge, and scientists are trustworthy:

> The possibility of exploiting the credulity, ignorance, and dependence of the layman is thus considerably reduced. Fraud, chicane, and irresponsible claims (quackery) are even less likely than among the "service" professions. To the extent that the scientist-layman relation does become paramount, there develop incentives for evading the mores of science. The abuse of expert authority and the creation of pseudo-sciences are called into play when the structure of control exercised by qualified compeers is rendered ineffectual.[19]

The meaning of life

A truly ludicrous example of the use of the ethos rhetorically appears in the article "How did all life begin?", by Keay Davidson, award-winner in 1986 for newspapers over 100,000 circulation.[20] In an article covering the Fifth International Conference on the Origin of Life at UC-Berkeley in July of 1986, Davidson depicts scientists as an argumentative, egocentric group, producing ever more outlandish theories. On the origin of life, "tempers have flared, scientific reputations foundered, and friendships soured" over a "gaggle of possible answers." (Davidson, p. 1)

Rhetorically, this article accomplishes a disparagement of origin-of-life studies, rendering them as incredible, and pseudo-science -- an exemplary warning of the consequences of deviation from the norms of science. The theories are reported in a

sequence that moves outward from probability and upward on a scale of grotesqueness: the origin of life is presented as the chance combination of chemicals in a primordial soup; descending from creatures emerged from crystallized clay deposits; as the spew of undersea vents; as borne on comets and meteorites that crashed on earth; and as deliberately deposited on the planet by aliens.

The field is a "tiny" and an "emotional one," represented as having more to do with personal beliefs than fact. Critics from "legitimate" disciplines are drawn on for comment, and suggest that "origin- of- life researchers, eager to prove some pet theories, have produced a huge pile of terrible science, really poorly done science that just doesn't stand up." Chemist Robert Shapiro, who is accredited critical commentator status throughout the article, describes the study as:

> ...having a tainted 'reputation (that) resembles the one, in days gone by, of a maiden of doubtful virtue, whose every appearance in public was accompanied by a background of unpleasant whispers.' (Davidson, p. 1)

The leading theory, that life originated when electrical energy passed through the primeval atmosphere to produce amino acids, the basic units of organic life, is reported as being disproven in subsequent understanding of the composition of that early atmosphere and contradictions in the amount of time such an evolution would take. Stanley Miller, the theory's proponent is represented as "still clinging" to a modified version. The critics "are dismayed," by this behaviour, and as Shapiro is quoted:

> Many origin-of-life scientists "have not responded to increasing adverse evidence by questioning the validity of their beliefs, in the best scientific tradition; rather, they have chosen to hold it as a truth beyond all question, thereby enshrining it as mythology," Shapiro charges in his book, 'Origins - - A Skeptic's Guide to the Creation of Life on Earth'.... (Davidson, p.2)

Shapiro here is the spokesperson of organized skepticism, as if the title of his book did not clearly indicate. When the norm is not followed, Unquestioned beliefs become no better than enshrined mythology, equivalent in rhetorical terms to the fundamentalist/creationist interpretations of the origin of life.

Generally rare in the sample of articles examined, in this article the physical appearances and personality quirks of scientists are given prominent play. They serve to reenforce the confirmation-by-negation of the norm. Miller is a "short, mild mannered" man, "whose eyes dart about nervously and whose softspoken comments sometimes trail off into mumbles." His chief antagonist over a specious scientific argument is "Fox of Miami" a "tall, rather handsome man in his 70s, with a cunning smile." The exchange reported between them is petty and ill-tempered:

> Like generals on opposite sides of a battlefield, Miller and Fox have a relationship that is less than cordial. Fox, Miller said, "speaks in, let's say, a different tongue. He doesn't talk scientific sense, in my book."
>
> Fox responds that Miller feels insecure because the original experiment wasn't his but, rather, that of his doctoral supervisor, Urey. "Stanley did a good job as a technician, but it's a question whether that's enough to earn a Ph.D," Fox added coldly. (Davidson, p.3)

Neither speaks scientific sense, but rather a discrediting discourse. Elsewhere, one of the theories is described as "flamboyantly irresponsible...wanton, amusing, promiscuous fiction." The final insult of the article juxtaposes the "scientific" theories, now thoroughly discredited by the deviant behaviour of their proposers, with "creation science," which teaches a literal interpretation of the Bible. The origin-of-life scientists "scorn one viewpoint even more angrily [than the most outrageous of their own] -- that of the creationists."

> "Everyone who is here believes there's a scientific solution to the problem of the origin of life," Margulis [Lynn Margulis, a Boston University biologist] said. "Nobody here thinks there is an 'extrasensory' solution or theological solution or 'divine intervention' solution." She is an atheist and thinks many other biologists are as well. (Davidson, p. 5)

This is the ultimate offense. Clearly throughout the article, the various theories are presented as beliefs, vociferously, unscientifically held -- as unquestioned, as enshrined, as mythologized as creationism, so adroitly referenced at the close of the article. Suggesting that many other biologists are atheist drops the debate to one motivated by dogma, and most certainly attaches a negative rhetorical value to it. Origin-of-life science is relegated

to the status of a pseudo-science, fit only to contend with other pseudo-sciences like creationism.

And finally, in the closure of judgment, one A. Graham Cairns-Smith, proponent of the theory that life began with inorganic matter -- arisen from clay. Cairns-Smith has been previously described, virtually as the pagan god Pan incarnate: "a slight, witty man with a puckish grin and curly dark hair:"

> "It's a half-baked subject," Cairns-Smith joked. "Sometimes I wonder whether it's real science."
> "But that's what cutting edges are like."

This comic piece illustrates Merton's warning observation that one of the connections between science and the social order, is one in which esoteric science has become a popular mysticism.[21] The increasing complexity of science and the specialized training necessary to understand or produce it has meant that scientists have "necessarily subscribed to a cult of unintelligibility."[22] The understanding gap between the scientist and the laity has been extended so wide that the lay person must take on faith "publicized statements about relativity or quanta or other such esoteric subjects" that run counter to common sense. Science and esoteric knowledge are often linked in a vocabulary of popularization that seems to stress, through invoking wonder and amazement appeals, the dissonance between scientific knowledge and common sense. The danger, Merton warned -- at a time when the racialist policies of fascism were being given scientific camouflage -- is that such a mode of communication can become part of the mechanisms of dominance.

> The presumably scientific pronouncements of totalitarian spokesmen on race or economy or history are for the uninstructed laity of the same order as announcements concerning an expanding universe or wave mechanics. In both instances, the laity is in no position to understand these conceptions or to check their scientific validity and in both cases they may not be consistent with common sense. If anything, the myths of totalitarian theorists will seem more plausible and are certainly more comprehensible to the general public than accredited scientific theories, since they are closer to common sense experience and cultural bias. Partly as a result of scientific advance, therefore, the population at large has become ripe for new mysticisms clothed in apparently scientific jargon.[23]

What Merton did not anticipate was the contribution his own work would make to that process, nor that the closing of the gap could become one of the ways in which science continued to be mystified and used in controlling social order.

Part three: Critical linguistic analysis

Don't you see that the whole aim of Newspeak is to narrow the range of thought? In
the end we shall make thoughtcrime literally impossible, because there will be no
words in which to express it. Every concept that can ever be needed, will be expressed
by exactly one word, with its meaning ridgidly defined and all its subsidiary
meanings rubbed out and forgotten....'Has it ever occurred to you, Winston, that by the
year 2050, at the very latest, not a single human being will be alive who could
understand such a conversation as we are having now?'

-Syme, philologist and compiler of the Eleventh Edition
of the Newspeak Dictionary, to Winston Smith.
George Orwell, *1984.*[24]

As defined in the preceding methodological chapter, the language of popular science
communication works in an ideological manner to control meaning. A characteristic
linguistic feature of this ideology is the dislocation of meaning from a specific context and
concrete expression with clear cause-and-effect statement, and the movement, through
syntactic transformation, of the representation of an event having cause or agency
towards a reinterpretation that locates it in a context presenting more general or less
immediate causation. Attribution of agency or causality is weakened or deleted by selection
or abstraction of effects, coupled with an explanation of those transformed effects by
causes of higher generality or abstraction. In this way, ideological work can be
accomplished to suppress, deny, or prefer meaning.

In popular science communication, the ideological work accomplished is to deny any
social contingency or uncertainty of scientific knowledge, in order to create and maintain
its authoritative status. The ideological conditioning at work will be located through
changes involving passivization, agent deletion, rewording, nominalization and embedding.
Texts will be analysed for transformations that move the representation of scientific
knowledge or methodology away from any social contingency, or recast its certainty and
authority in an idealized manner.

Performing a linguistic-based analysis on whole texts, as distinguished from the sentence or phrase constructions that are the usual grounds of such analyses, would be a daunting task. It is not possible within the scope of this thesis to accomplish such an analysis. (Indeed, there are few attempts. The most outstanding is Teun A. van Dijk and Walter Kintsch, *Strategies of Discourse Comprehension*, which attempts to develop a textual linguistics based on cognitive and interpretive processes. Over 400 pages of analysis follow from a two-page text drawn from *Newsweek* magazine. Even so, the authors do not claim exhaustiveness.[25]) The development of more sophisticated computer-assisted textual analysis based on artificial intelligence languages and linguistic systems may permit this type of "megacontent analysis," or "content mega-analysis." However, for the purposes at hand, a very careful selection from the repertoire of analytic tools has been made. The attempt to use these tools appropriately follows from certain assumptions made about the larger textual characteristics of popular science communication.

Typology of popular science texts

It is useful to make a further distinction within the range of popular science texts in the process of analysis. Of the two social purposes generally granted popular science communication -- the creation or development of science literacy and the provision of information appropriate to decisionmaking on issues involving science policy -- the first type is closest to exposition.

In the sample under analysis, for example, there are articles that are more explicitly directed towards informing policy. Policy articles are those such as those on the space shuttle program. One series, written in 1981 before and at the time of the first launch, looks at the endeavour in terms of a number of questions on the feasibility of the program against a technical background. Another set, written in 1986 in the wake of the destruction of the shuttle, addresses the question of responsibility, technical and other, for the explosion. Both clearly involve public issues. On the other hand, a number of the

articles have no obvious policy connection. Examples from the awards sample include articles on the new theoretical physics, the continual testing process involved in acceptance of relativity theory, or the construction of a space telescope. These last would seem to be simple exposition of scientific knowledge, which requires no public decision.

Cloitre and Shinn set out a typology of scientific exposition that can be used to characterize specialist, inter-specialist, pedagogical, and popular forms.[26]

Texts are analysed according to three categories of discursive features: argument, referent, and imagery. Classes within these categories further specification of features. For example, the referents one might find used in scientific exposition would include phemonena, the material entities or processes that are the objects of discourse; the experimental protocols and techniques used in the scientific practice, including methodology and instrumentation; research results in allied or neighbouring fields; historical accounts; and industry, including technological and economic factors.

Imagery can be further specified: graphs; geometric plots; icons, or "natural" representation such as photographs or drawings; reified imagery, or the representations of theoretical entities and forces; schemas, or selected structural features; and metaphors, which in Cloitre and Shinn's definition are linguistically based, multiple-referent constructs.

Argument in scientific exposition is restrictive, that is, circumscribed in order to achieve precision or in which propositions are strongly entailed; quantitative; and qualitative, or intuitive.

Popular exposition

When this system is used to describe scientific texts, certain patterns serve to characterize the different types of exposition. Specialist communication, by this scheme,

makes much more use of references to experimental protocol and phenomena, uses graph imagery, and restrictive, quantitative argument. Such specialist exposition only weakly exhibits the characteristics of qualitative argument, or the use of industry as referent.

Popular science texts can also be identified in the field of scientific exposition as exhibiting more strongly certain qualities of discursive features. Cloitre and Shinn note especially the stronger presense of historical referents, the predominance of iconic and metaphoric imagery, and the preferred use of qualitative argument.

Scientific exposition can be viewed as a continuum, and the various types are placed within it according to the characterizing strength or weakness of features drawn from this typology. Scientific exposition is a sort of distributive field, like a grammar, rather than a set of exclusive branchings. Any scientific text could include all or many of the features. It is in the strength of manifestation or of relations among the feature types and qualities that determines the nature of the text.

Popular exposition, for example, "almost always contains a quantitative dimension." But the quantitative arguments or elements function not to refine and restrict to degrees of precision, but are of orders of magnitude that function to impress or "boggle."[27]

Metaphoric imagery, of the kind commonly employed in popular texts, is contrasted with analogic imagery more frequently used in other texts:

> While analogy operates through deconstruction and comparison (a is like b) where similarities and limitations are always stipulated, in metaphor significance is generated through a process of fusion and even con-fusion (a is b).[28]

The knowledge derived from the type of communication strongly characterized by the use of metaphoric imagery, non-quantitative argument, referents other than

phenomena or the observational techniques used in the examination of phenomena is mystified. It is:

> [D]egenerated knowledge...knowledge which is so constituted so that the nature and relationships of its component elements totally preclude an unequivocal, or even, a coherent 'multivocal' grasp of the phenomena.[29]

But while such degenerated knowledge impedes a reconstruction of phenomena, it provides an excellent substitution in an "elaborately extra-phenomenological cluster" that gives the exposition a persuasive coherence and usefulness. Cloitre and Shinn provide a useful "whole text" parallel or frame for an analysis using linguistic categories on smaller units within texts. The transformations of transactive and nontransactive linguistic models in the texts under analysis is interpreted in much the same way -- as degenerating the context and contingency of scientific knowledge in order to reinscribe it in new models that give it more powerful coherence and certainty. In popular science communication, the boundaries of meaning are blurred in transformation, both metaphorically and syntactically, and new meanings created. Critical linguistics seems more appropriate to expository texts -- as defined by Cloitre and Shinn -- than to the type of "provision of relevant policy." Rhetoric adequately analyses the policy texts, as that is its function -- to persuade publics on issues of concern. Expository texts are more directly about knowledge claims, no less ideological, but less permeable to rhetorical analysis.

In the following analysis the direction of the transformation is traced through the various linguistic transcriptions entailed, and it is suggested that the process prefers new meanings that enhance authority.

The mystifying universe

The first sequence of transformations examined is in the 1982 award-winning article by Timothy Ferris, "Physics' newest frontier," from the *New York Times Magazine*.[30]

> Responsible for the breakthrough are new theories of physics -- known as "unified theories" -- that seek to improve scientific understanding of how nature functions from the very smallest to the very largest scale. These theories[,] which stand on the frontier of physics, are most precisely expressed not in words, but as mathematical equations. They imply that all the known forces in nature are manifestations of one basic interaction and that once, long ago, all were part of a single universal force or process. (Ferris, p. 37)

In the first sentence of this sequence, the oddly awkward thematic fronting of "responsible for the breakthrough" attributes an agency to the new theories, while burying the true agents -- the scientists who produce the theories -- and even further usurps the human agentative role by being "responsible", that is, "liable to be called to account" for producing breakthroughs. "Breakthrough" is itself a nominalization of "to force a way through against resistance:" a very important advance or discovery made when someone or something breaks through a barrier to knowledge or problem. Again, although the agency is not clearly stated, it is attributed to the theories, and in a way that displaces human agency. The actions of the scientists in producing the theories, and any knowledge of the relations or conditions of production has been lost in this transformation. In fact, in the next phrase, the anaphoric function (the repetition, or carrying back) of the pronoun phrase "...that seek to improve scientific understanding ...." shows the new theories acting on understanding that would in a clearer formulation be shown as producing the theories.

And as if to lift them decisively out of the sphere of human influence, the new theories stand on the frontier of what is known, already characterized as being "most precisely expressed" not in words, which are uniquely human artifacts, but in mathematical equations. They "imply," as a necessary part or condition -- not in the indirect, suggesting way -- an ultimate knowledge that unifies all others. Such powerful knowledge could not be created by so mundane and imperfect a process as consensus among scientists, but:

> Like archeologists unearthing the ruins of an ancient city, theorists are uncovering the outlines of a new, more profound and, in some ways, simpler physics....(Ferris, p. 38)

The knowledge is not constructed in any sense, but exists independently, only to be uncovered and expressed in mathematics.

But scientists are not just drones, labourers without power of their own. Scientists have their own agency, to wield the power scientific knowledge provides. Almost all of the verbal models in which scientists figure in this article are transactive, with scientists as the subject participants. ·

Physicists, theorists, and experimenters *seek, probe, ponder, discover, uncover, unearth, see, envision, find, report, study, reason, calculate, postulate, formulate, predict, investigate, explore,* and *reconstruct.* The verb forms are strong mental process agentive models, and most of the activity is, of course, connected to cognitive processes, to the manipulation of knowledge. Notable by their absense are verbal forms which might be associated with more ambiguity, modally less certain verbs such as *speculate, interpret,* or even *believe.*

The agency of scientists practising knowledge is strengthened by contrast to a textual environment of nontrasactivity. Phenomena in the universe are described mostly in nontransactive models, and figure in the text as self-generated, acausal, mysterious.

> Magnify your view of this letter -- x -- one million times, and you can see the molecules the ink is made of. This is the realm of chemistry. Magnify a billion times more, and you can see the atoms the molecules are made of -- the realm of atomic physics. Select one atom, magnify it 10,000 times, and you are inside the nucleus -- the realm of nuclear physics. (By now we can no longer see; light waves have become as large as Pacific groundswells viewed from a rowboat.) Smash the nucleus and its particles disintegrate in a bomb blast of still smaller particles. This is the world of the particle physicist. (Ferris, p. 38)

The sequencing and transformations in this passage are strange and disorienting. *Magnify* is ambiguous. Is it a command? If so, from whom? Or perhaps the deleted subject *someone* can be read back into the surface construction, as *Someone magnifies.* The context

suggests that the someone is the scientist, applying magnitudes of power to vision to increase the apparent size and accessibility of different realms of phenomena.

The strangeness is intensified with the shift in pronouns from *you*, to *we*. You, attributed with the power of science, have magnified phenomena millions and billions of times (boggled perhaps, as Clotre and Shinn would suggest, by the orders of magnitude involved), put yourself in the nucleus of atoms, and smashed that nucleus to cause a bomb blast. The inclusive *we*, now including the author of the article and exclusive of scientists, become blind and bobbing on an alien sea of lightwaves powerfully swelling from the energy of some distant and unknown storm. *We* have been suddenly stripped of the power of science to magnify, smash and retain control over phenomena. The climax of the sequence is an explosive disintegration into complexity. But, out of the chaos of strange forces, (sometimes seeming in our control, sometimes not) at their most energetic, the particle physicist emerges. It is a miraculous birth. "This is the world of the particle physicist."

The other levels of power, of magnification, are the realms of different, traditional, disciplines within science. This movement itself is a neat piece of lexicalization that classifies different levels of the material universe as the regal holdings of science. But the most powerful realm is that of the particle physicist, who emerges in person to take possession of it.

The object universe

The universe is a strange and powerful place, but one on which those who wield the power of scientific knowledge can act:

> The universe, as astronomers have found, is expanding, the clusters of galaxies hurtling away from one another. Run the expansion backward, physicists reason, and you come to a time some 20 billion years ago when everything must have been smashed together in a state of titanic heat and density. (Ferris, p.41)

The universe is expanding is a nontransactive, *the universe expands,* transformed with the auxiliary *be* and the aspectual form *-ing,* to a model that ostensibly expresses simultaneity with a specified period of time -- in this case the present. But the surface form *expanding* takes on some of the syntactic status of an attribute, and the construction a relational one. Thus, *expanding* is a transformed construction moved towards a status more of a state. It is employed both in the syntactic position and with the assumed meaning of an adjective.

At a deeper level, the structure of the first sentence could be recovered as, *Astronomers have found that the universe is expanding.* (In unmarked form, perhaps: *Astronomers find x, x = The universe expands.) As,* functioning as a relative pronoun, introduces the transactive phrase *astronomers have found.* This transactive is in the past tense, perfective aspect. That is, the action is completed, and more certain. This form, using *have* as auxiliary, in addition to being a temporal form, also shows that temporal relations are at least partially understood in terms of possession. Completed actions are quasi-possessions.

In this sentence, then, we have a structure in which the universe is again approaching a state-like condition. This state of the universe appears highly energetic, complex, and mysterious. Scientists appear clearly as agents, with temporal priority of action, and having a degree of possession over the universe. The universe is at least the object of their understanding -- scientists have found it to be expanding, the galaxies hurtling, and in that discovery they have taken a form of possession over it.

The trajectory of this transformation is made clearer by the next sentence. The nontransactive *expanding* has been transformed into a nominal, *expansion.* This transformation completes the effacement of agency in the universe, shifting thematic focus from the participants or causal agents of action to process. The change of verb to noun entails a corresponding shift in the range of meanings: process moves toward state,

activity toward object, specific toward general, and concrete toward abstract. A complex of relations is collapsed into a single entity, and that entity enters into new constructions with a different function.

In the case of this sequence, nominalization allows physicists to appear to manipulate the process of the universe expanding, even to running it backward. Physicists remained empowered by their knowledge, able to transactively apply reason to the universe. The effects of applying this scientific reason are to once again spacialize time and move from a present (in which the universe is expanding) to come to a past where the universe is completely changed. Matter and energy are one, but the physicist, syntactically at least, stands outside the moment of creation, assuming a perspective and a power previously only attributed to gods.

The lives of theories

Theories take on a life of their own in popular science accounts. Although scientists may appear to give birth to them, theories quickly seem to assume autonomous status. Thus:

> The concept of antimatter originated at the climax of a period of intense intellectual ferment that saw the birth of a revolution in physics. This activity resulted in one of the most powerful descriptions of nature ever conceived -- quantum mechanics, which has been called by Murray Gell-Mann "that magnificent and confusing discipline." (Fisher, p. 1)[31]

> An early unified theory, created by James Clerk Maxwell in 1864, established that electricity and magnetism are aspects of a single entity, the electromagnetic field. Maxwell's success in discerning unity behind what had seemed to be separate interactions has inspired phyicists ever since. (Ferris, p.44)

Theories may have their genesis with scientists, but in the process of transformation, move further and further from this source, gaining power and certainty. The generative agency of scientists is progressively removed as the scientific method is seen to test and refine any contingency away. In C. P. Gilmore's article, "After 63 years,

why are they still testing Einstein?", a cycle of certainty is run over and over again throughout the text. The account of a candidate theory is presented first as "Einstein's" or as belonging in some way, as to "the reigning giant of physics...Sir Isaac Newton," or as "according to Einstein." As theory develops this origin is suppressed or transformed. While still questionable or "revolutionary," it is associated with its proposer. Once confirmed by the objective criteria of science, theories are alienated from such possession, in linguistic concert to the rhetorical movement that establishes their conformity to the ethos of disinterestedness. In the transformation, certainty accrues. Thus:

> Mass is not constant either, *he said*, but it *will appear* to change as the relative speed of the mass and the observer measuring it change. The faster something goes, the more massive *it appears*. Light *doesn't* move in straight lines, but is bent and slowed by gravitational fields. And gravity *is not simply* a force acting at a distance between two bodies, *as Newton said*. *Einstein viewed* gravity as a warping of the fabric of space. (Gilmore, p. 58) (Emphasis added.)

Yet, only two paragraphs later, the identical propositions are made, but the linguistic formulation attaches much more certainty:

> Relativity predicts that size and mass change with speed, and by how much. It predicts that the flow of time is different at different speeds, and again by how much. It predicts that light bends in a gravitational field, that the speed of light is slowed by gravity, that time is slowed by a gravitational field, that gravity and acceleration are the same thing, and that accelerating bodies produce gravity waves. And it gives numerical values in all of these instances that can be checked against experimental results. (Gilmore, p. 59)

"Relativity" is the clear subject operator, and drives a series of parallel constructions that subordinate mass, the flow of time the speed of light, both syntactically in subordinate clauses, and in verbal models that are principally relational rather than actional. In the first sequence, an informal usage, coupled with a weakened modal certainty -- including a number of negations -- present a less authoritative account. The propositions about phenomena are thematically fronted, even though in the underlying structures they are predicate clauses. The focus suggests a precedence over the agency of

the proposers, a relation that is reversed in the second sequence, where theory has primacy of place and action.

These exemplifications are part of a larger system of linguistic transformations that supress or alienate any contingency from the representation of scientific knowledge. Further, they are supplemented with other linguistic strategies, such as attaching or attenuating certainty to scientists or theories, or focusing on the accuracy and precision of measurement or instrumentation -- so as to enhance the representation of scientific knowledge as authoritative.

NOTES: Chapter four

[1]   AAAS, Constitution (Seventh; effective January 1, 1973), Article II, Objectives, Handbook: Officers, Organization, Activities, 1986-1987 (Washington: AAAS, 1986), p. 133.

[2]   AAAS *Handbook*, p. 109.

[3]   The 1985 judges for print awards included the senior science writer for the *New York Times*, the publisher of *Scientific American* magazine, an academic from the Massachusetts Institute of Technology specializing in science communication, a former Presidential advisor on science, and three other scientists -- one from the federal government, another from the Smithsonian Institute.

[4]   Jon D. Miller, *The American People and Science Policy: the role of public attitudes in the policy process* (New York: Pergamon Press, 1983), p. 43.

[5]   Jon D. Miller, "Reaching the attentive and interested public for science." Sharon Friedman, Sharon Dunwoody, and Carol L. Rogers (Eds.), *Scientists and Journalists: reporting science as news* (New York: Free Press (Macmillan), 1986),pp 55-69.

[6]   Jon D. Miller, Robert W. Suchner, and Alan M. Voelker. *Citizenship in an Age of Science: changing attitudes among young adults* (New York: Pergamon Press, 1980), p. 99.

[7]   Miller et al., (1980), p. 115.

[8]   Miller et al.,(1980), p. 115.

[9]   Miller et al., (1980), p. 113.

[10]   Miller, (1986), p. 67.

[11]   Miller, (1986), p. 65.

[12]   Miller, (1986), p. 66.

[13]   Bertolt Brecht, *Galileo* (New York: Grove Press, 1966), p. 123.

[14]   Andrew C. Revkin. "Hard facts about nuclear winter," *Science Digest*, March 1985. References to this article are made to a special AAAS reprint made up from the actual magazine pages, and numbered pp. 1-8. All subsequent references to the article will appear in the form (Revkin, p. 00)

[15]   Boyce Rensberger, "'Star Wars' splits experts into two camps," *Washington Post*, March 1985 (Incorrectly cited in AAAS awards reprint). References to this series of articles: "'Star Wars' splits experts into two camps," (March 1985); "Computer bugs seen as fatal flaw in 'Star Wars'," (October 1985); and "H-Bomb blast planned to test 'Star Wars'" (December 1985) are made to a special AAAS reprint numbered pp. 1-14. All subsequent references to the article will appear in the form (Rensberger, p. 00)

16 C.P. Gilmore, "After 63 years why are they still testing Einstein?," *Popular Science*, December 1979, pp. 58-64. All susequent references to this article will appear in parentheses as (Gilmore, p. 00).

17 Robert K. Merton, "The normative structure of science," *The Sociology of Science: theoretical and empirical investigations*, Edited and with and introduction by Norman W. Storer (Chicago and London: University of Chicago Press, 1973), p. 277, 278.

18 Merton, "Normative," p. 276.

19 Merton, "Normative," p. 276, 277.

20 Keay Davidson, "How did all life begin?", *San Francisco Examiner*, August 6, 1986. Reference to this article is made to a special AAAS reprint numbered pp. 1-7. All subsequent references to the article will appear in the form (Davidson, p. 00)

21 Robert K. Merton, "Science and the social order," *The Sociology of Science: theoretical and empirical investigations*, Edited and with and introduction by Norman W. Storer (Chicago and London: University of Chicago Press, 1973), pp. 254-266.

22 Merton, "Social order," p. 263, 264.

23 Merton, "Social order," p. 264.

24 George Orwell. *1984* (Harmonsworth: Penguin Books, 1984), p. 48.

25 Teun A. van Dijk and Walter Kintsch, *Strategies of Discourse Analysis* (New York: Academic Press, 1983), 418 pages.

26 Michel Cloitre and Terry Shinn, "Expository practice: social, cognitive ,and epistemoligical linkage," *Expository Science: forms and functions of popularization*, Terry Shinn and Richard Whitley (Eds.) (Sociology of Science Yearbook, Vol. IX, 1985)(Dordrecht, Holland: D. Reidel, 1985), pp. 31-60.

27 Cloitre and Shinn, p. 48.

28 Cloitre and Shinn, p. 48.

29 Cloitre and Shinn, p. 48.

30 Timothy Ferris, "Physics' newest frontier," *New York Times Magazine*, 26 September 1982, pp. 36-70. All subsequent references to the article will appear in the form (Ferris. p. 00).

31 Arthur Fisher. "New Ferment in the mirror world of antimatter/antigravity," *Popular Science*, July 1986. Reference to this article is made to a special AAAS reprint numbered pp. 1-7. All subsequent references to the article will appear in the form (Fisher, p. 00).

CONCLUSION:

QUESTIONING AUTHORITY

Authority implies some warranting: authority can only be exercised over those who consent to it, and to have authority is to be able to induce compliance or belief in another. The inducement draws its force from some legitimating principle, in respect of which both parties consent to an unequal relationship of power. But though there may be an inequitable distribution of power in such a relationship, the consensual nature of authority may provide grounds for negotiation of legitimate power. As Connolly has suggested:

> Authority is effective because the parties *believe* it to be grounded, to be justified by relevant considerations. Thus superior knowledge or incumbency in a special position, the parties believe, justifies voluntary obedience by B to A's initiatives as a means of supporting broad social interests and objectives. But central to the idea of authority is the possibility that these beliefs might be mistaken or faulty in particular cases. We can thus have effective authority that is not grounded, and grounded authority that is not effective. Debates over the proper limits of authority, then, can be understood as efforts to establish considerations that warrant or ground authority and to state the limits within which these warrants properly operate.[1]

Popular science communication is a kind of textual negotiation of authority for science and its community of interests. But the negotiations themselves remain unequal in popular science communication, as the previous chapters have sought to demonstrate.

Popular science texts persuade with a kind of degenerate knowledge, or attempt to institute an unequal relationship of power through discursive controls. The grounds on which a legitimate authority might be established are excluded from understanding, or remain limited to the specialist domain of science.

With the potential for power systematically closed from negotiation by the modes of communication employed, all that remains to be determined is the manner of deference to

the authority of science. Popular science communication works principally to expand, not limit or condition, the warrant of science. The ideological characteristics of the communication work to obscure or suppress the possibility of faulty or mistaken belief, a possibility which in the political domain reserves some of the power of the warrant.

The previous chapters have attempted to demonstrate that popular science communication operates in two modes to secure a public warranting of science. The persuasive and controlling modes both turn on authority.

The persuasive mode was characterized as rhetoric and operationalized for analysis as referenced to a special ethos that was a measure of trustworthiness and authority. The "moral" character of scientists in terms of this ethos was made the measure of science.

The controlling mode was characterized as an ideological representation of scientific knowledge that suppressed, excluded, or transformed "uncertainty," or any social contingency for greater authority. The connection to any human agency informed by legitimating principles that might be examined and questioned is thereby transformed and the mechanism of authority in science itself is hidden, dislocated from specifics of context and causal relations.

The virtues of rhetoric

The rhetorical analysis revealed that in articles that were ostensibly about scientific or technological issues requiring public deliberation and decision -- such as commitment to the "Star Wars" defense policy, or the credibility of nuclear winter theories and their entailed consequences for disarmament policies -- arguments turned on the virtues of scientist proponents and opponents. This rhetorical grounding of argument in the personal virtues of scientists is a persuasive strategy, not an exposition of scientific knowledge. Texts are constructed not to inform decisions, but to guide them.

This is the legitimate function of rhetoric. Given the barriers to understanding presented by the specialized nature of scientific knowledge, the boundaries erected for its production and certification, scientific knowledge must be translated into common terms if it is to be communicated. However, the "common terms" of the rhetoric used in the science popularization texts examined in this thesis are ideological in origin, and the appeal to them serves more to naturalize this ideology, than it does communicate science.

The ideology is particularly useful in articulating science and the social order. The scientist is represented as a model citizen of liberal democracy, balancing individual and public interests in the free operation of a voluntary community that evolves universally valid standards as authority. In addition, as the production of knowledge is bound up with an ethos, a trust in the ethical behaviour of scientists translates into trust in the knowledge that they produce.

Rhetoric need not be malignant. There is a moral aspect to rhetoric, as Rom Harre notes:

> Aristotle's theory tends to enhance the moral standing of all concerned by emphasizing that 'rhetoric is the counterpart of dialectic,' and he supposes that the ultimate persuasive discourse is rational....Dialectic, the theory of correct reasoning, defines what is proper in a scientific discourse. Rhetoric, the theory of effective reasoning, defines what is potent in a persuasive discourse.[2]

Popular science communication as practiced, however, is immoral manipulation when its rhetoric appeals to positivistic "ideal" science for the purposes of utility to political decisionmakers, or for the purposes of scientific policyleaders attempting to secure resources. A virtuous rhetoric of popular science communication would not impair reason in order to privilege an ideology.

Meaning under control

Critical linguistics seems more appropriate to expository texts -- as defined by Cloitre and Shinn -- than to the type of text that seeks to guide and inform public issues. Rhetorical analysis is adequate to the policy texts, matching them in both form and function. Expository texts are more directly about knowledge (or knowledge claims), no less ideological, but less permeable to rhetorical analysis.[3]

The linguistic analysis provided evidence to suggest that popular science communication tends to use modality to indicate authority and develop a certainty of utterance, even when dealing with theoretical concepts in science. This movement is akin to the process in science itself that Kuhn described as the "invisibility of revolutions." Kuhn notes that the sources of authority in science (as well as the popular and philosophical accounts of science) record the stable *outcomes* of change, not the processes:

> The temptation to write history backward is both omnipresent and perennial....More historical detail, whether of science's present or of its past, or more responsibility to the historical details that are presented, could only give artificial status to human idiosyncrasy, error, and confusion. Why dignify what science's best and most persistent efforts have made it possible to discard? The depreciation of historical fact is deeply, and probably functionally, ingrained in the ideology of the scientific profession, the same profession that places the highest of all values upon factual details of other sorts.[4]

The presentation of scientists as having the power of agency -- and that power deriving from the nature of the knowledge they possess -- is also an underlying semantic encoded in these models of popular science communication. Such a presentation promotes a trust and belief in the efficacy of scientists and scientific knowledge, establishing an authority without having demonstrated its legitimacy clearly.

While contingency is suppressed or absent, measurement accuracy and methodology (which cannot adequately be interpreted by a nonscientist) are foregrounded to evoke certainty in popular accounts. Popular science texts appear to convey information

about the activity of science and the nature of the knowledge it produces, but do not deliver it. In this, science popularization reveals its characteristics as a language of control.

Questioning authority in texts

The analysis forwarded in this thesis rests on the adequacy of the understanding it provides of the nature of popular science communication. But that adequacy, in turn, is best measured in terms of the pragmatic force it has in countering the persuading, controlling nature of popular science communication, or in at least enabling a more open questioning and negotiation of authority in popular science texts.

The struggle with texts is not a vitiated version of more significant action, but a necessary first intervention. As Gunther Kress presents it:

> Clearly, words alone will not interrupt the processes at work. However, an understanding of the ideological and political effects of texts, and an understanding of their function in all social, economic and political processes is just one, but a necessary part of a strategy for intervening in the totality of these processes. It is important to know what texts come into being via whose agency; what readings are constructed in these texts, and how they might be resisted or subverted; what readers are envisaged for these texts and how these readers are positioned in these texts. Strategies to counter the weight of the dominant forces in this field are political strategies carried via linguistic/textual means.[5]

Understanding what discourse is, and how it constitutes reading positions based on social place and functions to position readers, are important elements of a strategy of opposition. Kress suggests that the task of analysis should be to interrupt this ideological positioning by constructing texts which offer different, alternative positions. Or in another direction, he suggests developing texts which encourage resistant readings of other, specific texts, instituting a kind of discursive struggle that could eventually overturn the dominant discourse.

Attempting to develop a sustainable practice or habit of critical reading -- rather than creating various positions in which a reader might find himself or herself -- might be

a better strategy for avoiding subjegation by discourse. Any textual strategy that does not sustain the power of interpretation of the reader must be suspect, no matter how sympathetically motivated. Part of this ongoing negotiation of authority must involve the production of texts that do not attempt to control meaning and position readers primarily as receivers, passive interpreters.

Every social action, cultural product -- or text -- is a *resource* for creating meanings. The forms of analysis that attempt to extract singular, authoritative or scientific meanings are inadequate. Or worse, they are repressive and seek to impose meaning in a restrictive interpretation. An alternative method of reading, of analysing texts, is needed, one that "preserves the phenomena," and keeps interpretation active, decentred, and productive of meaning.

Michael Mulkay has proposed such a method, appropriately enough, in a sociological examination of scientific discourse in a self-reflexive mode that seeks a more suitable correspondence of analytic form to social action. It is not my intention to extend the argument of this thesis to defend the specific analytical proposals made by Mulkay. However, the work has useful parallels for a strategy of critical reading and a communications model that involves relations of interpretative and communicative equality.

The word and the world

The principle exposition of this approach, an analytic text titled *The Word and the World*, is itself conceived and executed as a kind of "collaborative analytic dialogue" in which meaning is continually authored by analyst, social actor, and reader.

The style is a play of genres, including letters, one-act dramas, interview transcripts, straight scholarly dissertation, and other formal gamings. The cohering focus

is on a very specific scientific dispute: the process of oxidative phosphorylation in cell organelles called mitochondria.

Mulkay demonstrates that the textual practices employed by the two scientists on opposite sides prevent understanding: they are locked in an unresolvable struggle over interpretation in which each of the participants asserts a privileged access to the factual, real discourse of truth. The textual practices used in the struggle are imported from a genre (that of published research papers) that uses an authoritative, empiricist repertoire of meaning, a repertoire that requires a social and scientific outcome of subordination or superordination in discourse. The problem of meaning cannot be resolved.

Mulkay is also at work demonstrating that a social understanding of science can lead to productive practices. The problem manifests one of the processes of producing scientific knowledge elucidated in sociological analysis. Scientific knowledge is seen to be produced by interpretation directed by agreed upon methodology. If and where such procedures produce incompatible, incommensurable accounts, it is the interpretation that must be negotiated. In the case of the "ox phos" debate, negotiation breaks down -- or rather, is inevitably doomed to failure -- because the monologic form of texts used by both scientists denies the interpretive work that produced them, and leaves little or nothing of authority to negotiate.

For his own work in this analysis of inequitable communication, or asymmetrical relations of power in texts, Mulkay provides an "analytical dialogue." He opens his interpretation to one of the principals in the scientific dispute, first in an exchange of letters, then in a face-to-face dialogue ranging over all of the previous work. For a reader, this also provides an opportunity to see how Mulkay's own interpretive work is received and accepted, opposed, or negotiated by one of the social actors. It is a type of participatory research, similar in some respects to the type of audience-based communications research undertaken by Morley.

The intrepretive process in these exchanges confirms the importance of dialogic discourse for both natural and social sciences. It focuses on two essential concepts in natural science, and their treatment in the aspiring social science, replication and discovery. Both are textual procedures -- accounts of phenomena, or discourse -- entailing interpretive strategies.

In science, experimental replication is used to validate or verify knowledge, by "proving" that certain facts persist through different experimental conditions. Validation is at once linked to both sameness and difference. That is, replication confirms an interpretation in a procedure sufficiently different so as not to be mere repetition of phenomena (a logical impossibility), but sufficiently similar to be construed as a determination of the same, and therefore "objective" or real, facts or relations.

The essential verification processes of natural science are based on an interpretative account that allows construction of two different meanings from the same data -- replication, similar yet different. It follows that meaning analytically derived from texts by sociological (or any other) analysis is also interpretively accomplished.

But this characteristic of analysis need not be taken as a criticism, impugning either natural science methodologies or sociolgical ones. Mulkay turns a problem into a resource. Analysis *should* be reflexive, dialogic, and create an awareness of text as a resource from which the interpretative work of analysis produces *a* meaning or specific meanings from the potential multiplicity. Analysis must not be determining of its texts:

> ...the self-referential character of the sociological analyses of discourse is not something to be rejected or hidden, but rather to be welcomed and celebrated....

This self-referential quality of analysis is further elaborated in a one-act play. A scientist and three sociologists begin an argument about replication. In the course of the play, the dialogue makes a "strange loop" and the characters come to exchange positions

and repeat each others positions (the sociologists in their turn claiming replication procedures prove sociological theory, the scientist critiquing such proof as a convention of theory and a socially negotiated process). The point also made is that their texts are infinitely reproducible, similar but different. Social negotiation of meaning at the basis of both natural and social science observation is demonstrated with its authority both clearly visible and argument held contingent on consent to its legitimating principles.

The same sort of analytical dialogue is undertaken on discovery, generating a similar wealth of meaning. Discovery:

> ...is not to be treated by the analyst as a distinctive kind of action or product, but as a *method* whereby a particular intrepretive status is attributed to specific actions and/or textual products by those involved.[8]

Mulkay exposes the authority of sociological analysis by opening it to other forms -- particularly literary forms -- in a struggle to maintain a consciousness of textuality, and enhance the equality of interpretive exchange.

Parody of authority

Mulkay provides -- with a virtuosity, but one that undercuts itself so as not to prevent its usefully being taken up by analysts and readers alike -- a textual strategy suitable communicating about science without conceding the struggle for authority. He draws attention to the multiplicity of potential meanings of social action as text in focusing on a literary genre -- parody.

Like analysis, parody is a secondary text that is based closely on an original, but that differs in ways that allows observation of essential meaning (a replication technique), and that asserts a certain superiority of interpretation (a discovery technique) in so doing. Mulkay merges sociological analysis and parody to create "analytic parody," a form that is at the same time richer in meanings and more appropriate to the phenomena of discourse in its self-referentiality.

> Whereas conventional sociological analysis claims interpretive privilege for its own text, analytical parody directs attention to the possiblity of carrying out diverse kinds of sociological analysis, that is, saying various new and interesting things about the production of the social world, by means of any and every conceivable literary form (including of course,as one textual form, conventional analysis).[9]

This proposal begins by seeming eccentric, but achieves an explicit and continually acceded authority in its exposition of techniques of analysis.

Social science and communications seem driven in their normal operation to fix meaning in "scientific" methodologies, giving interpretive analyses an authoritative text. Mulkay, who has perhaps penetrated further into science with sociological analysis than most, has seen the essential absurdity of that paradigm for social analysis. The closer the approach of the social analysis to the object of analysis, the more the object dissolves into the relations of the analytic discourse.

Nondeferential treatment of science

Science might be better communicated in forms that parody authority, so that it is always consciously assented to, argued for, and legitmated in a manner that does not abuse understanding. It is difficult to imagine the character of such changes, and more difficult to imagine them realized. Science popularization has been very useful in its present form.

In the discourses that accompany the domination of meaning, that are ideological movements, the anomalies -- the alternatives. oppositions, ambiguities, the dissenting negotiations -- are reconciled; supressed. or transformed. The contingent and social is made to seem natural. and the uncertain made to seem obvious. Kress:

> The accounts provided within one discourse become not only unchallenged, but unchallengeable, as 'common sense.' If the domination of a particular area by a discourse is successful, it provides an integrated and plausible account of that area which allows no room for thought; the social will have been turned into the natural.[10]

There is a method of questioning authority, suggested by Richard Sennet in drawing on the work of French novelist Andre Gide, even an authority in which legitimating principles are systematically removed from negotiation and naturalized. It involves a "democratic deformation" of the reproduction of power relationships, a reflection *en abyme* that at the same time distorts.

> A reflection which is not quite the original has a social as well as a moral dimension. *En abyme* suggests a method for thinking about how the reproduction of power can be disoriented. The method is to treat controls as propositions rather than axioms at each echelon. A proposition can be validated, disproved, or seen to be both true and false. But if at every point in a link the validity and implications of a rule have to be discussed, then an active, interpretative search for the meaning of power is inaugurated, the activity of authority-making itself.[11]

This method requires entering more consciously into dialogue with texts, with the authority in texts. Dialogue, as Mulkay also suggests, is a form of discourse especially suited to explicit interpretive work, and brings into the open interpretive work hidden behind the authority of the text.

Dialogue in its dialectic, spirit of contradiction, turn-taking, pro and con characteristics has always been a form conducive to thought and the resolution of problems. It was also the form in which the first systematic written attempts at scientific popularization were cast, by Bernard Le Bovier de Fontenelle (1657-1757) in the seventeenth century.[12] Fontenelle's *Entretiens sur la pluralitie des mondes (Conversations on the Plurality of Worlds, 1686)* was a "brilliantly successful popularization of the Copernican system, which, until that time, had achieved very limited acceptance," and his works served as "the single most important bond between the philosophical-scientific revolution in process during his life and the philosophe movement just getting underway."[13] Fontenelle was a forerunner of the Enlightenment, engaged in a project of combining knowledge of nature and knowledge of human nature, before such attempts turned to ideology. Restoring something of the vitality and innovation of that time and that form to science popularization might produce a transformation in the relations of science

and the public, and restore something of the equity that has been missing ever since that time.

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> *But the little that is left* [of popular science communication, after the elimination of the deceptive and the esoteric] *is very valuable indeed. It is of great importance that the general public be given an opportunity to experience -- consciously and intelligently -- the efforts and results of scientific research. It is not sufficient that each result be taken up, elaborated, and applied by a few specialists in the field. Restricting the body of knowledge to a small group deadens the philosophical spirit of a people and leads to spiritual poverty.*

-Albert Einstein, 1948.

Foreword to Lincoln Barnett, *The Universe and Dr. Einstein* (New York: William Sloane Associates, 1948), p. 1.

NOTES: Conclusion

1   William E. Connolly, *The Terms of Political Discourse* (Second Edition), (Princeton, N.J.: Princeton University Press, 1983), p. 113.

2   Rom Harre, "Persuasion and manipulation," *Discourse and Communication: new approaches to the analysis of mass media discourse and communication*, Teun A. van Dijk (Ed.) (Berlin; New York: Walter de Gruyter, 1985), p. 127, p. 129.

3   Although Fahnestock has shown a potential for rhetorical analysis even in more expository texts, if original scientific texts as well as their popularized accounts can be assembled for analysis. The accommodations and rhetorical movements can then be directly mapped to the originals. In comparing original scientific reports and their subsequent transformations into popular accommodations, she identified appeals to certainty as an important element of the rhetorical attachment of praiseworthy values to science.

4   Thomas S. Kuhn. *The Structure of Scientific Revolutions* (Chicago and London: University of Chicago Press, 1962), p. 137.

5   Gunther Kress, "Discourses, texts, readers and the pro-nuclear arguments," *Language and the nuclear arms debate: Nukespeak today*, Paul Chilton (Ed.) (London and Dover. N.H.: Frances Pinter, 1985), p. 67

6   Michael Mulkay, *The word and the world: explorations in the form of sociological analysis* (London: George Allen and Unwin, 1985, 263 pp.).

7   Mulkay, p. 155. Mulkay is also using celebrated in a special sense, detailed in the closing sequences of the study. Celebration is meant as a ritual, or mature confirmation of the outcome of a process of negotiation, something of a public declaration of fact.

8   Mulkay, p. 174.

9   Mulkay, p. 174.

10   Kress, "Discourses," p. 72.

11   Richard Sennet, *Authority* (New York: Vintage, 1981), p. 179.

12   According to Maurice Goldsmith, *The Science Critic: a critical analysis of the popular presentation of science* (London and New York: Routledge and Kegan Paul, 1986), p. 3. Also Otis Fellows, "Bernard Le Bovier de Fontenelle," *The Encyclopedia of Philosophy* (New York: Macmillan, 1967).

13   Fellows, "Bernard Le Bovier de Fontenelle," p. 208.

APPENDIX A: AAAS Award-winning articles, 1980-1986.

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Broad, William J. "NASA had solution to key flaw in rocket when shuttle exploded." *The New York Times*. 1986 September 22. Reprinted by the American Association for the Advancement of Science - 1986 AAAS-Westinghouse Science Journalism Award: Newspaper over 100,000: 9-15; 1986.

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Davidson, Keay. "How did all life begin?" *San Francisco Examiner* 1986 August 6. Reprinted by the American Association for the Advancement of Science - 1986 AAAS-Westinghouse Science Journalism Award: Newspaper over 100,000: 1-7; 1986.

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Fisher, Arthur. "New Ferment in the mirror world of antimatter/antigravity." *Popular Science* July 1986. Reprinted by the American Association for the Advancement of Science - 1986 AAAS-Westinghouse Science Journalism Award: Magazines: 1-7; 1986.

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Hayes, Paul G. "Riding the ill winds of sulfur. The acid trail/Part 1." *The Milwaukee Journal* 1984. December 23. Reprinted by the American Association for the Advancement of Science - 1984 AAAS-Westinghouse Science Journalism Award: Newspaper over 100,000: 1-5; 1984.

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_____. "Jobs, trees and acid rain. The acid trail/Part 3." *The Milwaukee Journal* 1984. December 27(?). Reprinted by the American Association for the Advancement of Science - 1984 AAAS-Westinghouse Science Journalism Award: Newspaper over 100,000: 9-12; 1984.

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Joyce, Fay S. and Charles Stafford, "Analysis." *St. Petersburg Times.* 1981 April 24. Reprinted by the American Association for the Advancement of Science - 1981 AAAS-Westinghouse Science Journalism Awards: Newspapers over 100,000, wire service and syndicates: 44-47; 1981.

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