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BOUNDARY-WORK AND THE DEMARCATION OF SCIENCE FROM NON-SCIENCE: STRAINS AND INTERESTS IN PROFESSIONAL IDEOLOGIES OF SCIENTISTS*

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The demarcation of science from other intellectual activities—long an analytic problem for philosophers and sociologists—is here examined as a practical problem for scientists. Construction of a boundary between science and varieties of non-science is useful for scientists' pursuit of professional goals: acquisition of intellectual authority and career opportunities; denial of these resources to "pseudoscientists"; and protection of the autonomy of scientific research from political interference. "Boundary-work" describes an ideological style found in scientists' attempts to create a public image for science by contrasting it favorably to non-scientific intellectual or technical activities. Alternative sets of characteristics available for ideological attribution to science reflect ambivalences or strains within the institution: science can be made to look empirical or theoretical, pure or applied. However, selection of one or another description depends on which characteristics best achieve the demarcation in a way that justifies scientists' claims to authority or resources. Thus, "science" is no single thing: its boundaries are drawn and redrawn in flexible, historically changing and sometimes ambiguous ways.

Philosophers and sociologists of science have long struggled with the "problem of demarcation": how to identify unique and essential characteristics of science that distinguish it from other kinds of intellectual activities. Comte ([1853] 1975:72) distinguished positive science from theology and metaphysics in his evolutionary law of three stages, arguing that only science used "reasoning and observation" to establish laws of "succession and resemblance." Popper (1965:34, 41) proposed "falsifiability" as a criterion of demarcation: if a theory cannot, in principle, be falsified (refuted) by empirical data, it is not scientific. Merton (1973: Chap. 13) explains the special ability of modern science to extend "certified" knowledge as a result, in part, of the institutionalization of distinctive social norms (communism, universalism, disinterestedness and organized skepticism).

Recent studies, however, suggest that attempts to demarcate science have failed (Bohme, 1979:109), and that the *assumption* of

a demarcation between scientific and other knowledge is a poor heuristic for the sociology of science (Collins, 1982:300). Characteristics once proposed as capable of distinguishing science from non-science are found to be common among intellectual activities not ordinarily labeled scientific, or they are found not to be typical features of science-in-practice (e.g., Knorr et al., 1980; Elkana, 1981:41; Broad and Wade, 1982:8-9). Some dismiss demarcation as a "pseudo-problem" (Laudan, 1983:29).

Continuing debates over the possibility or desirability of demarcating science from non-science are, in one sense, ironic. Even as sociologists and philosophers argue over the uniqueness of science among intellectual activities, demarcation is routinely accomplished in practical, everyday settings: education administrators set up curricula that include chemistry but exclude alchemy; the National Science Foundation adopts standards to assure that some physicists but no psychics get funded; journal editors reject some manuscripts as unscientific. How is the demarcation of science accomplished in these practical settings, far removed from apparently futile attempts by scholars to decide what is essential and unique about science? Demarcation is not just an *analytical* problem: because of considerable material opportunities and professional advantages available only to "scientists," it is no mere academic matter to decide who is doing science and who is not.

This paper restates the problem of demarcation: characteristics of science are examined

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not as inherent or possibly unique, but as part of ideological efforts *by scientists* to distinguish their work and its products from non-scientific intellectual activities. The focus is on *boundary-work* of scientists: their attribution of selected characteristics to the institution of science (i.e., to its practitioners, methods, stock of knowledge, values and work organization) for purposes of constructing a social boundary that distinguishes some intellectual activities as "non-science." Boundary-work is analyzed as a rhetorical style common in "public science" (Turner, 1980:589; cf. Mendelsohn, 1977:6), in which scientists describe science for the public and its political authorities, sometimes hoping to enlarge the material and symbolic resources of scientists or to defend professional autonomy. The paper examines both *style* and *content* of professional ideologies of scientists, as illustrated in three examples: first, public addresses and popular writings of John Tyndall, an effective "statesman for science" in late Victorian England; second, arguments over the scientific status of phrenology in early 19th-century Edinburgh; third, a 1982 policy report by the National Academy of Sciences on scientific communication and national security.

SOCIOLOGICAL THEORIES OF IDEOLOGY

Two long-standing theoretical orientations dominate sociological studies of ideology, and these are especially visible in analyses of occupational or professional ideologies (cf. Carlton, 1977:24–28; Geertz, 1973:201). *Strain* theories are associated with Parsons (1967:139–65, 1951:331–54): ideologies provide "evaluative integration" in the face of conflicting demands, competing expectations and inevitable ambivalences of social life. They are symptoms—as well as symbolic resolutions—of role strain, contradiction, and disequilibrium (White, 1961; Sutton et al., 1956; Johnson, 1968). *Interest* theories are associated with Marx (e.g., [1846] 1976:28–30; cf. Seliger, 1977) and Mannheim (1936): ideologies are "social levers" or "weapons" used by groups to further their political or economic interests amidst universal struggles for power and advantage. They are manipulations of ideas to persuade people to think and act in ways benefiting the ideologist (Birnbaum, 1960; Winter, 1974).

For example, the ideology of business leaders has been explained alternatively as the result of "strains . . . in the business role" such as "conflicts between the demands of the particular position and the broader values of society" (Sutton et al., 1956:11, vii), and as "attempts by leaders of enterprises to justify

[their] privilege" through "expedient rationalizations of . . . material interests" (Bendix, 1963:xi, 449). The two theories are sometimes presented as mutually exclusive and competing: Sutton et al. (1956:12) "reject" the theory that "ideologies simply reflect . . . economic self-interest," while Seider (1974:812) finds the "Marx-Mannheim theory was . . . more useful than Sutton's role-strain theory in predicting the content of public political ideology" of business leaders.

The effectiveness of strain and interest theories has been impeded by "theoretical clumsiness" (Geertz, 1973:196) resulting, in part, from an "anarchy of linguistic differences" (Oakeshott, 1980:viii; on the diverse definitions of "ideology," cf. Mannheim, 1936; Birnbaum, 1960; Lichtheim, 1967; Gouldner, 1976; Larrain, 1979). The two theories agree substantially: both see ideologies as symbolic representations (whether sets of ideas, beliefs, values, wishes, consciousnesses or world-views); both suggest that ideologies selectively distort social "reality"; both assume that adequate explanation requires examination of the social context of ideological statements, focusing on structural sources and functional consequences of ideas. To add to the confusion, followers of Parsons allow that interests are "certainly an important determinant of ideological reaction" (White, 1961:9), while Marx traced the origins of ideology to the desire of ruling classes to conceal *contradictions* between the means and the social relations of production (cf. Larrain, 1979:45–61).

Geertz has taken two steps toward clarifying sociological theories of ideology. First, he rightly suggests that strain and interest theories need not be incompatible: an ideology can, at once, smooth inconsistencies and advance interests (Geertz, 1973:201). Second, Geertz recommends that sociologists examine the *rhetorical style* of ideological statements (cf. Dibble, 1973). Both strain and interest theories direct attention to social functions of ideologies while largely ignoring patterns in the symbolic formulations and figurative languages of ideologists. Geertz (1973:212–13) proposes the study of "stylistic resources" used in constructing ideologies: how do ideologists use literary devices of metaphor, hyperbole, irony, and sarcasm, or syntactical devices of antithesis, inversion, and repetition?

Thus, Geertz identifies two gaps in our understanding of ideology, one related to its content, the other to its style of presentation. First, if *both* strains and interests affect the content of ideology, a more encompassing theory will be required to articulate the *interaction* between them in the construction of ideological statements. Do strains and interests

play different roles in the formulation of ideologies? Second, what causes stylistic variation in the rhetoric of ideologists? Can we identify specific social conditions in which an ideology might be expected to take one or another stylistic form? The following analysis of professional ideologies of scientists begins to fill these two theoretical gaps.

Ideology and Science

The relationship between "science" and "ideology" has been described in significantly different ways (cf. Larrain, 1979:13–14). In a classic positivist tradition, the "certain" truth of scientific knowledge is the only means to detect discrepancies between ideological distortion and the way things "really" are (e.g. Comte, [1853] 1975:72; Durkheim, 1938:31–33; Parsons, 1967:153). In the short-lived "end-of-ideology" debate (Bell, 1962), science and ideology sometimes assumed a zero-sum relationship, so that "increased application of scientific criteria for policy determination [comes] at the expense of . . . political criteria and ideological thinking" (Lane, 1966:649). Retreats from naive positivism have taken several directions. Some suggest that because ideology inevitably intrudes into the construction of scientific knowledge—in social science (e.g., Zeitlin, 1968) and natural science (e.g., MacKenzie, 1981)—the line between scientific truth and ideological distortion is difficult to locate. Others suggest that the language of science is used to legitimate palpably ideological assertions: Braverman (1974:86) describes Taylor's "scientific management" as ideology "masquerading in the trappings of science." Still others define science as an ideology itself (Marcuse, 1964); for Habermas (1970:115) the *form* of scientific knowledge embodies its own values of prediction and control, and thus may substitute for "the demolished bourgeois ideology" in legitimating structures of domination and repression. Finally, to come full circle from Comte's positivist faith in the ability of science to separate truth from politically motivated distortion, ideology becomes a source of liberation *from science*: "it is one of ideology's essential social functions . . . to stand outside of science, and to reject the idea of science as self-sufficient," and to expose "the egoism, the barbarism and the *limits* of science" (Gouldner, 1976:36).

A common thread runs through these diverse descriptions of the relationship between science and ideology: all *assume* that science carries its own intellectual authority. In order for science to expose ideological distortion, or to legitimate capitalist structures of domination, scientific knowledge must be widely accepted

in society as a preferred truth in descriptions of natural and social reality. Yet none of the perspectives asks *how* science acquires that intellectual authority. Part of an answer to this large question will come from investigations of professional ideologies of scientists: What images of science do scientists present to promote their authority over designated domains of knowledge?

Curiously, ideologies of science have received only sporadic sociological attention (Daniels, 1967; Greenberg, 1967; Reagan, 1969; Tobey, 1971). Mulkey offers a promising agenda: he analyzes Merton's four norms not as constraints on scientists' behavior, but as "vocabularies" for ideological descriptions of science (1976, 1979:71–72, 1980:101). Especially when scientists confront the public or its politicians, they endow science with characteristics selected for an ability to advance professional interests. Scientists have a number of "cultural repertoires" available for constructing ideological self-descriptions, among them Merton's norms, but also claims to the utility of science for advancing technology, winning wars, or deciding policy in an impartial way. Mulkey's contribution is largely programmatic: it remains to demonstrate empirically *how* scientists in public settings move flexibly among repertoires of self-description. In other words, how do scientists construct ideologies with style and content well suited to the advancement or protection of their professional authority?

SCIENCE, RELIGION AND MECHANICS IN VICTORIAN ENGLAND

Science is often perceived today as the sole occupant of a distinctive niche in the "intellectual ecosystem" (Boulding, 1980). Other knowledge-producing activities, such as religion, art, politics, and folklore, are seen as complements to science rather than competitors. But science has not always had its niche, nor are the boundaries of its present niche permanent. The intellectual ecosystem has with time been carved up into "separate" institutional and professional niches through continuing processes of boundary-work designed to achieve an apparent differentiation of goals, methods, capabilities and substantive expertise.

Boundary disputes still occur: the recent litigation over "creationism" suggests that for some Christian fundamentalists, religion and science continue to battle for the same intellectual turf. To the victor go the spoils: opportunities to teach one's beliefs about the origin of life to biology students in Arkansas public schools (Nelkin, 1982). Scientists have often

come up winners in the long history of such boundary disputes: "in modern societies, science is near to being *the* source of cognitive authority: anyone who would be widely believed and trusted as an interpreter of nature needs a license from the scientific community" (Barnes and Edge, 1982:2). This authority has been cashed in for copious material resources and power: about \$1 billion of tax revenue was provided last year to support basic scientific research in American universities; "expert" scientists are called before courts and government hearing rooms to provide putatively truthful and reliable contexts for decision making; science education is an integral part of modern curricula, opening employment opportunities for scientists at almost every school and university. Scientists often win these professional advantages in boundary disputes that result in the loss of authority and resources by competing non-scientific intellectual activities.

Public addresses and popular writings by John Tyndall (1820–1893) are a rich source of information on how this boundary-work was accomplished in Victorian England (for biographical details, cf. Eve and Creasey, 1945; MacLeod, 1976a; Burchfield, 1981). Tyndall followed Michael Faraday as Professor and then Superintendent at the Royal Institution in London, where he was charged with delivering lectures demonstrating to lay and scientific audiences the progress of scientific knowledge.

At that time, career opportunities and research facilities available to British men of science were paltry (MacLeod, 1972; Turner, 1976; Cardwell, 1972). Thomas Henry Huxley, Tyndall's friend and Darwin's "bulldog," complained in 1874 that "no amount of proficiency in the biological sciences will 'surely be convertible into bread and cheese'" (Mendelsohn, 1964:32). Tyndall used his visible position at the Royal Institution to promote a variety of ideological arguments to justify scientists' requests for greater public support. He faced two impediments: the intellectual authority of Victorian religion and the practical accomplishments of Victorian engineering and mechanics. Tyndall's campaign for science took the rhetorical style of boundary-work: he attributed selected characteristics to science that effectively demarcated it from religion or mechanics, providing a rationale for the superiority of scientists in designated intellectual and technical domains.

Scientists' Struggle for Authority

The endless conflict between religion and science reached a crescendo in the decade following publication of Darwin's *The Origin of Species* in 1859. Turner (1978:357) describes

this as a "professional" conflict for "authority and prestige," rather than strictly an academic debate between two "theories" of natural history (cf. Turner, 1974a). The intellectual authority of long-standing religious beliefs, reinforced every Sunday from the pulpit, created resistance toward scientific explanations of natural phenomena. For example, Tyndall found himself embroiled in the "prayer gauge" debate, which was sparked by an 1872 article challenging Christians of the nation to conduct an experiment to determine the physical efficacy of prayer. It was then the custom for the British Prime Minister or Privy Council to ask a high official of the Anglican church to call for a national day of prayer as a response to national crises. Public prayers were called as hoped-for solutions to cattle plagues in 1865, a cholera epidemic in 1866, and a case of typhoid suffered by the young Prince (Edward) of Wales in 1871.

To Tyndall, public prayers "represented a concrete form of superstition whereby clergy with the approval of the state could hinder the dispersion of scientific explanations of natural phenomena or claim credit for the eradication of natural problems that were solved by the methods of science . . ." (Turner, 1974b:48). (When the young Prince recovered from typhoid, clergymen pointed to the effectiveness of the country's prayers.) Tyndall encouraged an experiment in which a selected hospital would be made the focus of national prayer, with a comparison of mortality rates before and after the day of supplication. The experiment was never conducted, but the furious debate provoked by its proposal gives a sense of how much "the scientific professions desired the social and cultural prestige and recognition that had been and to a large degree still was accorded the clergy" (Turner, 1974b:64).

The Church also held power over educational institutions and used it to stall introduction of science into the curriculum. During Tyndall's tenure as President of the British Association for the Advancement of Science in 1874, the Catholic Church in his native Ireland rejected a request from laymen to include the physical sciences in the curriculum of the Catholic university. Perhaps as a response to this, Tyndall's presidential address at Belfast was an unequivocal denial of the authority of religious beliefs over natural phenomena, and he made "so bold a claim for the intellectual imperialism of the modern scientific inquiry" (Turner, 1981:172) that churchmen and some scientists were outraged.

Victorian mechanicians and engineers presented a different obstacle to the expansion of scientific authority and resources. Practical in-

ventions of Victorian craftsmen—steam engines, telegraphs—did almost as much to stall the entry of science into universities as the stonewall tactics of the Church. Many Britons believed that technical progress in the Industrial Revolution was not dependent on scientific research, and some, like William Sewell, believed that science impeded the flowering of practical technology: “deep thinking [is] quite out of place in a world of railroads and steamboats, printing presses and spinning-jennies” (in Houghton, 1957:114). Many would have agreed with Victorian writer Samuel Smiles, who wrote in 1874: “One of the most remarkable things about engineering in England is, that its principle achievements have been accomplished, not by natural philosophers nor by mathematicians, but by men of humble station, for the most part self-educated . . . The great mechanics . . . gathered their practical knowledge in the workshop, or acquired it in manual labor” (in Robinson and Musson, 1969:1). If technological progress was detached from scientific research, then the need for greater financial support of scientists and enlarged scientific education would go unappreciated by the British public and its politicians.

Moreover, as engineers began to “professionalize” by claiming expertise over certain technical issues, they sometimes confronted scientists who tried to assert their own technical authority. From 1866 until his 1882 resignation-in-protest, Tyndall served as “scientific” adviser to the Board of Trade on the question of how best to illuminate Britain’s lighthouses. Although the operation of lighthouses had traditionally been an engineering matter, Tyndall argued that the engineers who advised the Board “had closed their minds to external innovation” and expressed “diffidence toward the encouragement of new scientific ideas” (MacLeod, 1969:31, 15). Tyndall believed that informed policy required more fundamental research, while engineers were apparently content to reach decisions with extant knowledge. In the end, Tyndall’s recommendations were ignored in favor of the engineers’, who “were already in positions of high civil authority . . . Practical men who had braved the brute force of nature to fashion pillars of stone and mortar had a strong emotional case against speculative men of ideas” (MacLeod, 1969:15).

Science as Not-Religion

Because religion and mechanics thwarted (in different ways) Tyndall’s effort to expand the authority and resources of scientists, he often chose them as “contrast-cases” when constructing ideologies of science for the public. In

drawing the boundary between science and religion, Tyndall emphasized the following distinguishing features:

(1) Science is practically useful in inspiring technological progress to improve the material conditions of the nation; religion is “useful,” if at all, for aid and comfort in emotional matters. In an 1866 discourse on radiant heat Tyndall says, “that the knowledge brought to us by those prophets, priests and kings of science is what the world calls ‘useful knowledge,’ the triumphant application of their discoveries proves” (Tyndall, 1905a:102, cf. 365). The contributions of religion lie elsewhere: religious thought is “capable of adding, in the region of *poetry and emotion*, inward completeness and dignity to man” (Tyndall, 1905b:209).

(2) Science is empirical in that its road to truth is experimentation with observable facts of nature; religion is metaphysical because its truths depend on spiritual, unseen forces assumed without verification. In the midst of the Prayer Gauge controversy, Tyndall observed that in science, “to check the theory we have simply to compare the deductions from it with the facts of observation . . . But while science cheerfully submits to this ordeal, it seems impossible to devise a mode of verification of their theories which does not rouse resentment in theological minds. Is it that, while the pleasure of the scientific man culminates in the demonstrated harmony between theory and fact, the highest pleasure of the religious man has been already tasted in the very act of praying, prior to verification, any further effort in this direction being a mere disturbance of his peace?” (Tyndall, 1905b:47–48).

(3) Science is skeptical because it respects no authority other than the facts of nature; religion is dogmatic because it continues to respect the authority of worn-out ideas and their creators. “The first condition of success [in science] is patient industry, an honest receptivity, and a willingness to abandon all preconceived notions, however cherished, if they be found to contradict the truth” (Tyndall, 1905a:307). The dogmatism imputed to theologians is a main theme in Tyndall’s diatribe against observation of the Sabbath: “the most fatal error that could be committed by the leaders of religious thought is the attempt to force into their own age conceptions which have lived their life, and come to their natural end in preceding ages . . . Foolishness is far too weak a word to apply to any attempt to force upon a scientific age the edicts of a Jewish lawgiver” (Tyndall, 1898:33, 36).

(4) Science is objective knowledge free from emotions, private interests, bias or prejudice; religion is subjective and emotional. Tyndall observes that the book of Genesis should be

read as "a poem, not [as] a scientific treatise. In the former aspect, it is forever beautiful; in the later aspect it has been, and it will continue to be, purely obstructive and hurtful. To *knowledge* its value has been negative . . ." (Tyndall, 1905b:224). While considering the topic of miracles and special providences, Tyndall (in 1867) writes: "to kindle the fire of religion in the soul, let the affections by all means be invoked . . . [But] testimony as to natural facts is worthless when wrapped in this atmosphere of the affections; the most earnest subjective truth being thus rendered perfectly compatible with the most astounding objective error" (Tyndall, 1905b:19–20). A military metaphor suggests that this boundary-work for Tyndall was more than philosophical speculation: "It is against the objective rendering of the emotions—this thrusting into the region of fact and positive knowledge of conceptions essentially ideal and poetic—that science . . . wages war" (Tyndall, 1905b:393).

Science as Not-Mechanics

When Tyndall turns to build a boundary between science and mechanics, he attributes to science a different set of characteristics in response to the different kind of obstacle presented by the technical achievements and authority of engineers and industrial craftsmen. Significantly, characteristics here attributed to science are not always consistent with those attributed to science when Tyndall demarcated it from religion.

(1) Scientific inquiry is the fount of knowledge on which the technological progress of inventors and engineers depends. "Before your practical men appeared upon the scene, the force had been discovered, its laws investigated and made sure, the most complete mastery of its phenomena had been attained—nay, its applicability to telegraphic purposes demonstrated—by men whose sole reward for their labours was the noble excitement of research, and the joy attendant on the discovery of natural truth" (Tyndall, 1901:221–22). "The professed utilitarian . . . admires the flower, but is ignorant of the conditions of its growth . . . Let the self-styled practical man look to those from the fecundity of whose thought he, and thousands like him, have sprung into existence. Were they inspired in their first inquiries by the calculations of utility? Not one of them" (Tyndall, 1905a:312).

(2) Scientists acquire knowledge through systematic *experimentation* with nature; because mechanics and engineers rely on mere observation, trial-and-error, and common sense, they cannot explain their practical successes or failures. Tyndall makes this distinc-

tion in an 1876 discourse in Glasgow on the science of fermentation and the mechanical art of brewing beer: "it might be said that until the present year no thorough and scientific account was ever given of the agencies which come into play in the manufacture of beer . . . Hitherto the art and practice of the brewer have resembled those of the physician, both being founded on empirical observation. By this is meant the observation of facts, apart from the principles which explain them, and which give the mind an intelligent mastery over them. The brewer learned from long experience the conditions, not the reasons, of success . . . Over and over again his care has been rendered nugatory; his beer has fallen into acidity or rottenness, and disastrous losses have been sustained, of which he has been unable to assign the cause" (Tyndall, 1905b:267).

(3) Science is theoretical. Mechanics are not scientists because they do not go beyond observed facts to discover the *causal principles* that govern underlying *unseen processes*. "Our science would not be worthy of its name and fame if it halted at facts, however practically useful, and neglected the laws which accompany and rule the phenomena" (Tyndall, 1905a:95–96). "One of the most important functions of physical science . . . is to enable us by means of the sensible processes of Nature to apprehend the insensible" (Tyndall, 1905a:80). Tyndall's choice of words in the next two passages seems odd for one who elsewhere speaks the language of naive empiricism: "the visible world [is] converted by science into the symbol of an invisible one. We can have no explanation of the objects of experience, without invoking the aid and ministry of objects which lie beyond the pale of experience" (Tyndall, 1883:33). "The theory is the backward guess from fact to principle; the conjecture, or divination regarding something, which lies behind the facts, and from which they flow in necessary sequence" (Tyndall, 1894:141–42).

(4) Scientists seek discovery of facts as ends in themselves; mechanics seek inventions to further personal profit. On the electric light, Tyndall notes: "Two orders of minds have been implicated in the development of this subject: first, the investigator and discoverer, whose object is purely scientific, and who cares little for practical ends; secondly, the practical mechanic, whose object is mainly industrial . . . The one wants to gain knowledge, while the other wishes to make money . . ." (Tyndall, 1905b:472–73). The lust for profit among mechanics is said to impede technological progress: "The slowness with which improvements make their way among

workmen . . . is also due to the greed for wealth, the desire for monopoly, the spirit of secret intrigue exhibited among manufactures" (Tyndall, 1898:136). These attitudes are not common to scientists: "The edifice of science had been raised by men who had unswervingly followed the truth as it is in nature; and in doing so had often sacrificed interests which are usually potent in this world" (Tyndall, 1905b:403).

(5) Science need not justify its work by pointing to its technological applications, for science has nobler uses as a means of intellectual discipline and as the epitome of human culture. Tyndall asks: "But is it necessary that the student of science should have his labours tested by their possible practical applications? What is the practical value of Homer's Iliad? You smile, and possibly think that Homer's Iliad is good as a means of culture. There's the rub. The people who demand of science practical uses forget, or do not know, that it also is great as a means of culture—that the knowledge of this wonderful universe is a thing profitable in itself, and requiring no practical application to justify its pursuit" (Tyndall, 1905a:101). And to an American audience: "it is mainly because I believe it to be wholesome, not only as a source of knowledge but as a means of discipline, that I urge the claims of science upon your attention . . . Not as a servant of Mammon do I ask you to take science to your hearts, but as the strengthener and enlightener of the mind of man" (Tyndall, 1901:217, 245).

This last attribution seems odd. If utilitarian consequences of science are often mentioned to justify increased resources for scientific research, why does Tyndall also present an image of "pure" science to be appreciated as a means of high culture and intellectual discipline? For two reasons, Tyndall demarcated the merely practical mechanician from the more-than-practical scientist. First, if science was justified only in terms of potential industrial accomplishments, government officials could argue (as Gladstone—Prime Minister for much of this period—often did) that profits from scientifically inspired innovations would repay private industrialists who invested in scientific research. By emphasizing that science has cultural virtues beyond practical utility—virtues not likely to be appreciated and financially supported by profit-seeking industrialists—Tyndall presented an "alternative case" for government grants to scientists. Second, Mendelsohn (1964) has suggested that descriptions of science as industrially practical might not have persuaded Oxford and Cambridge Universities to enlarge their science curricula. As part of the education of Britain's

cultural and political elite, science was less attractive as a means to make money and more attractive as the discoverer of truth and as a source of intellectual discipline.

Tyndall's choice of religion and mechanics as contrast-cases was not an idle one: each was an impediment to public support, funding and educational opportunities essential for the growth of science in Victorian England. Tyndall demarcated science from these two obstacles, but the characteristics attributed to science were different for each boundary: scientific knowledge is empirical when contrasted with the metaphysical knowledge of religion, but theoretical when contrasted with the common-sense, hands-on observations of mechanicians; science is justified by its practical utility when compared to the merely poetic contributions of religion, but science is justified by its nobler uses as a means of "pure" culture and discipline when compared to engineering. Alternative repertoires were available for Tyndall's ideological self-descriptions of scientists: selection of one repertoire was apparently guided by its effectiveness in constructing a boundary that rationalized scientists' requests for enlarged authority and public support.

Still, Tyndall was not disingenuous in describing science in one context as "practically useful," and elsewhere as "pure culture." It would be reductionistic to explain these inconsistent parts of a professional ideology merely as fictions conjured up to serve scientists' interests. There is, in science, an unyielding tension between basic and applied research, and between the empirical and theoretical aspects of inquiry. Tyndall's "public science" exploits this genuine ambivalence by selecting for attribution to science one or another set of characteristics most effective in demarcating science from religion on some occasions, from mechanics on others.

This ideology, however inconsistent or incomplete, seems to have improved the fortunes of science in the decades immediately following Tyndall's death in 1893. Scientists "had established themselves firmly throughout the educational system and could pursue research and teaching free from ecclesiastical interference" (Turner, 1978:376), and by 1914 public money for civil scientific research reached 2 million pounds, or an unprecedented 3.6 percent of the total civil expenditure (MacLeod, 1976b:161, cf. 1982).

PHRENOLOGISTS AND ANATOMISTS IN EARLY 19TH-CENTURY EDINBURGH

Boundary-work is also a useful ideological style when monopolizing professional au-

thority and resources in the hands of *some* scientists by excluding others as "pseudoscientists" (cf. Mauskopf, 1979; Wallis, 1979; Collins and Pinch, 1982). The debate over phrenology illustrates how one group of scientists draws a boundary to exclude another also claiming to be scientific.

Phrenology began in the late 18th century with anatomist-and-physician Franz Joseph Gall, who argued three essential principles (cf. Cantor, 1975:197): the brain is the organ of the mind; the brain is made up of separate organs, each related to distinct mental faculties; the size of the organ is a measure of the power of its associated mental faculty. The faculties included sentiments such as combativeness, self-esteem, benevolence, and veneration, and intellectual faculties such as imitation, order, time, number, tune, and wit. An individual with a large organ for "amativeness" was expected to have a large appetite for "feelings of physical love." Phrenologists claimed to be able to judge a person's mental character by examining the pattern of bumps on the outside of the skull: a protuberance in the forehead indicated intellectual prowess because this was the region for organs of reflection. The journey of phrenology from serious science to sideshow legerdemain is a consequence of boundary-work by phrenologists and their scientific adversaries, a debate which peaked in Edinburgh in the early 1800s.

The Scottish controversy was fueled by an 1803 article in the *Edinburgh Review* which described phrenology as "a mixture of gross errors, extravagant absurdities," "real ignorance, real hypocrisy," "trash, despicable trumpery" propagated by "two men calling themselves scientific inquirers" (in Davies, 1955:9-10). This opinion was shared by Edinburgh's intellectual elite, including anatomists at the City's prestigious medical school. However, prominent Edinburgh phrenologists—Johann Spurzheim (a Gall student) and his most vociferous recruit George Combe—enjoyed popular reputations as *legitimate scientists* at least until 1820. Anatomists offered public descriptions of science that effectively pushed Combe and phrenology outside its boundaries. Combe in turn offered a competing description of science, making it appear that he was unjustly banished and that he had as much claim to the mantle of science as anatomists.

Alternative Images of Science

The repertoires differed on three issues: (1) Anatomists tried to discredit the scientific legitimacy of phrenology by exposing its political and especially religious ambitions, which were said to corrupt phrenologists' ability to

objectively evaluate knowledge claims (cf. Shapin, 1979:140). Alternatively, Combe presented an image of science as essentially limitless: phrenological science could provide a sound foundation for deciding religious or political questions. Early 19th-century scientists desired a peaceful coexistence with the Church, to be accomplished by a careful demarcation of scientific from religious questions (cf. DeGiustino, 1975:50, 104; Cannon, 1978:2). Edinburgh anatomists perhaps felt threatened by presumptions that science provided the *one* truth: Combe claimed that "phrenology held the key to all knowledge and provided the philosophical basis for a true approach to Christianity" (Cantor, 1975:204). When phrenologists offered a "scientific" theory that religiosity was a function of the size of one's organ for "veneration," the domain of religion had obviously been encroached upon (Cooter, 1976:216). Anatomists implied that because Combe placed a quasi-religious mission ahead of the dispassionate search for knowledge about natural phenomena, he was no longer within science. Perhaps they also convinced powerful Scottish churchmen that intrusion of phrenology into religion was not the work of *bona fide* scientists.

(2) For Combe, phrenology relied on empirical methods like any other science: "Experience alone can decide concerning the accuracy or inaccuracy of our observation and induction" (in Cantor, 1975:211). Critics argued, however, that theories of phrenology were so vague as to remove them from "adequate" empirical testing. Francis Jeffrey, adversary of Combe, could find no logical reason why there was no organ for "love of horses" to accompany one proposed to explain "love of children," and concluded that phrenology "abounds in those equivocations, by which it may often escape from direct refutation . . . [It was] a series of mere evasions and gratuitous assumptions" (in Cantor, 1975:213; cf. Young, 1970:43). William Hamilton, a philosopher, conducted experiments apparently contradicting Combe's hypothesis that the cerebellum controlled sexual activity and that it was *larger* in men than women. Hamilton found the opposite but Combe did not retreat, instead defending phrenology as an "estimative," not an "exact" science. Hamilton's calibrations were irrelevant for Combe because phrenology "concerned approximate determination of quantities, in particular, the size of the cranial contours as gauged by *the feel of the phrenologist* . . ." (in Cantor, 1975:214-15). This subjectivism was enough for Hamilton to dismiss phrenology as pseudo-science: "so long as phrenology is a comparison of two hypothetical quantities—a science of propor-

tion without a determinate standard and an acknowledged scale— . . . I deem it idle to dispute about the application of a law which defines no phenomena, and the truth of a hypothesis which has no legitimate constitution" (in Cantor, 1975:215).

(3) Anatomists accused phrenologists of relying on popular opinion to validate their theories while ignoring opinions of scientific "experts." Hamilton asked Combe to "produce a single *practical anatomist* who will consent to stake his reputation" on the truth of phrenology (in Cantor, 1975:216). Combe replied that "experts" could not serve as dispassionate judges of phrenology because most had previously expressed their contempt for it. Combe advocated scientific populism, telling his audiences in 1818: "Observe nature for yourselves and prove by your own repeated observations the truth or falsehood of phrenology" (in Shapin, 1975:236). Hamilton countered: "no useful purpose would be served by submitting the points at issue to an ignorant and non-vocal public who could not clearly see the finer points under discussion" (Cantor, 1975:216). Both sides claimed that their position was "more scientific." Combe placed himself with Galileo, Harvey, and Newton, whose truths were at first denied by established "scientific" experts. Anatomists argued that only those with sufficient training and skills could evaluate technical claims about the structure and function of the brain.

Why did anatomists exclude phrenologists from science? First, phrenology challenged orthodox theories and methods, and anatomists may have suffered losses to professional reputations and opportunities had Combe been successful in his claim to science (Shapin, 1979:169). Traditional divisions of labor within the university (anatomists studied the structure of the body, moral philosophers studied its mental and behavioral functioning) were threatened by phrenologists' claim that "theirs was the only complete science of man" (Cooter, 1976:214). Second, Combe's democratic ideal of certifying truth by popular opinion challenged the authority of scientific experts. Third, as we have seen, phrenologists' desire to meld science and Christianity could have inspired a religious backlash against other scientists, at a time when religion may have had greater hold on public sympathy than science. On the other side, Combe sought scientific legitimacy in part to advance his phrenologically inspired social and political reforms (cf. Shapin, 1975:233). He successfully lobbied for rehabilitative programs in prisons (cf. Parssinen, 1974:6) on grounds that prisoners must be prepared for occupations suited to their innate capacities (which were to be

ascertained by a scientific feel of bumps on their heads).

But anatomists were successful in putting the boundary between their science and phrenology: Combe was denied the chair of Logic at Edinburgh University; phrenologists were not allowed to use lecture halls at the Edinburgh School of Arts; phrenological issues were rarely admitted to the proper forum for scientific debate, the Royal Society of Edinburgh; Combe was not allowed to form a "phrenological section" in the British Association for the Advancement of Science (Parssinen, 1974:9; Shapin, 1975:229ff). Selected phrenological ideas from Gall were incorporated into the legitimate science of physiological psychology (cf. Boring, 1957:13; Smith, 1973:86–87) without admitting Combe to the scientific community, thus avoiding threats to professional authority and resources of Edinburgh anatomists. Combe's ideology of science (as expandable into religious questions, as estimative or subjective in methodology, and as capable of being evaluated by non-specialists) instead served as a vehicle for his exclusion from science as alternately defined by anatomists. The boundary dispute between anatomists and phrenologists was a contest for the authority to call oneself a scientist and to claim scientific legitimacy for one's beliefs. Phrenology lost: "science" assumed boundaries that left no room for it within.

"NATIONAL SECURITY" AND THE AUTONOMY OF MODERN SCIENCE

Once scientists accumulate abundant intellectual authority and convert it to public-supported research programs, a different problem faces the profession: how to retain control over the use of these material resources by keeping science autonomous from controls by government or industry. Public and political pleas for regulation of science often result from dissatisfaction with its *practical* accomplishments: either scientists fail to provide the technological fix that the public desires, or they produce technological capabilities that the public fears or loathes. Boundary-work is an effective ideological style for protecting professional autonomy: public scientists construct a boundary *between* the production of scientific knowledge and its consumption by non-scientists (engineers, technicians, people in business and government). The goal is immunity from blame for undesirable consequences of *non-scientists'* consumption of scientific knowledge.

An illustration comes from a September 1982 report entitled *Scientific Communication and National Security*, produced by the Committee

on Science, Engineering and Public Policy of the National Academy of Sciences (NAS, 1982). Some U.S. government officials now worry that rapid increases in Soviet military strength are due, in part, to their exploitation of American science and technology. Members of the Reagan Administration have responded by proposing and, at times, implementing stricter controls on the open circulation of scientific and technical knowledge.¹ The restrictions elicited outrage from the scientific community, captured in the title of a *Science* editorial: "Hand-Cuffing Science" (cf. Culliton, 1983).

In response to efforts to expand government control over the circulation of scientific *knowledge*, an NAS Panel on Scientific Communication and National Security was created to examine the question "What is the effect on national security of technology transfer to adversary nations by means of open scientific communication, either through scientific literature or by person-to-person communications?" (NAS, 1982:91). The Panel was made up of representatives of organized science, industry, and government. Whether its recommendations are in the best interests of national security is a matter for the public and its legislators to debate. However, the professional interests of science seem well served, for the Report recommends, in effect, that the overwhelming majority of scientific communications should remain free from government restraints, and that national security will be more effectively attained not through controls on science but through preserved autonomy and enlarged resources to enable American science and technology to retain its international preeminence.

To justify these recommendations, the Panel presents four arguments:

(1) The Report isolates a "core" of science by demarcating the production of scientific knowledge from its consumption. Selected characteristics are attributed to science in order to distinguish it from technological applications: scientific work is housed mainly in universities, not in industrial firms or governmental agencies; the goal of science is the creation, dissemination and evaluation of

knowledge as its own end, not as a means for material production; open scientific communication transmits theoretical and empirical knowledge about nature, not "know-how" or "recipes" immediately transferable to production of hardware (NAS, 1982:45, 62).

(2) This core of university-housed, "basic" scientific research is not a significant source of "technology transfer" benefiting Soviet military strength, and thus "no restrictions of any kind limiting access or communication should be applied to any area of university research . . ." (49). "While there has been extensive transfer of U.S. technology of direct military relevance to the Soviet Union from a variety of sources, there is strong consensus that scientific communication, including that involving the university community, appears to have been a very small part of this transfer . . ." (13-14). The source of the problem lies elsewhere: "legal equipment purchases, outright espionage, illegal conduct by some individuals and corporations in international trade, and secondary transfers through legal or illegal recipients abroad to the hands of U.S. adversaries" (41).

(3) Government controls on open scientific communication would have deleterious side effects. First, scientists would be deterred from choosing to do research in militarily "sensitive" areas, thus hampering American efforts to produce its own innovative military hardware (45). Second, if controls limited international exchanges between American and Soviet scientists, then progress of American science might be impeded in those research areas where the Soviets are especially strong, for example, plasma physics, condensed-matter physics and fundamental properties of matter (25). Third, the progress of American science in general would suffer: "Free communication among scientists is viewed as an essential factor in scientific advance. Such communication enables critical new findings or new theories to be readily and systematically subjected to the scrutiny of others and thereby verified or debunked" (24). Fourth, constraints on scientific communication would slow the rate of *technological* innovation, both military and civilian: "The technological leadership of the United States is based in no small part on a scientific foundation whose vitality in turn depends on effective communication among scientists and between scientists and engineers" (43).

(4) American military supremacy, in an age of high-tech weaponry, is better achieved not by controls on scientific communication, but by providing enlarged resources and improved facilities to scientists. "Current proponents of stricter controls advocate a strategy of security

¹ The Department of Defense recently blocked presentation of about 150 of the 626 papers to be read at the 26th annual meeting of the Society of Photo-Optical Engineers in San Diego (August 1982). They acted on grounds that certain papers (federally supported but "unclassified") on optical technologies used in laser communication had potential military applications, and that the meetings were attended by scientists from the Soviet Union and Eastern Europe (NAS, 1982:12, note 1).

through secrecy. In the view of the Panel, *security by accomplishment* may have more to offer as a general national strategy. The long-term security of the United States depends in large part on its economic, technical, scientific, and intellectual vitality, which in turn depends on the vigorous research and development effort that openness helps to nurture" (45). The Panel does not miss an opportunity to hint at the inadequacy of Government support of science: "Federal funding at universities, measured in constant dollars, leveled off about 15 years ago, and thus recent growth in the system has been slight, making it more difficult to replace obsolete equipment and to undertake new, and more expensive, enterprises . . ." (23).

The boundary-work here is subtle and complex: on one hand, the Panel asserts that university-based science yields "basic" rather than "applied" knowledge; on the other, they assert that university-based science is essential for technological progress. The two assertions are not necessarily contradictory: "basic" knowledge can be transformed into "applied" knowledge and, with time, yield military and industrial products. The sociologically interesting point is this: a boundary between basic and applied science is clearly established when the Panel wants to cordon "science" (i.e., basic research at universities) from government controls on communication; but the boundary is obscured, if not dissolved, when the Panel wishes to remind legislators that *even* basic science makes important contributions to technological progress. The Panel notes: "in many fields, at the cutting edge of science, the distinction between basic and applied research was becoming less relevant" (101–102). But elsewhere, it is *relevant and possible* for the Panel to distinguish basic research from its technological potential, and to argue that the Soviets acquire militarily useful information from *non-scientific* applications of scientific knowledge.

Since Tyndall, the ideology of "the practical benefits of pure science" has been used to justify public support for scientific research. With the Reagan Administration proposing cutbacks in the budget of the U.S. National Science Foundation, it may be politically expedient to emphasize once again the utilitarian justification of science. But in the context of "national security" it may not help to play that song too loudly, for to avoid government restrictions on scientific communication, some distance between basic and applied science must be established. Thus, the boundary between the production and consumption of scientific knowledge remains ambiguous in the Report, but usefully so for scientists' pursuit of two distinct

professional goals: autonomy and public support.

The persuasiveness of this Report hinges on the effectiveness of its boundary-work. If the Panel succeeds in demarcating the university-based production of "basic" scientific knowledge from its technological consumption and application, then legislators may accept its conclusion and follow its recommendations. Because the responsibility and blame for leaks of militarily useful technology to the Soviet Union is not to be placed on science but on individuals or corporations outside the community of American university-based scientists, the case for increased government controls on scientific communication is less compelling. The continued autonomy of scientists may depend on the effectiveness of this ideology.²

CONCLUSION: THE AMBIGUOUS BOUNDARIES OF "SCIENCE"

At first glance, Tyndall's exhortations for public support of science seem remote from the Edinburgh phrenology debates or from the military exploitation of scientific knowledge, at least until the concept of "boundary-work" is introduced. The three examples of ideologies of science have a common rhetorical *style*: attributions of selected characteristics to the institution of science for purposes of constructing a social boundary that distinguishes "non-scientific" intellectual or professional activities. Geertz's suggestion to examine the "stylistic resources" of ideologists has proved fruitful: "boundary-work" is a sociological parallel to the familiar literary device of the "foil." Just as readers come to know Holmes better through contrasts to his foil Watson, so does the public better learn about "science" through contrasts to "non-science."

Moreover, the analysis begins to identify occasions where boundary-work is a likely stylistic resource for ideologists of a profession or occupation: (a) when the goal is *expansion* of authority or expertise into domains claimed by other professions or occupations, boundary-work heightens the contrast between

² More recent political developments must worry the scientific community: *Science* (4 February 1983:473) reports that the Reagan Administration has "launched a high-level review of ways to control the publication of scientific papers that contain certain unclassified but militarily sensitive information . . . The review will be more concerned with how, rather than whether, publication of such information should be controlled." Boundary-work is not always successful, though this case is far from decided (cf. Chalk, 1983).

rivals in ways flattering to the ideologists' side; (b) when the goal is *monopolization* of professional authority and resources, boundary-work excludes rivals from within by defining them as outsiders with labels such as "pseudo," "deviant," or "amateur"; (c) when the goal is *protection of autonomy* over professional activities, boundary-work exempts members from responsibility for consequences of their work by putting the blame on scapegoats from outside. Because expansion, monopolization and protection of autonomy are generic features of "professionalization," it is not surprising to find the boundary-work style in ideologies of artists and craftsmen (Becker, 1978) and physicians (Freidson, 1970; Starr, 1982). The utility of boundary-work is not limited to demarcations of science from non-science. The same rhetorical style is no doubt useful for ideological demarcations of disciplines, specialties or theoretical orientations *within* science. Kohler's recent study of biochemistry notes: "Disciplines are political institutions that demarcate areas of academic territory, allocate the privileges and responsibilities of expertise, and structure claims on resources" (1982:1).

Analysis of the *content* of these ideologies suggests that "science" is no single thing: characteristics attributed to science vary widely depending upon the specific intellectual or professional activity designated as "non-science," and upon particular goals of the boundary-work. The boundaries of science are ambiguous, flexible, historically changing, contextually variable, internally inconsistent, and sometimes disputed. These ambiguities have several structural sources. First, characteristics attributed to science are sometimes inconsistent with each other because of scientists' need to erect separate boundaries in response to challenges from *different* obstacles to their pursuit of authority and resources. For Tyndall, the empirical and useful *fact* was the keystone of science as not-religion, but the abstract and pure *theory* was the keystone of science as not-mechanics. Second, the boundaries are sometimes contested by scientists with different professional ambitions. Edinburgh anatomists protected their claim to expertise and authority by arguing that only specialists could evaluate claims to scientific knowledge; Combe argued that scientific claims were open to confirmation by anybody, an attempt to sell phrenology as "science" and thus to surround his quasi-religious and political reforms with "scientific" legitimacy. Third, ambiguity results from the simultaneous pursuit of separate professional goals, each requiring a boundary to be built in different ways. For the NAS Panel on scientific communication and national security, technologi-

cal fruits are placed "inside" science when the goal is justification of public support for science, but they are excluded when the goal is protection of the autonomy of scientists from government regulation.

Both "strains" and "interests" help to explain the ambiguous content of scientists' ideologies. Merton ([1963] 1976:33) argues that science, like any social institution, is "patterned in terms of potentially conflicting pairs of norms" (cf. Mitroff, 1974). Scientists cannot avoid ambivalence: for example, they should be "original" (by striving to be first to announce a significant discovery) but "humble" (by not fighting for one's priority if the discovery is announced by multiple investigators). These juxtapositions of norm and counter-norm do more than create "inner conflict among scientists who have internalized both of them" (Merton, [1963] 1976:36): they also provide ideologists with *alternative* repertoires for public descriptions of science. Internal inconsistencies in what scientists are expected to provide diverse ideological resources for use in boundary-work. The three examples illustrate several antinomies in the institution of science: scientific knowledge is at once theoretical and empirical, pure and applied, objective and subjective, exact and estimative, democratic (open for all to confirm) and elitist (experts alone confirm), limitless and limited (to certain domains of knowledge).

If "strains" *enable* alternative repertoires, "interests" *guide the selection* of one or another repertoire for public presentation. Ideologists are able to endow science with just those characteristics needed to achieve professional and institutional goals, and to change these attributed characteristics as circumstances warrant. Still, no one can accuse Tyndall, Edinburgh anatomists, or the NAS Panel of "bad faith": science *is* both pure and applied, theoretical and empirical. To reduce ideologies of science to illusions concocted only to serve professional interests assumes an unrealistically gullible public and a cynical and *merely* instrumentalist scientific community. But to reduce the ideologies to reflections or resolutions of strains forgets that scientists too struggle for authority, power, and resources. Neither strains nor interests are themselves sufficient to explain the successful ideologies of science.

This paper offers one escape from seemingly interminable debates over the uniqueness and superiority of science among knowledge-producing activities. Demarcation is as much a practical problem for scientists as an analytical problem for sociologists and philosophers. Descriptions of science as distinctively truthful, useful, objective or rational may best be ana-

lyzed as ideologies: incomplete and ambiguous images of science nevertheless useful for scientists' pursuit of authority and material resources.

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