LEGITIMATIONS OF SCIENCE IN A CHANGING WORLD

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In communication between science and the polity, when scientists present themselves in the public realm, it is often hard to distinguish between substantial argument and legitimatory tactics. For instance, it is clear that not all risks and abuses associated with science-based products and processes can be blamed on science. But is it then allowed to claim all the benefits as evidence of the benevolent nature of science ? I would say not, but scientists, almost to a man, take the position captured in an aphorism of Jerry Ravetz:

> Science takes the credit for penicillin, while Society takes the blame for the Bomb. [1]

A legitimatory tactic need not be based on the use of double standards. But many legitimations actually used by scientists towards politicians, towards the public or in internal debates, derive their persuasive function from such a dual structure.

A recurring legitimatory argument in debates on science policy is that of science as the goose that lays golden eggs. Already in 1862, Helmholtz offers his audience this argument in order to draw the moral that science should be supported but not controlled by the nation :

> The scientists -- for the benefit of the entire nation and almost always at its request and expense -- are seeking to multiply the knowledge which can serve the increase of industry, wealth, and the beauty of life, the improvement of the political organization and the moral development of the individual. Yet, not immediate utility must be looked for, as is so often done by the uninformed. Everything that informs us about the natural forces or the forces of the human spirit is valuable and in time may prove useful, normally in a place where one had least expected this. [2]

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1 Jerry Ravetz, '... et augebitur scientia', in: Rom Harrè (ed.), **Problems of Scientific Revolution** (London: Oxford University Press, 1975), 46.

2 Quoted after Wolfgang van den Daele, 'The Ambivalent Legitimacy of the Pursuit of Knowledge', in: Egbert Boeker and Michael Gibbons (eds.),**Proceedings of the Conference Science, Society and Education** (Amsterdam: VU Bookshop, 1978), 23-62, at p. 31-32.

In another country and century, another statesman of science, Vannevar Bush, uses the theme of the goose and the golden eggs to argue for the establishment of a National Science Foundation in the U.S.A. that will spend money on scientific research without its beneficiaries being directly accountable to the government:

> The scientists doing basic research may not be at all interested in the practical application of his work, yet the further progress of industrial development would eventually stagnate if basic scientific research were long neglected. (...) Statistically it is certain that important and highly useful discoveries will result from some fraction of the undertakings in basic science; but the results of anyone particular investigation cannot be predicted with accuracy. Basic research (..) provides scientific capital (...)
> Scientific progress on a broad front results from the free play of free intellects, working on subjects of their own choice, in the manner dictated by their curiosity for exploration of the unknown. Freedom of inquiry must be preserved (..).[3]

In the wake of World War II, which was fought by science as much as it was fought by the military, Vannevar Bush and others like him could be successful in arguing for patronage without external controls. The 1950s and 1960s became the heroic period of science policy. However, the historical success of the argument should not deceive us as to the quality of the argument. The contribution of basic research to national goals, and to practical ends in general, cannot be measured unambiguously,[4] and it is possible to hold that the eggs of the goose are golden only within the confines of its laboratory nest.[5] The dual character of the argument becomes clear when one realizes that no amount of retrospective evaluation of the contribution of basic research provides ground for extrapolation to the future, least of all if Helmholtz's and Bush's central point is

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3 Vannevar Bush, **Science, The Endless Frontier** (1945, reprinted Washington, D.C.: National Science Foundation, 1960), p. 18-19, 12.

4 Compare the opposite conclusions reached by the American studies Hindsight and TRACES, as reviewed in I.S. Spiegel-Rösing, **Wissenschaftsentwicklung und Wissenschaftssteuerung** (Frankfurt/M: Athenäum, 1973), 116-121.

5 The power of science lies to a large extent in its capacity to create controlled circumstances in laboratories. To apply science, outside reality has to be first transformed so as to resemble the original experimental conditions. See Arie Rip, 'The Development of Restrictedness in the Sciences', in: Norbert Elias, Herminio Martins and Richard Whitley (eds.), **Scientific Establishments and Hierarchies** (Dordrecht: Reidel, 1982), 219-238.

included, that the geese of science should be left free to roam as they please.

Historians of science have traced the emergence of the argument of the roaming goose as a reaction to the gradual incorporation of science in society during the 19th centruy. Circumstances have differed in different countries, but George Daniels' description of the "professional schizophrenia" of 19th-century American scientists is applicable to all Western industrializing countries:

> Utility is not to be a test of scientific work, but all knowledge will ultimately prove useful.[6]

What is important for our purpose is that since the late 19th century, a legitimation of science as the goose that will continue to lay golden eggs when it is allowed to roam, is available to scientists (and others) as a repertoire. This legitimation is now part of the culture of science and can be used as self-evident, at least by the scientists themselves. No proof is needed, only rhetorical flourishes to capture the audience.[7]

Other legitimations of science, not related to its practical utility, are possible and have been used: science as the paragon of rationality and the ally of emancipatory social movements; or science as a neutral inoffensive way to study the "Book of Nature".[8] Again, they are often linked with claims for the social and cultural autonomy of science. Galilei, the most prominent culture hero of the scientific tribe, is invoked whenever external controls threaten the "freedom" of science, and his example serves to rhetorically argue that any attempt at external direction of science will throw us back to the irrationa-

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6 George H. Daniels, 'The Pure-Science Ideal and Democratic Culture', **Science 156** (1967) 1699-1705.

7 A striking example is provided by molecular biologists in the debate about recombinant-DNA research. In public statements, e.g. a Congressional hearing, the eventual utility is the most frequently used argument. The same scientists, when interviewed for the MIT Oral History project, were rather sceptical about utility, and emphasized the issue of freedom of research. These data derive from an unpublished paper of Michael Altimore, 'The Rhetoric of Scientific Controversy: Recombinant DNA' (Philadelphia, PA: 4S Annual Meeting, 1982); see also id., 'The Social Construction of a Scientific Controversy: Comments on Press Coverage of the Recombinant DNA Debate', **Science, Technology and Human Values** (Fall 1982) 24-31.

8 See Van den Daele, op. cit. note 2, and Arie Rip, 'De gans met de gouden eieren en andere maatschappelijke legitimaties van de moderne wetenschap', **De Gids 145**(5)(1982) 285-297.

lity of a Dark Age.[9]

Legitimatory repertoires in the culture of science may also have an internal role. The well-known Mertonian norms, canonized by the acronym CUDOS, which stands for commun(al)ism, universalism, disinterestedness and organized scepticism, were mostly drawn from official pronouncements of scientists upon the worth of science. Merton himself was motivated to present these norms by a wish to defend science and democratic societies against totalitarian threats.[10] This particular legitimatory repertoire has been taken, by a whole generation of sociologists of science, to represent the norms guiding the actual behaviour of scientists. More recently, the whole notion of "normative" sociology of science is being criticized, and the CUDOS norms are seen as a professional ideology.[11] But even professional ideologies have an impact internally: as a constraint on the actions of scientists, and as a repertoire of arguments in the competitive struggles with colleagues.[12]

The function of legitimatory repertoires

Legitimatory repertoires are important in political negotiations with political establishments. Characteristically, the statesmen of science feel they have to defend the image of science, as it is drawn in these repertoires, against "muckraking" by outsiders or by defecting scientists.[13] Whether this image of science is correct or not, it has to be kept intact to enable its usage in laying claim to social

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9 Galilei's symbolic role is complemented by Lysenko, the culture devil, who represents the evil that results from political constraints on science. The one or the other can be invoked, depending on circumstances. Galilei can, however, be used whenever any constraint is felt, including unfavourable treatment by editors of scientific journals. See for example Ungar's comment on his referee's report: G. Ungar e.a., **Nature 238** (1972) 209-210, and the preceding papers.

10 See Robert K. Merton, 'Science and the Social Order' (1938) and 'Science and Technology in a Democratic Order' (1942), reprinted in: Norman W. Storer (ed.), **Robert K. Merton. The Sociology of Science. Theoretical and Empirical Investigations** (Chicago: University of Chicago Press, 1973), 254-266 and 267-278.

11 S.B. Barnes and R.G.A. Dolby, 'The Scientific Ethos: A Deviant Viewpoint', **European J. Sociology** 11 (1970) 3-25; J. Law and D. French, 'Normative and Interpretive Sociologies of Science', **Sociol. Rev. 22** (1974) 581-595; Michael Mulkay, 'Norms and Ideology in Science', **Social Science Information 15** (1976) 637-656.

12 Arie Rip, **Maatschappelijke verantwoordelijkheid van chemici** (Doctoral Dissertation, University of Leiden, 1981), 148. A fascinating and detailed study of CUDOS norms as tactical arguments is provided by R.V. Kemp, 'Controversy in Scientific Research and Tactics of Communication', **Sociol. Rev. 25** (1977) 515-534.

13 Alvin M. Weinberg, 'Scientific Choice and the Scientific Muckrakers', **Minerva 7** (1968/69) 52-63, where one of the "muckrakers" is Daniel Greenberg, **The Politics of Pure Science** (New American Library, 1967).

support of science. Sociologists and political scientists have analyzed these negotiations for legitimation.[14] What interests us here is the nature of the repertoires and their relation to the changing situation of science as a profession.

As a starting point, we note (again) that legitimatory repertoires are not arbitrary window-dressing. They are connected to themes and processes within science. The universalism and disinterestedness of science, as celebrated in the Mertonian norms, is bound up with the quest for "facts". The robustness of facts is guaranteed by the disinterestedness of the scientist (and *vice versa*), and robust facts have universal validity, which would be undermined when universalism was not the norm in judging research outcomes. In the positivistic image of science, this is as it should be, and nothing more needs to be said about the practice of science and its worth. As recent sociology of science is showing, however, there is more to science than the organized scepticism of a community of ideal ratiocinators. There is a duality to be found, which could be an essential element of science.

Life in a laboratory is devoted to the quest for facts -- but facts have to be constructed, painstakingly and with the help of outcomes of experiments, interpretations of the literature and tactics of presentation and argument.[15] A fact cannot be sought and found, like the holy grail. It is the temporary end result of the struggle to increase the degree of facticity that one may attribute to one's claims: starting with "interesting speculations" and "unconfirmed results", one rises to more general acceptation and the robust facts that feature in textbooks. Such struggles are like war games, and in their more expansive moods, scientists will be the first to attest to this. James Watson's Double Helix is a classic example,[16] but anyone

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14 For instance Peter Weingart, 'Science and Technology in a Legitimation Crisis. Hypotheses and Indicators', in: W. Callebaut e.a. (eds.), **Theory of Knowledge and Science Policy** (Gent: Communication and Cognition, 1979), 378-393; and id., 'The Scientific Power Elite - A Chimera: The De-Institutionalization and Politicization of Science', in: Elias e.a., op. cit. note 5, 71-87. And Yaron Ezrahi,'The Politics of the Social Assessment of Science', in E. Mendelsohn, D. Nelkin, P. Weingart (eds.), **The Social Assessment of Science. Proceedings** (Bielefeld: USP Wissenschaftsforschung, Universität Bielefeld, 1978), 157-183; and id., 'Science and the Problem of Authority in a Democracy', in Thomas F. Gieryn (ed.), **Science and Social Structure: A Festschrift for Robert K. Merton** (New York, N.Y.,: The New York Academy of Sciences, 1980), 43-60.

15 Bruno Latour and Steve Woolgar, **Laboratory Life. The Social Construction of Scientific Facts** (London: Sage, 1979).

16 James D. Watson, **The Double Helix** (Atheneum Press, 1968). See also the new, critical edition by Gunther S. Stent (London: Weidenfeld and Nicolson, 1981), which includes comments and reviews, and the printing history, which tells about the many criticisms from Watson's colleagues that made Harvard University Press decide not to publish the book.

sitting in at informal conversations of scientists notes how scientific argument, competitive tactics and concern for reputation are inextricably mixed.

Gilbert and Mulkay have collected many instances of statements of scientists and classified them into an "empiricist" repertoire (about facts waiting to be discovered, scientific arguments unadulterated by war game tactics) and a "contingent" repertoire (in which the war game comes first and scientists are prepared to embrace a social-relativist view of science: truth is what is at any one moment believed by the dominant group).[17] So there is a double standard, schizophrenia (my term, not Gilbert and Mulkay's) even within the practice of science. The dual repertoires are not a ploy of the sociologist: for scientists, the tension between the two provides the point in many "scientific" jokes.[18] To take a step further than Gilbert and Mulkay are willing to go: the dual repertoire is not just a reaction to different perceived audiences and a (mild or virulent) form of self-deception. Part of it is bound up with the programmatic nature of scientific statements: they are claims that in the end, what is now perhaps a not very well supported speculation will accumulate so much "facticity" that the positivist phraseology will be justified after the event. Such claims use the "empiricist" repertoire in a functional way, to indicate what the end result should be. The claims are part of the struggle that is reflected in the "contingent" repertoire, and their occurence serves as a constraint on the arbitrariness of its social relativism.

When scientists have to make statements in the public arena, they will again use a dual repertoire. Claims are often phrased much more authoritatively than the scientific evidence would allow -- but that is because statements by scientists are always programmatic, and because in this way, public confidence in science will be bolstered. Instead of a "contingent" repertoire that reflects the struggles going on within science, there is now a repertoire of political negotiation and image-building tactics. And the "empiricist" repertoire is replaced by a repertoire of facts and official legitimations.

It is instructive to analyze what happens when the strategy of authoritative presentation of science backfires. As many recent controversies, ranging from the nuclear energy issue to the creation-evolution debate, show, scientists can be forced to retreat from their original, authoritative position. A number of defensive tactics are then available to restore legitimation, which can be related to different reactions to the retreat:

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17 G. Nigel Gilbert and Michael Mulkay, 'Warranting Scientific Belief', **Social Studies of Science** 12 (1982) 383-408.

18 Michael Mulkay and G. Nigel Gilbert, 'Joking Apart: Some Recommendations Concerning the Analysis of Scientific Culture', **Social Studies of Science** 12 (1982) 585-613.

- One reaction, often reserved for sympathetic audiences only, is to submit that to retreat from an original claim only shows the rational beauty of science: it corrects itself, it improves all the time, it is not dogmatical.
- A second reaction is to be harsh on the original claim: scientists should be much more careful in making public statements, not bow for the pressure to say "yes" or "no" unequivocally.
- The third reaction is to emphasize the necessity of a unified front: hammer out differences in private, public discord is a sign of weakness.

The first reaction is related to the defensive tactic of being dogmatic about being undogmatical. The provisional and tentative character of scientific knowledge can be glorified, but it risks losing the possibility, on pain of inconsistency, of pronouncing authoritatively against unwanted views. The recent history of science in the public arena is replete with examples where scientists are willing to be dogmatical. Velikovsky's views were condemned out of hand; there was no need to read his books first, because they could contain nothing but nonsense.[19] Flat refusals to investigate the worth of "folk" therapies and alternative cancer drugs are justified by reference to the scientific method which the scientists follow but the proponents of alternatives neglect.[20] The difficulty in the position of the scientists is especially clear in the creation-evolution debate: the argument of the openness of science to new (and interesting) hypotheses was turned against the scientists when creationists developed creation science, taking the first chapter of Genesis as their "hypothesis", and lobbied for "equal time" for the evolution hypothesis as well as the creation hypothesis.[21] To be able to keep the new intruders "outside", the openmindedness of science has to be reserved for insiders.

Boundary maintenance is also important for the defensive tactic related to the third reaction: build new procedures and institutions to contain dissensus, and to contain it within science. The proposal of a Science Court to improve the "presumptive validity of the scien-

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19 Michael Mulkay, 'Some Aspects of Cultural Growth in the Natural Sciences', **Social Research** 36(1) (1969), and Alfred de Grazia (ed.), **The Velikovsky Affair** (London: Sidgwick & Jackson, 1966).

20 Gerard E. Markle and James C. Petersen (eds.), **Politics, Science, and Cancer: The Laetrile Phenomenon** (Boulder, CO: Westview Press, 1980).

21 Dorothy Nelkin, 'Science or Scripture: The Politics of "Equal Time"', in G. Holton and W.A. Blanpied (eds.), **Science and Its Public: The Changing Relationship** (Dordrecht: Reidel, 1976), 209-228.

tific input" into public policy making will serve as an example.[22] The basic premise of this proposal is that controversies become difficult to handle because value components are mixed up with scientific components, especially in areas of uncertainty. The Science Court would provide an environment where scientists, in an adversary proceeding, would hammer out differences and separate value-laden views from "facts". The scientists-judges then produce a verdict, specifying what is known (i.e. what is consensual) and what has to remain undecided. There are many problems with this proposal, but what interests us here is that in spite of the criticisms that have been made it continues to be put forward as a solution. The fact that it has yet to be implemented shows that the proposal is more important for legitimation of science than for improving decision making. If a boundary has to be regained, there must be a boundary lost: scientists clearly feel exposed now that the inner, "contingent" workings of science are on view in public whenever scientists have to participate in decision making.

Instead of by recreating a boundary, loss of legitimation through public dissensus of scientists might also be countered by re-asserting the role of scientists as neutral, uninvolved observers. In the controversy around the impact of halocarbons on the ozone layer, for instance, some of the scientists who participated in the original warning started having second thoughts when the public debate became heated, fuelled by the various scientific prognoses being put forward. One of them is quoted as saying:

> At the moment, half-baked ideas are being produced at a ferocious rate.
> That's all-right when you're only talking to your friends. But it's most regrettable that scientists are telling politicians that they must regulate (as if the evidence was hard).[23]

The ozone layer controversy does not only show how the idealized role of the scientist as outsider authority is used as a resource in the debate; it also shows that such a role is not applicable any more

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[22] A. Kantrowitz, 'Controlling technology democratically', **Amer. Sci. 63** (1975) 505. See further Robert S. Banks, 'The Science Court Proposal: A Literature Review and Case Study', **CRC Critical Reviews in Environmental Control** (August 1980) 95-131.

[23] M. McElroy, quoted in **New Scientist 70** (24 June 1976) 685. The ozone layer controversy provides many examples of legitimation and negotiation. In Lydia Dotto and Harold Schiff, **The Ozone War** (Garden City, N.Y.: Doubleday, 1978), a staff member of the White House is quoted (about issues of SuperSonic Transport) as saying that if these scientists couldn't agree with one another, the White House wouldn't pay any attention to any of them. And on top of that: "No one believes scientists anyway". (p.61) It is clear that in the public domain, scientific consensus is an indicator of political unification, not of truth.

(if it ever was). Scientists are expected to speak out and to raise an early warning based on their expert knowledge. But to get a hearing, the "early warner" has to shout, perhaps overreach himself by presenting his claim as more certain than it is at that stage. The same kind of tight-rope walking between claims and evidence that occurs within science, is now required for assertions in the public arena.[24]

The thread that runs through this analysis is the disappearance, or at least the blurring of the boundary between science and the public domain. The dual repertoire of scientists is exposed and has to be extended and adapted to the requirements of the public practice of science. At the same time, the traditional legitimatory repertoires of science lose their obviousness. They are part and parcel of the new public practices, not limited anymore to negotiations for social support of science. What new repertoires will emerge is not clear yet, although it is clear that the defensive reactions which hark back to old roles and legitimations, are insufficient.

Professional insecurity and the need for legitimation

Why are defensive reactions felt to be necessary ? Why do scientists see a legitimation crisis, find the ghost of anti-science everywhere, think of themselves as a Gideon's band that should defend science and try to raise auxiliary forces ?

> Yet as we go out to proselytize we recognize the time is late,
> for the world is on the verge of turning against us.[25]

The Biblical ring of these words is not the only reason why the feeling of being threatened can be seen as a case of cognitive dissonance and its resolution by increased fervour for the (lost?)

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[24] A much more difficult situation confronts the professional scientist in an organization, who finds it necessary to warn, at an early stage, of problems or risks associated with a current project of the organization. In such a "private arena", his chances of getting a fair hearing may be low -- but going public ("whistle blowing") on the basis of the limited evidence is a socially and professionally risky step.To protect such potential professional dissenters, professional associations and the Nuclear Regulatory Commission in the U.S.A. have instituted special procedures. See Rosemary Chalk and Frank von Hippel, 'Due Process for Dissenting "Whistle-Blowers"', **Technology Review** (June/July 1979) 49-55.

[25] Richard L. Wolfgang, at the International Conference on Education in Chemistry, Snowmass- at-Aspen, CO, July 1970, as quoted by David Layton, **Science for the People** (London: George Allen & Unwin, 1973), opening page.

cause.[26] Scientists say they just "know" that the anti-scientific attitude is widespread, they do not need any opinion polls. And if confronted by results of opinion polls from Europe and the U.S.A., showing that public confidence in science remains high on all counts, they find reasons not to believe these results. Or devise new questionaires to show up latent ambivalence in the attitude toward science, this being the very least they would settle for.[27]

On a small scale, we have documented the projective nature of the views of scientists about anti-scientific attitudes.[28] A number of chemists were asked to indicate how they felt the public would react to a series of statements covering various aspects of attitudes toward chemistry. The same statements were given to a sample of the (urban) population. Comparing the two sets of data, a consistent pattern was found: the chemists themselves took a positive view of chemistry, the public was positive, but slightly less so. And in the perception of the chemists, the public would be more, sometimes much more negative than they actually were. The largest differences between perceived and actual public opinion were found for those statements in which the professional identity of the chemist was involved. e.g. "I do not trust chemists very much".

Since our results fit in very well with a variety of other indications, we may claim that scientists are insecure in their professional identity, and this insecurity is the root of defensive reactions to the new role of science in society. To some extent, the nature of scientific work is responsible for the concern with pro-

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[26] Compare the famous study by L. Festinger, H.W. Riechen, and S. Schachter, **When Prophecy Fails** (Minneapolis, Minn.: University of Minnesota Press, 1956), of the reactions of a small sect to the failure of the prophecized end of the world.

[27] The point about scientists (in this case chemists) knowing that people think bad about them is made by Albert C. Zettlemoyer, **Chem.Engin. News** (Dec. 6, 1982) 37. When the results of the first European opinion poll were in, showing confidence in science with about 80% of the population, meetings were convened to discuss what had, evidently, gone wrong. A new opinion poll was proposed, that should probe further to discover negative attitudes to specific sciences and technologies. See Commission of the European Communities, **Science and European Public Opinion** (Brussels, October 1977) and id., **The European Public's Attitudes to Scientific and Technical Development** (Brussels: February 1979). The issue is more complicated than presented in the main text. Government officials and decision makers are confronted with a loss of legitimation of government decisions. They also find that to invoke scientific expertise is not a sufficient legitimation anymore. What is easier than to put the blame on a supposed loss of confidence in science, instead of a loss of confidence in decision making ? So government officials and scientists all too easily converge in their suspicions of opinion poll outcomes that suggest continuing trust in science.

[28] Internal report, Chemistry and Society Programme, University of Leiden, 1982. Some results were discussed in A. Rip, W. Slot, 'Beelden van chemie', **Chemisch Magazine** (Oktober 1982) 600-602.

fessional security. Doing research, trying to domesticate the chaos that is the research front, is an insecure activity, that can be accomodated psychologically only when there is stability in other respects. Thus as a profession, scientists are no revolutionaries. External pressures and constraints quickly induce feelings of being threatened, disliked or misunderstood. The same focus on security in order to be able to get on with research implies that complaints are rarely turned into action; prudential acquiescence is the rule.[29]

A second reason for professional insecurity is the change, for scientists as for other professionals, in their position in the societal division of labour. Collegiate control and legitimate authority in the professional domain, all painstakingly won during the 19th and early 20th century, are eroded now that the structure of industrial societies is changing.[30] For different disciplines, there are large differences in the nature of their professionalization and the security of their position. Chemistry might feel the pressures for change more severely than other disciplines. But none can evade them completely.

A third reason why insecurity is so pervasive is bound up with the legitimations of science themselves. Success in getting a legitimation accepted may undermine its original function to increase support for science. The argument for the rationality and problem-solving capacity of science, if believed, draws science into the public arena where it gets dirty hands and cannot keep up the image of neutrality so necessary to keep its rationality pure. The legitimation of science as the goose with the golden eggs has been institutionalized in science policy, but politicians and the public are becoming a bit impatient. Patronage continues, but control is not as unthinkable as it used to be:

> The danger of political constraints on science now comes not so much because politicians disapprove of the methods of science, but because they take seriously what some scientists tell them about the way in which scientific discovery leads to practical benefits. Once they believe that, it is inevitable that they should try to control practical outcomes by anticipating the effects of research, and manipulating it in one way or another.[31]

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[29] Joseph Haberer, **Politics and the Community of Science** (New York: Van Nostrand Reinhold, 1969) presents historical case-studies of "prudential acquiescence". See also id., 'Politicalization in Science', **Science 178**(1972) 713-724. The conforming attitude of scientific institutions is noted by Van den Daele, op. cit. note 2, p.34.

[30] Terence Johnson, **Professions and Power** (London: Macmillan, 1972).

[31] Don K. Price, 'Endless Frontier or Bureaucratic Morass?', **Daedalus** (Spring 1978) 75-92, at p. 85.

If one then looks at the reactions of scientists to measures of the politicians, for instance the Rothschild reforms in Britain in the 1970's, the picture is as before: scientists are able to complain, but not to mobilize themselves as a political force.[32]

Implications

In conclusion, we can say that defensive reactions are to some extent unavoidable. Professional insecurity is bound up with the nature of scientific practice, as well as with the changing position of science and scientists in society. But defensive reactions can also be a symptom of cognitive dissonance, and of not wanting, or not being able to see what is happening "outside". Legitimatory repertoires, created as a resource, may become a constraint when circumstances change. Many of the traditional legitimations of science have a dual structure, based on the notion that science should be a black box with inner workings accessible to scientists only. Now that science is becoming part of the public domain, this kind of legitimation is counterproductive. In a changing world, not to recognize one's legitimatory repertoire for what it is, is to remain its prisoner.

My analysis based on the existence of dual repertoires within science and a lack of them when science functions in more public domains, has implications for future developments, of legitimation of science as well as for specific issues like scientific languages ("jargon"), scientific advice and popularization.

Scientific languages, i.e. linguistic repertoires specific to a scientific specialty or discipline, arise as ways of expressing the facticity sought after in a specific research area. For outsiders, the use of a specific scientific language is a barrier to communication. But it should be noted that the barrier is not in the language itself, but in the practice in which such a language is functioning. The "hardness" of facts in a research area is described with the help of its scientific language, and impossibility of deconstruction of the facts, the final goal of scientific work, is indicated by the unassailability of the description. From the outside, deconstruction of such description becomes already impossible because of the impenetrability of the "jargon". Outsiders have to accept the facts as they are presented, impenetrability carries authority. Although abuses are possible (facticity being simulated by the use of appropriate "jargon"), there are few problems in the communication between scientific specialties and disciplines. Scientists use the results of other specialties, including their "jargons", as if they were unproblematic, as tools in their own work.

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[32] S. Yearley, 'Analysing Science and Analysing Scientific Discourse. On the Argumentative Strategy of Scientists in the Public Realm' (paper, Deutschlandsberg, Conference of the European Association for the Study of Science and Technology, September 1982).

When the audience is no longer composed of scientists in other fields, but of non-scientists -- decision makers when scientists act as advisers, the public when scientists have to make a public stand or turn to the popularization of science --, the role of scientific languages changes. Again, hardness of facts is related to the impenetrability of the language in which they are clothed. But now the facts are not being used in other scientific practices, and the authority they carry cannot be revoked by deconstructing them (if necessary), only by delegitimating them. One way of delegitimating scientific authority then is to condemn scientific language as unnecessarily esoteric, as a protective cloth screening off science from criticism. Even in the case of popularization of science, there is impenetrability. Understanding of science is provided solely on the terms of the popularizer, and by giving up the original descriptions couched in scientific language, the popularizer also makes it impossible for the layman to find a way of deconstructing the facticity of the message.

As the last remark shows, impenetrability may be constructed even without esoteric scientific languages. In the case of scientific advice, a comparable situation exists, where decision makers prefer to base their action on their trust in the adviser, rather than their understanding of the advice -- "don't bore me with the details, just tell me the facts" is the attitude. Here, as well as when scientists have to take a public stand, e.g. in a controversy, the authority of impenetrability may be used defensively, to protect science from possible delegitimation. One may deplore this, just as one may deplore the esoteric character of scientific language. But as long as such practices, linguistic or otherwise, are functional in the relevant situations they will continue.

When looking for changes, one should take the analysis in this paper into account. Within science, impenetrability is complemented with deconstructability by the functioning of dual repertoires, supported by the social institutions of science. In more public domains, such dual repertoires, nor the social institutions to carry them, are always available.

For scientific advice, a new situation may be emerging. Controversies over scientific advice have led to processes of public deconstruction. No generally accepted social institutions have yet arisen, but the existence of controversy is accepted as legitimate and procedures to handle dissensus are sought. For the popularization of science, the prospects are less sanguine. There is much willingness, on the side of scientists, to popularize or assist in populari-

zing,[33] but always in the expository or defensive mode. Deconstruction is rare -- possibly because of the power and status differentials involved --, and so there is little incentive to evolve a dual repertoire.

For the general issue of legitimation of science in a changing world, the same type of argument applies. Instead of a struggle for facticity as within science, there must now be a struggle for "legitimity" in the public domain. But there are no dual repertoires, nor social institutions to carry the continuous production of legitimation. The tactics and strategies described in this article can be seen as attempts or experiments. What is clear, however, is that present ambivalent legitimations of science like the goose with the golden eggs, should not function defensively, to protect an impenetrable black box of pure science. To become dynamic, legitimity has to be achieved by the actual production of (intended) usefulness, and the evolution of a social institution that carries the testing and potential deconstruction of such usefulness. Then, fashionable terms like innovation and socially relevant science will leave a residue in the long term.

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[33] Popularization of science has always been considered important, from 17th and 18th century notions of "natural theology", the 19th century ideals of scientific progress, to the 20th century idea of shaping society (and the continuing needs of science to legitimate itself). The present willingness of scientists is documented by Sharon Dunwoody and Byron T. Scott, 'Scientists as Mass Media Sources', **Journalism Quarterly 59** (1) (Spring 1982) 52-59.

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