# 3. The importance of post-modern science policy[1]

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## 3.1 INTRODUCTION

Policy making typically is a modernist venture. One indicator is the strong 'instrumentalist' push for robust methods that allow the policy maker to make a difference, to exert influence – in other words, to act at a distance. Illustrative for the problematic is the message of the subtitle of Pressman and Wildavsky's book, 'How Great Expectations in Washington Are Dashed in Oakland' (Pressman and Wildavsky 1984), which summarizes both the modernist thrust and its limitations. There is an illusion of agency, and while it can be productive (it gets something done, even if not what was originally intended) it is in principle counterproductive, when it neglects complexities and the own dynamics of the world out there in Oakland – and this reduces the chance of achieving its objectives.

In the modernist approach, there is a strong presumption that the world is an obstacle to the necessary change towards a better world, and that action of the policy maker is required to realize this change. But if the world is essentially in flux, that is, Heraclitean,[2] non-linear, chaotic in the sense that multi-actor interactions determine what happens, action predicated on the modernist idea of an actor working to realize his goal, and achieving it because of his efforts, will by definition be unproductive.

The challenge of steering in a Heraclitean world is exacerbated for science (as well as technology) policy because it creates flux: novelty creation is an essential feature of science and technology. Thus, there are good reasons to explore what I call post-modern science policy, in a loose sense of the term 'post-modern'.[3]

I am not implying that individual policy makers are never sensitive to the perplexities, and the limited scope of their action among a multitude of actions and interactions. The structure and culture of the policy environment forces a modernist approach upon them, as a sort of occupational disease, whether they identify with it or not.

Modernism is pervasive, also in terms of the content of science policy. If science policy making is about the attempt to steer research and profit from knowledge production, directly or indirectly by creating institutions or structures (framework conditions), the temptation is to treat knowledge as a commodity. But science is a search practice, and the aimed-for commodity slips through policy fingers. It is the research practices which determine what is possible and which outcomes can be realized, not the stylized products that count as scientific facts.

In addition to the insufficiently recognized complexity of knowledge production, there is flux also in the sense of overall changes in knowledge production which require new responses. That there are such changes is now widely recognized, and has become an accepted topic of conversation with science policy makers; one example is the thesis of a new mode (Mode 2) of knowledge production (Gibbons et al. 1994). But responses are shaped more by policy bravura ('Let's do something, and be seen to be doing it!') than by real understanding (cf. Rip 2000).

Thus, it is important to understand what is happening with knowledge and knowledge production, and on that basis speculate about productive approaches. This is not necessarily an argument for post-modernism. It might be 'enlightened modernism' that we need, in the sense that policy is not seen as the once-and-for-all cause of aimed-at effects, but as one stimulus among others, and to be complemented by continual repair work.[4]

In very general terms, I would argue that one needs to relativize (ironize?) one's modernism in order to be productive. This is visible in the new philosophies of steering, and in the view of implementation as joint learning rather than forceful policy realization (cf. Rip and Nederhof 1986). Thus, also in science policy, modernist policy making is not necessary – but it is difficult to avoid. For one thing, policy makers are accountable, and therefore need universalistic criteria (cf. the issues of allocation of funding to projects and programmes) and action that can be attributed to them (cf. policy bravura). This is exacerbated by the rationalistic image of science, and the predominance of scientists in policy making (as staff and as advisors).

In practice, one does see examples of institutional-entrepreneurial action which includes repair work, as in the orchestrating of science by programme directors or committees. In other words, new philosophies of steering can be taken up in practice, even if they cannot be argued officially for fear of seeming to be too laid-back and/or too Machiavellian.

## 3.2 COMPLEXITIES OF KNOWLEDGE PRODUCTION

Given that in official policy repertoires as well as in practical action, the complexity of research, and of knowledge production more generally, is insufficiently recognized, science policy appears to address only part of its domain. And when the complexities then reassert themselves, it has no ‘tools’ for repair work other than a modernist assertion of (assumed) force. This is particularly true for top level science policy.[5] At the intermediary level, there are stronger links with practices, and willingness to do repair work (even if goals and legitimations remain modernist, and the ‘illusion of agency’ remains strong).

There are at least four elements of complexity:

- *Knowledge is not a commodity.* Production of knowledge is not like production of a good. It is a search practice, and cannot be regularized completely (Rip 1995). When routine measurements dominate, as in medical-analytic laboratories, regularization is important. What remains is that, in economic terms, knowledge production is embedded in a service. It is also difficult to put an economic value on knowledge. Special practices, or institutions like the patent system, have to evolve before there can be some (limited) valuation. Knowledge is not scarce, in the sense that it remains available after being consumed. But knowledge is ‘sticky’, embodied (in people, in artefacts) or when available in a text, requires special competence to be taken up (cf. also Callon’s argument that science is not public of its own accord, Callon 1994).
- *Knowledge is never completely codified.* Neither research nor knowledge are ‘packaged’ of their own accord. Packages are constructed when reporting to particular audiences. While this feature may be recognized in the abstract, it is neglected in the practice of policy makers and intermediary agencies. One example is how funding agencies (understandably) want to know what the impact has been of the projects they have funded. But then they run into the project fallacy: value and impact depend on the overall research activity over time, not on something that has been packaged as a project to mobilize funding.
- *Research and knowledge are open-ended, and richer than the particular purpose they were created for.*[6] In economics, this ‘richness’ is discussed under the heading of ‘spillovers’, and an OECD study emphasizes the importance of ‘spillovers’ from basic research, and the emergence of networks (of firms, sometimes including research institutes) to capture the added value (OECD 1992).
- *Eventual take-up of knowledge (‘application’) is an activity in itself, not the effect of a knowledge push.* And even more important, it is almost

always indirect: knowledge products are delivered into a knowledge reservoir, carried by what one might call an epistemic community (cf. Haas 1992), and knowledge users pick up their own new combinations from the reservoir.[7] Note that the phrasing is sufficiently broad to encompass other knowledge than 'traditional' scientific knowledge.

Clearly, these complexities are a challenge to the modernist approach. Conspicuous failures are not always recognized, for one thing because policy goals are redefined after the fact,[8] or forgotten. The other reason, as I have intimated a number of times already, is due to repair work by actors, often at lower levels in the system, and thus less visible to the outside world.

## 3.3 NEW (AND OLD) FORMS OF KNOWLEDGE PRODUCTION

There is recognition of secular changes in knowledge production, some of the claims resonating with my terminology of post-modernism, as when Ravetz offers his diagnosis of 'post-normal science' (Funtowicz and Ravetz 1993; see also Dankel et al. 2017). Gibbons et al's diagnosis of a 'Mode 2' of knowledge production (Gibbons et al. 1994) has drawn a lot of attention, also with policy makers (see Rip 2000; Hessels and Van Lente 2008), and while it does identify important changes, it is too sweeping, and posits the shift from Mode 1 to Mode 2 in a triumphant way.[9] In addition, Mode 1 is itself a historically located phenomenon, and one might conceive of further modes (earlier as well as running in parallel).[10]

So let's identify some important changes and their implications. One intriguing change is how simulation replaces direct experimentation (as controlled experimentation has replaced experience of complex reality, i.e., the 'natural history' mode of knowledge production). Computational physics and chemistry are but one example, and while they owe their scope to the power of modern computers, the epistemological shift does not depend on computing capability as such. This point returns in the observation that computer models are used increasingly to find out 'something' about the 'real' world. The recent, and stronger, statement of the International Panel on Climate Change about effects of human activities on climate was not based on measurement, but on increased sophistication of the models. Computer models have become an esoteric mode of storytelling.

A further example: software science and engineering live in a grey zone where research is done, learning occurs, findings are published and taken up – but findings about what? It is definitely unlike traditional 'high' science, and it is not fully covered in R&D statistics. Its focus on programming (and the rules involved) may indicate a structural equivalence with social engineering, both traditional and modernistic, and the ways in which cumulative experience is distilled into knowledge.[11]

Knowledge is also produced in direct relation to professional and craft practices, rather than being transferred to them from the 'high' science mode of knowledge production. This mode of knowledge production has, for historical and political reasons, been neglected in science policy. It is essential, though, to understand technical and engineering sciences, and to my mind, social sciences as well.[12]

There is overlap with knowledge produced in the form of (strategic) intelligence. Spying, in its various guises, need not be principally different from scientific observation and analysis, especially of the 'natural history' kind. The acceptance of this mode of knowledge production has become even more important with the extent of human intervention. Chernobyl fall-out (and the knowledge of Cumbrian sheep-farmers, cf. Wynne 1996) and introduction of genetically modified organisms into the environment (cf. Krohn and Weyer 1994) are dramatic examples. To put it in Star Wars terms: the environment strikes back, and natural history – intelligence about the reactions of the environment – is reinstated, most easily in its late 20th-century format of computer modelling and relevant data collection.[13]

These examples of modes of knowledge production imply that quality of knowledge and quality assurance cannot be defined in terms of universalism and a royal road to rationality. Instead of universalism, cosmopolitanism sustained by 'circulation' is the criterion (cf. Rip 1997). Instead of abstract rationality, relevance to the purpose at hand (a pragmatic criterion) becomes important. Quality assessment can then not be the prerogative of the 'peers' anymore (cf. the call for 'extended peer review' in Funtowicz and Ravetz's (1993) diagnosis of post-normal science).

## 3.4 POST-MODERN SCIENCE POLICY?

How to take complexities into account, and avoid unproductive illusions of agency? To offer a master plan here would be inconsistent with my argument. But I can identify elements or features of post-modern science policies.

One element of enlightened science policy is patronage, in particular with its possibility of the patron having discretion in allocating resources rather than following universalistic criteria. Universalistic criteria in the support of science have become increasingly important since the 1960s, in particular for the allocation of funds; the whole debate on 'fairness' in peer review is predicated on universalism as the standard. In contrast, charities and other big foundations can be enlightened patrons of science, and have been quite successful, the Rockefeller Foundation being a prime example.[14] Can funding agencies, which are linked to government bureaucracies, do this as well?

There is the obvious risk of getting captured in clientelist relationships. On the other hand, entrepreneurial individuals in agencies have been able to exert some discretion. This requires *Zivilcourage*, and an organizational culture of *Zivilcourage*, to take risks and accept failures.

Another element is to include knowledge policy in science policy.[15] To have a productive knowledge policy, key aspects of knowledge and knowledge production have to be taken into account. One aspect is that the transformation of local experience into cosmopolitan knowledge is not limited to high-science approaches, but does require 'circulation' to be robust. Another aspect is the function of knowledge reservoirs (also within science) and how these are structured – which is perhaps more important than the production of new knowledge. Thirdly, packaging into knowledge products is important for transfer, but does not by itself guarantee quality and relevance.

These approaches avoid the limitations of modernism at the side of knowledge production, because they focus on process rather than product. They can still fall victim to modernism at the policy side: the need to show agency. This particular aspect of modernism may well be impossible to overcome.

Post-modernism appears to undermine reasons and motives for agency, but modernism remains attractive in the sense that it does push towards action – through an illusion of agency. It is the combination of this push for action with negation of complexity by seeing it as a barrier to be overcome which creates the problems. Particularly important, as I have emphasized, is that repair work and other routes towards productivity are not taken up.

I proposed alternative science policy approaches, including a plea for patronage, with its discretion (and the risk of arbitrariness). This is one example of allowing for local variation, for heterogeneity (cf. Rip 2000), which can be seen as characteristic of post-modernism.

National research systems can be more or less supportive of heterogeneity, and bottom-up dynamics in aggregation into agendas and policies. National research systems have been mapped onto the dimensions of 'aggregation' and 'steering' (Rip and van der Meulen 1996). At the time, we identified the Japanese and the Dutch systems as scoring high on aggregation and low on steering – that is, post-modern. There are signs that the systems are recently moving towards stronger top-down dynamics (including New Public Management) – a case of modernism winning out? But there are also counter-indications, for example in relation to so-called Grand Challenges, where governments are learning to delegate most of the 'steering' to new constellations of actors (Kuhlmann and Rip forthcoming).

Heterogeneity is a more pervasive issue. It can be visible in institutions (which differentiate, create new linkages, and accept 'non-standard' institutions in their midst), in modes of knowledge production (interdisciplinary, variety of locations) and in cultures (renewed interest in local knowledge).

Phrased in this way, there is overlap with the diagnosis of Ulrich Beck and others of 'reflexive modernization' or 'a second modernity' (Beck et al. 1994; Beck and Lau 2005). One feature of reflexive modernization is the recognition of the irrelevance of traditional dichotomies; as Beck phrases it, it is not a matter of 'either-or', but of 'and-and'.[16,17] Unfortunately, Beck and his collaborators have not addressed issues of science policy, other than critical remarks about expertise and a general point about the emergence of sub-politics.

To conclude, I will quote from a relevant and perceptive report of a European Expert Group:

> the governance of scientific institutions is under pressure, not least because of different contexts of governance, simultaneously pushing innovation, democratization and scientific integrity. New forms of governance are emerging: the discourse on responsible development and innovation, including attention to ethics and codes of conduct; interactive forms of technology assessment; and experiments with public engagement. Again, these are not without tensions, but they indicate that we do not have to fall back on traditional forms of governance. The challenge is to support ongoing dynamics, rather than containing them, so dynamic governance is called for. (EU MASIS Expert Group 2009, p. 4)

The report is concerned with ongoing changes, but refuses to treat them as problems that can and must be solved somehow through the agency of policy actors. Ongoing dynamics, understanding and supporting them (without abdicating the responsibility of a government) are upfront. As a Heraclitus of the 21st century, they're proposing dynamic governance.

I have used the notion of post-modern science policy as a rallying call. In a sense, it was a ladder to get to the next floor of the building, and as the philosopher Ludwig Wittgenstein phrased it, it can now be thrown away. But we still need a rallying call? Like 'dynamic governance'?

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## NOTES

1. The text of this chapter draws heavily on Rip (1998b).
2. After the aphorism attributed to the pre-Socratic Greek philosopher Heraclitus, 'You never step in the same river twice.' The ontological version was phrased by Heraclitus as 'panta rhei, kai ouden menei' (everything is in flux, and nothing is stable). Can one do policy at all when there is no substantial point of reference? I have called this the challenge of Heraclitean steering (Rip 1998a), addressing

flux without a solid Archimedean foundation. (Archimedes supposedly said: give me a solid resting point (dos moi pou sto) and I will move the world.)

3. So in this text I am not taking a position in the debate on post-modernism in philosophy and critical social theory. The reason to use the term is to have a resonance with that debate, in particular its suspicion of 'grand narratives' – like my point about the illusion of agency. There is overlap with my argument about a co-evolutionary approach, in particular to science and technology and science and technology policy (Rip 2006).
4. It is an irony of history that modernism, linked up with the Enlightenment, has become 'blind', and needs to become 'enlightened' itself.
5. This focus on 'steering' is more visible in some countries, like the UK, than in others, like the Netherlands, see the comparisons in Rip and van der Meulen (1996).
6. In Rip and Nederhof (1986), we give an example of a microbiological project which figured first in the National Coal Research Programme, and then in the Biotechnology Programme. Relabelling of research (to attract further resources) is therefore not necessarily swindling new sponsors out of money. And even if it starts out as a swindle, it can turn into a new and fruitful venture – provided the researchers are pushed to do something related to the label.
7. This notion was introduced in the 1970s (Gibbons and Johnston 1974), but deserves to be taken more seriously.
8. A striking example is the Alvey Programme in the UK in the 1980s. The goal was to improve UK competitiveness in information and communication technologies. This was not achieved, but the means, collaboration between industries and between industry and academia, had created a collaborative culture. The latter was eventually quoted as the important goal to be achieved in such programmes (see Rip 1990 on R&D programme evaluation).
9. The thrust of their argument can be summarized as: Mode 2 is upon us, so better not resist.
10. Since Mode 1 appears to have become dominant in the course of the 19th century, one should inquire into what happened earlier. I have proposed to identify a Mode 0, visible since at least the 16th century (Rip 2000); see also Rip (2011).
11. See the interesting example discussed in Stephen Lansing's (1991) book, *Priests and Programmers*. A 'modern' water management system was introduced in Bali (Indonesia), bypassing the traditional management system centred on the priests in their temples (e.g., deciding upon letting certain fields dry out for a time, which – as it turned out later – killed the weeds and the pests). The 'modern' system led to great problems, and a software engineer and an anthropologist collaborated in the repair work: the anthropologist reconstructing the traditional system and its rationale, the software engineer putting the rules of these practices in a computer program. Jakarta could accept the new programmes as rational and modern (because computerized), and the priests went along because it was their rules which were programmed. (I'm grateful to David Turnbull who drew my attention to this example at the EASST/4S Conference in Bielefeld, 13–16 October 1996.)
12. In engineering and technology, knowledge production proceeds through aggregation within the frame of a 'technical model' and the further elaboration of such 'technical models' (Disco et al. 1992). Similarly, whole areas of social and behavioural science appear to work with 'social models', for example, the organ-

ization as a machine, producing outputs, and being overseen by a manager. This 'social model' allows organization and management scientists to do research in an academic setting, which is relevant to professional practices of managers (though not necessarily of workers).

13. An interesting further twist is how data collection is now taken up in so-called citizen science, and citizens are becoming involved in analysis of astronomical observations and calculations by sharing computer power.
14. The Rockefeller Foundation, based in the USA and with its entrepreneurial director Warren Weaver since the 1930s, had a generalized interest in natural and social science, linked to its concern about the future of urban-industrial society. It stimulated new developments in biology (including work that paved the way for molecular biology), anthropology and social science from the 1930s until at least the 1960s (Kohler 1976; see also Abir-Am 1982). Being funded by the Rockefeller Foundation added to the reputation of the researcher and the research institution.
15. Harry Collins advocated knowledge policy in the early 1980s (also in the *EASST Newsletter*). Recently, I have insisted on the importance of analysis of Knowledge, Research and Innovation Systems, rather than Research and Innovation Systems (Rip and Mouton 2006; Rip 2008).
16. 'The constructed nature of the boundaries is understood even when they are treated as solid under the circumstances at hand' (Beck et al. 2003, p. 20).
17. This is reminiscent of one of Latour's arguments in 'We Have Never Been Modern' (Latour 1991). Latour's work in general is one of the backdrops to this chapter. I only mention his play on the Archimedean quest for a solid reference point in 'Give Me a Laboratory and I Will Raise the World', where his key point is that such a solid detail is not given but actively constructed (Latour 1983); and his argument, like mine about search practices as constitutive, in 'From the World of Science to the World of Research?' (Latour 1998).

## REFERENCES

Abir-Am, Pnina (1982). The Discourse of Physical Power and Biological Knowledge in the 1930s: A Reappraisal of the Rockefeller Foundation's 'Policy' in Molecular Biology. *Social Studies of Science* 12(3), 341–82.

Beck, Ulrich and Christoph Lau (2005). Second Modernity as a Research Agenda: Theoretical and Empirical Explorations in the 'Meta-Change' of Modern Society. *British Journal of Sociology* 99(4), 525–57.

Beck, Ulrich, Anthony Giddens and Scott Lash (1994). *Reflexive Modernization*. Cambridge: Polity Press.

Beck, Ulrich, Wolfgang Bonβ and Christoph Lau (2003). The Theory of Reflexive Modernization: Problematic, Hypotheses and Research Programme. *Theory, Culture & Society* 20(2), 1–33.

Callon, Michel (1994). Is Science a Public Good? *Science, Technology & Human Values* 19(4), 395–424.

Dankel, Dorothy J., Nora S. Vaage and Jeroen P. van der Sluijs (eds) (2017). Post-Normal Science in Practice. *Futures* 91 (August), 1–90.

Disco, C., A. Rip and B.J.R. van der Meulen (1992). Technical Innovation and the Universities: Divisions of Labour in Cosmopolitan Technical Regimes. *Social Science Information* 31(3), 465–507.

EU MASIS Expert Group (2009). *Challenging Futures of Science in Society. Emerging Trends and Cutting-edge Issues*. Brussels: European Commission, Directorate General for Research.

Funtowicz, S.O. and J.R. Ravetz (1993). Science for the Post-normal Age. *Futures* 25(7), 735–55.

Gibbons, Michael and Ron Johnston (1974). The Roles of Science in Innovation. *Research Policy* 3(3), 220–42.

Gibbons, Michael, Camille Limoges, Helga Nowotny, Simon Schwartzman, Peter Scott and Martin Trow (1994). *The New Production of Knowledge. The Dynamics of Science and Research in Contemporary Societies*. London: Sage.

Haas, Peter M. (1992). Introduction: Epistemic Communities and International Policy Coordination. *International Organization* 46(1), 1–35.

Hessels, Laurence and Harro van Lente (2008). Rethinking New Knowledge Production: A Literature Review and a Research Agenda. *Research Policy* 37(4), 740–60.

Kohler, Robert E. (1976). The Management of Science: The Experience of Warren Weaver and the Rockefeller Foundation Programme in Molecular Biology. *Minerva* 14(3), 279–306.

Krohn, Wolfgang and Johannes Weyer (1994). Society as a Laboratory: The Social Risks of Experimental Research. *Science and Public Policy* 21(3), 173–83.

Kuhlmann, Stefan and Arie Rip (forthcoming). Next Generation Innovation Policy and Grand Challenges. Paper for a Special Issue of *Science and Public Policy*, ed. Jakob Edler and Wouter Boon.

Latour, Bruno (1983). Give Me a Laboratory and I Will Raise the World. In Karin Knorr-Cetina and Michael Mulkay (eds), *Science Observed: Perspectives on the Social Study of Science*, London and Beverly Hills, CA: Sage, pp.141–70.

Latour, Bruno (1991). Nous n'avons jamais été modernes. Essai d'anthropologie symétrique. Paris: Editions La Découverte. English translation Harvester Wheatsheaf, 1993.

Latour, Bruno (1998). From the World of Science to the World of Research? *Science* 280(10 April), 208–9.

OECD (1992). *Technology and the Economy*. Paris: OECD.

Pressman, Jeffrey L. and Aaron Wildavsky (1984). *Implementation. How Great Expectations in Washington Are Dashed in Oakland; Or, Why It's Amazing That Federal Programs Work at All, This Being a Saga of the Economic Development Administration as Told by Two Sympathetic Observers Who Seek to Build Morals on a Foundation of Ruined Hopes*, 3rd edn, Expanded. Berkeley, CA: University of California Press.

Rip, Arie (1990). Implementation and Evaluation of Science & Technology Priorities and Programs. In S.E. Cozzen, P. Healey, A. Rip and J. Ziman (eds), *The Research System in Transition*. Dordrecht: Kluwer Academic, pp. 263–80.

Rip, Arie (1995). New Combinations. *European Review* 3(1), 83–92.

Rip, Arie (1997). A Cognitive Approach to Relevance of Science. *Social Science Information* 36(4), 615–40.

Rip, Arie (1998a). The Dancer and the Dance: Steering in/of Science and Technology. In Arie Rip (ed.), *Steering and Effectiveness in a Developing Knowledge Society*. Utrecht: Uitgeverij Lemma, pp. 27–49.

Rip, Arie (1998b). Modern and Post-modern Science Policy. *EASST Review* 17(3) (September), 13–16.

Rip, Arie (2000). Fashions, Lock-ins, and the Heterogeneity of Knowledge Production. In Merle Jacob and Thomas Hellström (eds), *The Future of Knowledge Production in the Academy*. Buckingham: Open University Press, pp. 28–39.

Rip, Arie (2006). A Co-evolutionary Approach to Reflexive Governance – and Its Ironies. In Jan-Peter Voß, Dierk Bauknecht and René Kemp (eds), *Reflexive Governance for Sustainable Development.* Cheltenham, UK and Northampton, MA, USA: Edward Elgar, pp. 82–100.

Rip, Arie (2008). Dynamics of Knowledge, Research and Innovation Systems. UNESCO Global Research Seminar, Paris, 28–29 November.

Rip, Arie (2011). Protected Spaces of Science: Their Emergence and Further Evolution in a Changing World. In Martin Carrier and Alfred Nordmann (eds), *Science in the Context of Application: Methodological Change, Conceptual Transformation, Cultural Reorientation*. Dordrecht: Springer, pp. 197–220.

Rip, Arie and Barend J.R. van der Meulen (1996). The Post-modern Research System. *Science and Public Policy* 23(December), 343–52. Also published in Rémi Barré, Michael Gibbons, Sir John Maddox, Ben Martin and Pierre Papon (eds), *Science in Tomorrow's Europe.* Paris: Economica International, 1997, pp. 51–67.

Rip, Arie and Johann Mouton (February 2006). *Africa Prime, a Demonstrator Project.* Enschede. Final Report of the Africa PRIME project supported by the European Union Network of Excellence PRIME.

Rip, Arie and Anton J. Nederhof (1986). Between Dirigism and Laissez Faire: Effects of Implementing the Science Policy Priority for Biotechnology in the Netherlands. *Research Policy* 15, 253–68.

Wynne, B. (1996). May the Sheep Safely Graze? A Reflexive View of the Expert-Lay Knowledge Divide. *Risk, Environment and Modernity: Towards a New Ecology* 44–83.