

How to make the mode 2 thesis sociologically more robust?

A comment on Monika Kurath and Janus Hansen

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Over the last years, the intense and vivid debates which had developed around the so called mode 2 thesis after the publication of "The New Production of Knowledge" (Gibbons et al. 1994) and "Re-Thinking Science" (Nowotny et al. 2001) seem to have significantly abated. Nevertheless, the controversial issues that were raised in those disputes are, of course, far from settled or out-dated. Quite to the contrary, the questions concerning the changing relations of science and society and the potential emergence of new forms of knowledge production and expertise, termed "socially robust knowledge" and "socially distributed expertise" by Nowotny et al. (2001), still are highly relevant for STS. Given this background, the publication of Monika Kurath's (2009) and Janus Hansen's (2009) papers in the last issue of STI-Studies offers a good chance to reconsider these issues from some temporal distance. In my comment, I will make some remarks on how the mode 2 thesis is addressed and criticised in each of the two papers and then, in my short conclusion, argue for a primarily heuristic use of this thesis and the concepts mentioned above.

Nanotechnology governance – without socially robust knowledge?

In her paper on "Nanotechnology Governance", Monika Kurath, uses the concept of "socially robust knowl-

edge" in order to examine to which extent the alleged "governance turn" in recent science and technology policies is actually linked with greater accountability and public participation. While there is obviously much talk of "public engagement" or "responsible technology development" in the field of nanosciences and nanotechnologies (NST), Kurath's comparative analysis of 14 self-regulatory and soft law schemes and six public engagement projects presents rather disillusioning results. With regard to the mode 2 thesis it appears to be particularly alarming that the soft law and self-regulatory initiatives "considered little societal knowledge (...) and were rarely subject to external evaluation, testing, and improvement" (Kurath 2009: 101) and, similarly, most of the public engagement projects were still shaped by "the notion of a boundary separating science and the public into two societal actors on either side of an expert/lay divide" (Kurath 2009: 101-102).

These findings seem to explicitly contradict or even refute one of the core assumptions of the mode 2 thesis: the assertion of a shift from the production of scientifically reliable to socially robust knowledge. The latter is characterised by Nowotny et al. (2001: 167), besides other criteria, as "infiltrated and improved by social knowledge" and subject to frequent testing,

feedback and evaluation by a variety of actors. I would, however, suggest that what is undermined by Kurath's findings is first and foremost a certain evolutionist interpretation of changes in the relationships of science and society, of expert and lay actors. This reading which apparently is supported by the mode 2 authors themselves relies on the *co-evolution* of "mode 2 science" and "mode 2 society" (Nowotny et al. 2001: 30–49) which is held to result in convergent trends within the two spheres. It thus underestimates the essentially political, i.e. contingent and contested nature of new modes of producing and evaluating (scientific) knowledge.¹ If one abandons the questionable background assumptions of co-evolutionary "coincidences and correspondences" (Nowotny et al. 2001: 30) between science and society, one gets a more differentiated understanding of the shift from reliable to socially robust knowledge and its limitations. It becomes clear then that the extent to which environmental and consumer organizations or "ordinary" citizens are involved in the production and assessment of knowledge primarily depends on the openness of institutional settings and policy arenas as well as on the power relations of different actor groups. Thus, even in the field of NST, where upstream public engagement recently became "a fashionable term in science communication" (Kurath 2009: 89), it seems to be the rule rather than the exception that established actors only pay lip service to the rhetoric of public participation. Nevertheless, Kurath's analysis also shows that some of the employed governance mechanisms, mainly in the UK, actually did "provide a substantial level of exchange and mutual learning" (Kurath 2009: 101). In addition, the fact that – after the GMO disaster in Europe – governments in almost all Western countries feel obliged to adopt at least

¹ See for more detailed criticisms of this point Pestre 2003; Wehling 2006a.

the rhetoric of dialogue and public engagement in their NST programmes is striking. This exemplarily highlights that the legitimation and acceptability of new science and emerging technologies become increasingly dependent on wider social processes in which a great variety of actors potentially play an important role.² Since the concept of socially robust knowledge reflects such dynamics, it proves to be a useful heuristic and analytical tool to study how new constellations of social actors emerge in relation to the production of knowledge.

But the extent to which scientific knowledge is in fact "infiltrated" by social knowledge and subject to external evaluation cannot be not predetermined on a theoretical level but has to be established empirically. Kurath's study thus confirms the heuristic fruitfulness of a central concept of the mode 2 thesis while her findings simultaneously challenge an interpretation of this thesis in terms of an evolutionary master-trend from mode 1 to mode 2, from reliable to socially robust knowledge.

Rectifying mode 2 with Lumann?

Janus Hansen raises two more theoretically demanding objections to the mode 2 thesis. He, firstly, questions the assumption of a convergent and homogeneous transformation of all modern societies towards "mode 2 societies", an assumption which according to Hansen is at least implicitly suggested by the work of Nowotny, Gibbons and colleagues. He rightly asks "how this implicit assumption of convergence can be transformed from

² Beyond the massive social conflicts over GMOs, there are indeed many more examples of the involvement of social actors in the production and assessment of (scientific) knowledge. An illuminating case in point is the engagement of patients' associations and health movements in medical research; see for instance Epstein 1996; Rabeharisoa/Callon 2002; Brown 2007; McCormick 2009, and for an overview Epstein 2008.

a conceptual a priori into a question suitable for theoretically grounded, empirical examination" (Hansen 2009: 72) and argues for comparative, especially cross-national research in order to account for variations in different social contexts.³ It should be clear from my comments on Kurath's paper as well as my earlier criticism of the model of co-evolution underlying "Re-Thinking Science" (Wehling 2006a) that I largely agree with Hansen's suggestion. Likewise, I have little doubt that the concept of "political culture" recently re-adopted by Sheila Jasanoff might prove fruitful in order to capture variations in the ways different societies deal with the challenges posed by scientific knowledge and novel technologies (see Hansen 2009: 79-81). I would, however, like to add one qualification: cross-national comparisons will certainly remain important but no longer seem sufficient to fully understand the variety of forms in which knowledge is produced, legitimized and evaluated in different cultural and political contexts. This is due not only to the diminishing influence of nation-states on globalizing economies and sciences, but also to the fact that international or transnational institutions play an increasingly important role in shaping research and innovation policies and in regulating science and technology.⁴ Cross-national comparisons therefore have to be complemented with comparative research into the institutional cultures of different transnational organizations as well as with what I would term "cross-technological" comparisons. Nanotechnology, for instance, is framed and institutionally

³ One could reasonably argue that this is exactly what Kurath does in her paper. I presume, however, that Hansen argues for more detailed, qualitative research than Kurath's rating of social robustness along a numerical classification.

⁴ Among the sample of 20 nanotechnology governance projects analyzed by Kurath five are launched by supranational (EU) or international bodies (OECD) and another five by private actors.

dealt with similarly in many countries, but quite differently from other technologies such as agrobiotechnology or human genetics and biomedicine.

Hansen's second criticism of the mode 2 concept is in my view much less convincing than his call for comparative research. Opposing the mode 2 claims of "dissolving boundaries between science and society" or even societal de-differentiation, Hansen resorts to Luhmann's theory of social systems with a twofold aim: On the one hand, he adheres to "socially significant distinctions" which, according to Hansen (2009: 74) "should not be overlooked or abandoned for both analytical and normative reasons", namely the distinctions between science as a functionally specialised (sub-)system and society or other subsystems such as politics or economy. As is well known, according to Luhmann, science constitutes an autonomous subsystem by exclusively referring in its communicative operations to the binary distinction of true vs. false. On the other hand, Hansen calls for greater attention to the differences between "two levels of social reality", namely science as a subsystem of society and organizations such as universities or industrial R&D departments which operate with reference to more than one subsystemic code (Hansen 2009: 76). While it is certainly true that in "New Production of Knowledge" and "Re-Thinking Society" de-differentiation often is too hastily proclaimed and levels of analysis are not clearly separated, I have serious doubts whether Luhmann's theory provides a perspective to adequately capture the complex and flexible relations of science to the state, the economy, the media and the public in contemporary societies. I rather suspect that systems theory draws too static a picture of science as a self-referential communication system the "core" of which (the true-false distinction) is by definition immune to transformations.

Although this point would certainly deserve much more detailed elabora-

tion, I can only very briefly sketch the argument here. Even if one admits that scientific communication “in the final instance” (Hansen 2009: 75) recurs upon a distinction between true and false, one should acknowledge that this conception of scientific communication is equally restrictive and selective. In particular, it remains un-specific with regard to a great number of issues which are exceedingly important both for the dynamics of science and the relations of science and society but cannot be meaningfully expressed in terms of true or false – and, at the same time, offer various opportunities for the engagement of non-scientific actors. A pertinent example is the choice of research questions and priorities which obviously cannot be judged as true or false but merely as more or less interesting, promising or relevant. Therefore it is hardly surprising that a broad range of actors (from politics, economy, civil society and the like) strive to influence, often successfully, the research agenda of science. A second case in point is the scientific creation and subsequent diffusion of new entities such as GMOs, embryonic stem-cells, nanoparticles or human-animal chimaeras. Again, the question is not whether these entities are “true” or “false” but whether it is considered acceptable, in terms of risk or ethical justification, to create, utilize and release such entities. And again, social actors massively intervene in discussions on such issues, as the fierce conflicts over agrobiotechnology or stem cell research show. Further examples are the debates on unknown and unforeseeable risks which result in a remarkable “politicization of non-knowledge” (cf. Wehling 2006b; Bösch et al. 2010) or conflicts over the design of clinical drug tests and safety research on GMOs. In all these cases, important areas of scientific communication (or scientific practice, as I would prefer to say) are (potentially) opened to negotiations with a variety of actors, thus

confirming the heuristic and analytical relevance of concepts such as “socially robust knowledge” or “socially distributed expertise”. Yet, what is not contested in all these cases is that scientific communication is (or should be) about truth; what *is* debated and transformed, however, are the social contexts and the ways in which questions of true or false are addressed.

To put the argument briefly: With regard to the relations between society and science and to emerging new modes of knowledge production in contemporary modernity, the question of whether or not de-differentiation occurs on the very general level of binary codes of communication is less important than most Luhmannians as well as many of their critics usually believe. Instead, the focus on functional differentiation or de-differentiation tends to distract our attention from the far more significant developments on the “lower” levels of social reality. Thus the occasional talk of de-differentiation in the work of Nowotny and colleagues is sociologically less informative than the many examples they give of how the institutionally fixed and stabilized “boundaries” between science and society are contested, permeated, transgressed, and reconstructed.⁵

Conclusion

Both papers inspiringly contribute to renewing the debates on the mode 2 thesis. They do not only point to its limitations but also sketch out promising perspectives to overcome some of these limitations, for instance by comparative research focusing on how the supposedly new relations between science and society differ across national, cultural, or institutional contexts. I suggest to conclude that the

⁵ I agree with Hansen that the differences between science (or scientific practices) and, for instance, economic or political action are also normatively significant; yet it is far from self-evident what practical consequences should be drawn from this fact.

mode 2 thesis should be understood and used as a “tool-box” of inspiring and sensitizing concepts (such as “socially robust knowledge”) rather than as a (sociological) theory of “mode 2 science” and “mode 2 society”. Nevertheless, contrary to Hansen, I do not see the need nor the benefit of remedying the weaknesses of the mode 2 thesis by resorting to Luhmann’s systems theory, for this theory with its focus on the utterly abstract distinction of true and false has little to offer to adequately understand those new modes of interaction between science and society to which the mode 2 thesis has successfully drawn our attention.

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