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Scientification of politics or politicization of science? Traditionalist science-policy discourse and its quarrels with Mode 2 epistemology

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> What is 'politics' for the productive class becomes 'rationality' for the intellectual class. What is strange is that some Marxists believe 'rationality' to be superior to 'politics', ideological abstraction superior to economic concreteness. (Gramsci, 1992, p. 231)

1. *Introduction*

The recent claims about new modes of knowledge production presented as the Mode 2 thesis have divided the science policy community. This article sets out to outline and critique a number of counter claims made recently, especially by Weingart (1997), as to the lack of validity of certain Mode 2 assumptions. The article systematically goes through and interrogates a wide variety of recently publicized 'Mode 2 critiques', through a close reading of Weingart's original article. The article concludes that most of these critiques have been based on a conception of science and policy as epistemically dichotomized, thus begging the question of the possibility of a socially 'atomized' scientific community, as well as running against a plethora of STS research.

2. *Background*

A high level of public policy dependence on scientific knowledge may be considered a defining feature of late twentieth-century science-society relations. Lately, science policy scholars have paid increased attention to the examination of the implications of this dependence for the epistemology of scientific knowledge. Several observers have noted that this relation has given rise to new forms of knowledge production and according to a number of recent contributions to this discussion, the science policy literature is now rife with new descriptive terminologies for this phenomenon (Elzinga, 1997). Post-normal science (Funtowicz and Ravetz, 1992), Mode 2 (Gibbons *et al.*, 1994) post-academic science (Ziman, 1996), and trans-science (Weinberg, 1972) are but some of

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these new terms. In particular Weingart reflects on this debate in the article 'From 'finalization' to 'mode 2': old wine in new bottles?' in a recent issue of *Social Science Information* (Weingart, 1997). He raises a number of important questions about the nature of the claim that there has been a shift to a new mode of knowledge production, its empirical substance and import for the epistemology of scientific knowledge. The present article sets out to defend and elucidate, on a meta-theoretical level, the claims made within the new production of knowledge theses (i.e. Mode 2) through a response to Weingart's critique of the same. Weingart's article seems especially suited as a point of departure for the present argument as it broadly represents some of the most important counter claims made to Mode 2 arguments, while at the same time attempting an epistemological grounding.

Weingart's claims may be summarized as follows:

The theses about new modes of knowledge production (Mode 2):

- Are revisions of previous exaggerations advanced in the 'finalization' thesis by Böhme, Van den Daele and Krohn (1973), and Schäffer (1983). Moreover, the authors of these new (Mode 2) claims are replacing the exaggerations of the finalization thesis with opposite 'excesses', and have failed to acknowledge the priority of the 'finalization' thesis (Weingart, 1997);
- Do not provide a framework that is generalizable to science as a whole, but should be limited to the domain of the natural sciences and within this domain, to science for policy e.g. climate change research, meteorology etc;
- Exaggerate the independence of transdisciplinarity. Even research within inter-disciplinary research projects has its reference point in disciplines.
- Are in part founded on the erroneous assumption that institutional changes in the context of knowledge production can effect epistemological changes. Contra the new modes of knowledge production theses, there is no fundamental change in epistemology. Put differently, science influences society but science itself remains a closed entity uninfluenced by social factors;
- Can be reformulated as the interaction between science and policy, where the two 'republics' retain functional and structural integrity.

Since this article starts out from a fairly advanced understanding of the debate, the reader will find it useful, if not necessary, to return to these points throughout the reading of the text. This article responds to the points made by Weingart with the following arguments:

- The debate on the new modes of knowledge production in science is a highly politicized one which must be understood in the context of the broader discussion about the transformation of the university system;
- The view of science in society implied in Weingart's analysis of modes of knowledge production and the post-normal science theses is deeply rooted in an understanding of science as an activity that influences society, but is itself unaffected by social factors. Put differently, Weingart's system of science is an epistemically privileged activity guided only by the search for truth. It is only those areas of science that have not yet managed to free their objects of inquiry from value laden considerations that are subject to societal influence;
- The internal/external, science/policy dichotomies that issue from the above stand in stark opposition to other findings in recent social studies of science. This body of

work shows science to be a social process whose resultant knowledge claims may be understood as a product of the mutual interplay of institutional, epistemic, social and other forces.

In order to demonstrate the above we shall now go through and respond to Weingart's main arguments as outlined above.

3. *The politics of the dispute*

Weingart begins by taking to task the proponents of the arguments about new modes of knowledge production for failing to acknowledge the priority of the 'finalization' thesis advanced by Böhme, Van den Daele and Krohn (1973), and Schäffer (1983). This charge, as any graduate student would know, is a grave one but nevertheless has been a complaint in science that may be traced at least as far back as the quarrels between Newton and Hooke at Royal Society meetings a couple centuries ago. Moreover, it is part of the received wisdom in the community of science studies scholars that academic discourse develops incrementally most of the time, often in the form of continuous 'rediscoveries' (or 'reformulations') of particularly useful ideas. The intellectual pre-history of the now so popular concepts of Mode 2 and post-normal science are certainly no exceptions in this regard, why should they be?

Having exhausted the priority issue, Weingart turns to what in our view is one of the more interesting parts of his argument. This is the problem of the differential reception of the 'finalization' and the new modes of knowledge production theses. It would seem that the 'finalization' thesis was bitterly opposed by politicians and academics alike, whereas the new modes of knowledge production claims are being embraced with enthusiasm. It seems to us that this reading of the situation says more about Weingart's love for dichotomies than it does about the academic response to the Mode 2 thesis. The literature available thus far, as well as some of that being planned, shows a mixed record with large numbers of academics lining up on either side of the issue (Fuller, 1997; Rip and Van Der Meulen, 1996; Shils, 1992; Slaughter and Leslie, 1997).

Nevertheless, we concede Weingart's point that in both instances the reception of these claims is in part due to their real or imagined normative implications for science policy and the public legitimation of science. Although the main issue for Weingart is epistemology and not politics, we would like to dwell a little more on the political aspects of the issue. The reason for this is that we think that the changing socio-economic and political context of science is a critical issue in the debate.

Weingart argues that the new claims about the implications of the science-policy interaction for the production of knowledge are loaded with both epistemological and political content. We would like to extend this claim and argue that it is also valid for understanding Weingart's position in the debate, despite his attempts to dress it in epistemological guise. The entire debate about new forms of knowledge production (including this article) is backdropped by a tense political climate in the universities of Europe and North America. This tension is in part due to the fact that university research and education increasingly have to justify their right to exist in terms of immediate social and political utility.

After decades of science policy rhetoric that alternatively depicted science as both forces of production and cultural artefact, the idea that public spending on science has to be justified in terms of the cost and benefit by measure of economic growth is finally

here. This entails that society now feels entitled to pose the following questions to science: 'Who should be doing what, in what setting, to what end(s), for whose benefit and at what cost to whom?' (Fuller, 1997). No academic area that incurs a cost on society as a whole and on academe in particular in the sense that it diverges money from policy related Mode 2 fields could be said to stand outside 'the immediate connection to social values' (Weingart, 1997). In times when large parts of academe are experiencing budget cut-backs and 'legitimacy crises' it is irresponsible to propose that high energy physics and palaeontology do not, or should not, concern citizens and politicians. These fields are intermeshed in the Mode 2 problematique just as much as for instance environmental risk research.

The Mode 2 thesis may be read as an attempt on the part of one sector of the scientific community to respond to these issues. That this response is dictated by the situatedness of Mode 2 researchers in contract research is so obvious that it is unworthy of mention. The truth is that this sector of the scientific community has an immediate vested interest in depicting science as an epistemically privileged activity that is both shaped by, and responsive to, social forces. Contract researchers have been marginalized in the university for some time now, the new political environment is a window of opportunity to publicize their existence and make known that they have always been meeting the criteria that now seem to be applicable to the other parts of the scientific establishment. Like Weingart, they too are dressing their political claims in epistemological clothing. Thus, instead of contract research being depicted as a growing phenomenon within the university system, the Mode 2 discussion gives the impression that traditional tenured disciplinary research is now a silent backwater.

This is as Weingart quite rightly puts it an exaggeration, the relation between disciplinary and transdisciplinary research is still a grey area. However, attempts to make claims of independence from traditional disciplines for transdisciplinary inquiry predate even the 'finalization' thesis. Strangely enough one of the reasons for this tendency is that the competition for scarce resources within academe encourages boundary management both within and across disciplines. This brings us to the second and third of Weingart's claims, i.e. the issue of whether institutional changes in science can in fact induce epistemological changes.

4. *Is the epistemology of science immune from institutional influences?*

The argument that the Mode 2 thesis is not 'generalizable to science as a whole' builds on a narrow view of the organizational and epistemological multitude of academe. Moreover, a traditional reductionist understanding of knowledge production creeps into Weingart's (1997) argument as he disputes whether Mode 2 really leads to any 'fundamental change in epistemology'. What constitutes a 'fundamental change', and even if this could be defined, a fundamental change in *what* one may ask? What is the name of this epistemology that obviously demands the generalizability of ideas to 'science as a whole' for their validity to be accepted, and at the same time is so homogeneously constructed that it can be reformed in a fundamental way?

If one looks at the everyday world of the research community, one finds not a homogenous but a diverse group constituted by tenured staff, contract researchers and research students to mention only a few. This diversity is also present at the level of evaluation criteria for scientific work. Researchers are not united in their support for any one particular set of criteria on which to judge research. A large body of work

including that from the feminist (Hawkesworth, 1989), and STS traditions (Shapin, 1994) has repeatedly demonstrated that the position researchers take on evaluative criteria is bound up with their epistemological commitments, which is in turn based on their experiences within and outside of research. These again may be many and varied. However, this work is probably what Weingart (1997) alludes to when he states that Mode 2 claims are "motivated by ideas of politically 'more correct' science".

Let us examine closer this claim about the immunity of epistemology from changes in the institutional arrangements of knowledge production. What is this epistemology to which Weingart refers? Is it some meta-theoretical device that is actually formulated and explicitly embraced by the research community, or is it implicitly formulated through the actions of the actors in this community (hence to be reformulated when community structure changes)? It seems that Weingart has already answered these questions for us. Epistemology of science lies homogeneously still, a dense mass of unchangeable postulates implicitly embraced by the whole of the research community, hovering silently outside of human action, yet somehow reproducing this action in the image of science itself.

Weingart (1997) asks for empirical substantiation of the proposition that 'institutional change in science will also entail epistemological changes' despite the fact that we have only his vehement insistence that there has been no 'fundamental change' as substantiation of the counterfactual. The term institution is a slippery one and in challenging Weingart here we risk being accused of attributing to him an understanding of institution which he does not hold. Recognizing this we would follow in Weingart's footsteps and not define what we mean by institution. However, we would like to point to some work that at least in our understanding of the term 'institution' provides substantiation for the claim that institutional changes do affect epistemological ones.

Hence we would like to draw the reader's attention to the debate in American sociology that has spanned more than four decades, about the role of funding in promoting a bias towards quantitative methods in American sociology (Alpert, 1954, 1955; Fisher, 1993). Focusing on the National Science Foundation (NSF), which supported basic research, Alpert contends that the initial mandate of the Foundation did not include social science. Moves were made to introduce elements of social science (Alpert, 1954, 1955), but the external political climate continued to encourage caution, and the internal climate was dominated by natural science. As a result social scientists seeking NSF support adopted 'a strategy of protective coloration, of allying one's cause with stronger others…' (Platt, 1996). This, together with the political need to differentiate social science from socialism or social reform (with which it was often confused), led to an emphasis on 'the similarities between social and 'natural' science by focusing on methods of inquiry…' (Riecken, 1983). Although it is arguable whether this was deliberate agency policy it did constitute a perceived condition of survival, because the alternative was seen as no NSF money for social science (Platt, 1996). The net result was undeniably that those social scientists that wanted NSF funding were constrained in their choice of, and more important *development* of, methodology. In our view, this constitutes evidence for institutional shaping of epistemology.

Further empirical support for institutional shaping of epistemology may be found in Rappert's (1995, 1997) empirical survey of research projects funded by Britain's Economic and Social Research Council (ESRC). This survey showed that research practices are affected by requirements of user relevance. All the projects surveyed by Rappert met the criteria of user relevance. Contra to Weingart's belief that Mode 2 research tends to be confined to sectors such as health, energy, environment and

technology policy, the research surveyed by Rappert covers a surprisingly diverse range of themes and the involved researchers came from disciplines ranging from mathematics to history.

An example cited in Fuller (1997) neatly demonstrates how difficult it is to attribute priority to institutional factors or epistemological prerequisites as the driving forces behind changes in science. According to Fuller, one may explain the use of equilibrium models in economics as a case of interdisciplinary borrowing from physics. However, the accuracy of this account is difficult to establish since an adverse job market for physicists and theoretically based engineers also played a role in inducing migration from these disciplines to economics. Nowotny's alleged argument (unsubstantiated by reference in Weingart, 1997) in that every institutional change induces epistemological changes would obviously be exaggerated, but to vehemently insist on the other extreme as Weingart does is just as extreme. Once we apply empirical evidence to the problem as Weingart suggests, we converge on an already established finding in STS, i.e. that there is a mutual interplay between institutional and epistemological factors in knowledge production.

5. *The autonomy of science*

Rather than deepening the inquiry on how corporate interest is 'corrupting' science, Weingart seems more interested in turning the question around and asking how science has influenced other parts of society. More specifically, Weingart wants to know whether the ever growing scientific component in society at large stems from a 'genuine production of knowledge' (whatever that is) or merely some form of knowledge 'processing and transfer' (we are still puzzled as to the meaning of this). The old analytical dichotomies between science and non-science, between science and society, dichotomies that STS scholars have attempted to inform and break down for at least two decades now, are obviously alive and well in the hearts of some members of the community. Not only that. Weingart seems to have loaded his analytical dice to the benefit of a 'science prevails' view of the picture. This is where the buck stops. Science should not, *could not*, be 'corrupted' by society, only society by science.

In the same vein, Weingart responds to the Mode 2 claim about knowledge in the context of application with the following remark:

> The new 'fundamental problems' which arise from contexts of application by way of feedback processes can have a constitutive and sustained impact on knowledge-production only if they are subject to the differentiated communication on 'truth' (Weingart, 1997, p. 601).

But what of the research object as such? Is that not created and understood trans-disciplinarily (global change, technological assessment etc.), and constituted exactly in the funding process? Why does Weingart wish to divorce the *formulation* of research problems from the process of doing research, creating scientific results etc., and by extension divorce problem selection from the constitution of disciplines and their intellectual products? Not only does this represent a nominalistic interpretation of academic disciplines, but it also implies a distinction that only holds water insofar as one is prepared to accept a slightly modified version of the context of discovery-context of justification thesis (another old dichotomy). This dichotomy, here implicitly embraced by Weingart but originally launched by the logical positivist Hans Reichenbach, probably represents what was the greatest bottleneck for a humanised, politicized and 'embodied' conception of science.

Weingart (1997) contends that 'changes of disciplines and the emergence of new ones are generated in the system of science'. Yet the history of academe shows several cases of new disciplines being formed specifically in response to political or social concerns. For instance, the formation of development studies as an area of study that exists qua transdisciplinary entity and as a sub-component of other disciplines such as economics and sociology was a response to a post World War II concern with the economic welfare of the newly decolonized nations.

Instead of fighting to retain this internalist/externalist typology for understanding the interaction of science and society, we should be moving to a more dynamic set of categories. A more fruitful approach for understanding science in society would be to expand the concept of science to allow us to track the influence of regulatives (economics, politics, sign of the times etc.) on the evolution of disciplines. In Weingart's view, science is a self-contained system of homogeneously defined rules for proper epistemological conduct, and in that case an entity clearly outside of the interest of a social study of science. If, on the other hand, the system of science were to be treated as an amorphous set of practices that constantly hand over the responsibility of defining 'proper conduct' to political and capital interest, then the 'system of science' would certainly lend itself to fruitful social analysis and interpretation. It is only sad that the standpoint of Weingart's, and many with him, does not allow for such an analysis to be carried out to the fullest.

6. *Mode 2 claims as science-policy interaction*

There exists a severe problem with Weingart's reformulation of Mode 2 into a form of interaction between science and politics, that is a scientification of politics, a politicization of science, and a mediatization of their relationship. The problem is two-fold: (1) Mode 2 obviously does not equate politics with industry or capital interest; and (2) Gibbons' *et al*. emphasis on the latter is exactly that which is brought to the forefront and given most weight in their assessment of current trends in S-T policy. It would therefore be presumptuous of the reader to assume that a reduction of Gibbons' *et al*. argument into one of science's relative position *vis a vis* politics or policy is a valid one, a step Weingart is more than willing to take with or without empirical backing. There is in fact (today) no *a priori* reason for equating politics with capital, especially not in the light of the ongoing 'triple helix' discussion, where representatives from both camps quite visibly come out as representing just that, two camps, two strains of interest.

A recurrent theme in Weingart's argument is that the epistemological changes in science suggested by Gibbons *et al*. (1994) are not epistemological at all, but rather institutional. As examples of 'new' epistemologies potentially replacing old ones Weingart mentions physiocentrism, feminism and ethnocentrism. However, he hastens to bring to our attention that:

> Developments of this kind are limited to subject matters which are potentially value-laden: the human and social sciences, technical sciences and the technological post-paradigmatic natural sciences as well as all research fields in which different research lines merge to heterogeneous complexity' (Krohn, 1997, p. 21).

In other words these developments may be found in almost all of science except for a very small number of areas which Weingart would have us believe constitute the 'untouched epistemological core' or gold standard by which one should judge the entire system of science.

Epistemology, if one follows Weingart in divorcing it from institutional arrangements, would be reduced to the simple issue of explicating criteria for valid knowledge i.e. by formulating inference rules and rules for confirmation. The present authors have never, within the framework of even remotely strategic science, seen such criteria explicitly formulated in terms of Weingart's above-mentioned -isms.

The elbowroom of science is certainly shrinking due to renegotiations of the science-society contract, i.e. a 'politicization of science' in Weingart's terms. What fails to be apparent however is the counter-process to this depicted by Weingart, i.e. a simultaneous 'scientification of politics' where 'the spirit of truth seeking science' comes to influence 'virtually all other functional subsystems', and where systematic and 'certified' knowledge will prevail. Counter to Weingart's suggestions that the depicted counter-process is strengthening science's traditional identity, the present authors would like to suggest another reading. Weingart's 'scientification of politics' could in fact be read as being nothing more than politics co-opting the language of science, maybe even together with some of the 'certified' actors of science. In this process, political interest would certainly not be likely to internalize the ethos or epistemologies of science, rather this would be just another aspect of Weingart's politicization of science, or what some authors have referred to as Mode 2.

Weingart also seems to have misunderstood the relation between the 'triple helix' discussion and the Mode 2 thesis. He argues that 'a development running seemingly counter to 'Mode 2' is appearing: industries and universities are forming ''strategic alliances'''. These strategic alliances have been in operation for at least a decade now and have not been confined to the US but are also found in countries such as Sweden. We fail to understand how these developments could be perceived as running counter to Mode 2. In our view, the 'Triple Helix' thesis is focused on the organisational context of Mode 2 research. Thus, the phenomenon of universities providing venture capital – an occurrence that is already present in Europe – is not divorced from Mode 2, on the contrary it is one of the sites at which Mode 2 research resides. Weingart (1997) again prefers his phenomena neatly compartmentalised and wants to see the 'Triple Helix' as part of a 'globalization' trend, and Mode 2 as something else. Perhaps, it is this preference for neat little compartments rather than seeing the connections beyond the fancy words that has given opponents of the new production of knowledge theses (including Weingart) the impression that there is a proliferation of 'vague metaphors' and 'fads and fancies' in the contemporary literature.

7. *Conclusion*

In conclusion, we would like readers to cast their minds back some 29 years ago when a book entitled *Scientific Knowledge and its Social Problems* was published (Ravetz, 1971). The arguments put forward in that text would have met with Weingart's approval in that the author tried to demonstrate the workings of science *qua* science. Along the way Ravetz lamented the corruption of science by political and capital interests, however, he maintained that the internal workings of science itself were more or less free from these influences. Two decades later Jerome Ravetz and Silvio Funtowicz, put forward a radically different thesis; the now well established notion of post-normal science (Funtowicz and Ravetz, 1992). It is ironical that Ravetz's revision of his 1971 view of science should, in 1997, be critiqued on grounds that he himself would have favoured more than two decades ago. However, it is not unexpected given that the most common

trajectory for the reception of new ideas in academe may be summarized as follows. Critics' first response: new idea does not apply at all, no support in the literature etc. Second response: idea has been formulated before (usually by some colleague of the critic). Third response: the text that has 'said it before' is invoked to show how the new idea is wrongheaded. Academe at large is pervasively politicized, the multitude of ways in which this manifests itself, as well as its impact on modes of knowledge production has yet to be addressed by traditional science-policy discourse.

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