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Fashions, Lock-ins and the Heterogeneity of Knowledge Production

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Buzz-words and storylines

Change must be in the air, given the popularity of phrases such as Mode 2 knowledge production (Gibbons *et al.*, 1994), the second academic revolution (Etzkowitz, 1990, 1998) and triple helix (Etzkowitz and Leydesdorff, 1997). However correct the diagnoses of the state of knowledge production implied in these phrases might be, the phrases are also rhetorical ploys. They posit a dichotomous history, which emphasizes that we are entering a new phase, or era, that is very different from what we had before. It is argued that Mode 1 is now obsolete, academics should be entrepreneurs and interactions between universities, industries and government agencies are to be welcomed. In our modern world, this message is normatively loaded. Actors feel the normative pressure of the changes labelled in this way, and react conservatively, opportunistically or by embracing the new mode. Analysts of science policy and sociologists of science respond to such rhetorics critically, and emphasize continuity, either in what is happening ('there is no dichotomy') or in the analysis ('we have identified this before': see Weingart, 1997), or in both. Again, while they may well be right, such a response is a rhetorical ploy itself, an alternative storyline.

In this chapter I want to do three things. First, I try to understand the interest in Mode 2 and other labels as a fashion in science policy, and how it may, or may not, reflect transformations in science organization and science policy.

Second, I shall propose an alternative storyline myself, one in which changes in science occur all the time, but also lock-ins, linked to stable configurations of scientific establishments and the societal embedding of science. The Gibbons *et al.* notion of an academic-disciplinary Mode 1 refers to such a lock-in. There is, however, more to knowledge production than the lock-in. In fact, features of Mode 2 – for example, 'discovery in the context of application', have been the rule rather than the exception. In the period of Mode 1, when the modern research system came into its own, examples abound: chemistry and pharmacy, electronics and solid state science, were progressing as much outside academe as within it, and with lots of 'triple helix' interactions. When a stable configuration threatens to break down, actors as well as analysts have opportunities to outline a brave new world. The game of fashionable labelling is not an innocent one: there is a danger of a too rapid lock-in, and this is what may happen at present, with an emerging regime of strategic science.

That this is a danger – this is my third point – derives from my diagnosis, which holds that heterogeneity of knowledge production is not a feature unique to Mode 2, but may be found in modes of knowledge production in all ages. Thus, there is an argument to embrace heterogeneity, rather than reduce it. This proposal is particularly important when one considers the situation of non-Western and/or multicultural, newly 'scientizing' countries.

Fashions in science policy and potential lock-ins

If Mode 2, with its emphasis on non-disciplinarity and discovery in the context of application, has been there all the time, why the sudden interest? Taking a page out of the (severely underdeveloped) sociology of fashions in science policy, I would argue that the Mode 2 thesis has become so popular (at least with science policy makers) because: (a) it *names* a feature of science which has become more relevant; (b) it creates an occasion for policy making; and (c) it feeds the need for mimesis in science policy making. There is an interesting precedent: the emergence of 'big science' as a label around 1960, and its rapid rise to become a fashionable concern, as well a continuing feature of the organization of science and science policy decision making – witness the present Big Science Forum consultations organized by the OECD.

The recent interest in the 'relevance' of science and the emergence of the category of 'strategic research' in science policy making have their fashionable aspects as well. To bring out the similarities, I will discuss big science at some length, and then position the discussion of the relevance of science and indicate what the changes are, as well as their limitations.

Big science

According to Derek De Solla Price, the term 'big science' was coined in 1961 by Alvin Weinberg (then Director of the US Atomic Energy Agency and member of the President's Science Advisory Council).[1] Whoever first thought of the term, it clearly captured something that was in the air. De Solla Price's book *Little Science, Big Science* was bedside reading for the emerging group of science policy functionaries (often scientists) at the time.

Weinberg and De Solla Price, each in his own way, used the label big science against a background of difficult funding decisions and hard choices in science, a perceived change in the 'style' of doing big science, in contrast to that of little science, and a general transformation of science to 'stable saturation'. When De Solla Price (1963: 31) argued that 'the state called Big Science actually marks the onset of those new conditions that will break the tradition of centuries [of logistic growth]', he was seeing the decisions about large facilities and projects as only one of the locations where the transformation was recognized and articulated.

Thus, big science is a label, and a label that continues to be used because it captures something about a transformation of science and a decision-making problem at the same time. Its prominence derived from a diagnosis of change in science, about which 'something' had to be done. However, the phenomenon of big science, in terms of organization, size and the need for hard choices, had been present before. It is instructive to detail this point further.

The aspect of costs and investments can be put up front: 'Big Science means big machines' (*The Economist*, 1990: 14). The Task Force on Science Policy of the Committee on Science and Technology, US House of Representatives, took this aspect seriously and defined big science as 'facilities constructed since 1920 at a cost of approximately $25 million or more in 1984 Dollars or its equivalent' (Science Policy Study, 1987). There are good reasons for such a definition: large amounts of money and often high political visibility require special efforts to mobilize the necessary resources, and special attempts at justification of the expense.

This kind of big science, however, did not appear on the scene only with high-energy physics facilities in the 1950s. The wind tunnels in the 1930s and the big telescopes that were built from the late nineteenth century on were big science projects in terms of physical size, investment and resource mobilization requirements, but not (at least not at the beginning) in terms of division of labour and team effort. The big expeditions of the nineteenth and early twentieth centuries easily qualify as big science in terms of investment, team effort and resource mobilization, but are not 'big machines'. The Task Force was surprised by how much big science occurred before the era of big science.

What about the organizational aspects? In a study of the development of the space telescope, Smith (1989: 379) defines big science as 'characterised by large multidisciplinary teams, a division of labour, team commitments, agreements and negotiations on common purposes, and hierarchical organisation.' Team research and hierarchical organization on a grand scale are very visible in German research laboratories at the end of the nineteenth century. In fact, this led to concerns about *Grossbetrieb*[2] in science, which was one of the arguments for the establishment of the Nobel Prize, and contributed to its wide appeal:

> the prizes took on symbolic significance because they revived, on the international level, an emphasis on individual achievement that seemed to be receding from the day-to-day experience of national scientific

> communities. Scientists who deplored the deemphasis of the creative efforts of the individual that they felt was the consequence of *Grossbetrieb* and feared that they would be reduced to an anonymous mass of research workers could take heart in the way individual achievement was singled out for reward by the Nobel prizes.
>
> (Crawford, 1984: 205)

The trend towards *Grossbetrieb* had been gathering force in the last decades of the nineteenth century, and had its predecessors in meteorological and geological programmes. Responses to such a trend are always ambivalent, and often delayed. It is not uncommon that irreversible changes in the organization of science (and perhaps the nature of quality in science) have already occurred before practitioners realize it and raise their voices in concern or applause. After a time, a compromise is reached, e.g. by occasionally celebrating individual achievement symbolically, while continuing to press for more resources necessary to outfit the laboratories and support the assistants.

In general, there is a continuity of practices, but also changing circumstances and new concerns. It is the latter consideration, the need to come to terms with what is becoming salient, which explains fashionable interest – in *Grossbetrieb* in science around the end of the nineteenth century, in big science in the early 1960s and now in Mode 2 and other labels which attempt to capture a transformation that is becoming salient.

Responses to present changes

As before, one can see two types of response. One is of science policy makers, science analysts and enterprising scientists embracing the new fashion (in the case of scientists, also as a means to legitimate activities). They can do so with some effect because it is not just a fashion; it is also an attempt to name changing practices and contexts, and to come to terms with them. Chapter 4 in this volume provides some detailed examples. For the Netherlands, we have shown that the earlier champions of relevance of science have in fact become a new elite in the institutions of the national research system (Van der Meulen and Rip, 1998), and this appears to be a general phenomenon in industrialized countries.

The other response to ongoing changes is one of reluctant accommodation, particularly visible with the old elite, the spokespersons for established science. An instructive example is provided by a conference held in October 1994, where 'the world science leaders' met in Jerusalem to discuss strategies for the national support of basic research (see Asher *et al.*, 1995). They were leaders as seen from within the world of science, however, and they evaluated changes in the world 'outside' science as to whether these were threatening to science (as the leaders knew it) or not. They were defensive, but prepared to defend the bastion of science. This attitude is clearly reflected in the following citation taken from the Proceedings of this meeting: 'as research resources tighten, science will have to define and fight for its priorities.' And they conceded that an assessment of science, however distasteful, had to be taken up in earnest: 'if we do not measure ourselves, somebody else will – "upper management," the government, funding agencies, whoever – and they will probably do an even worse job of it.' They locate their discussion against a backdrop of the 'collapse of public and political confidence' in the importance of science (Asher *et al.*, 1995: 19, 217, 13).

The above reflects the concerns of an establishment which is becoming insecure and is trying to overcome its insecurity by reasserting its position and seeking scapegoats (here, in an assumed lack of confidence with its sponsors and with the public). In the case of science establishments, there is the additional possibility of identifying one's own cause with that of rationality and progress, and creating a mythology of battling against the forces of darkness. It is worth noting this last point, because it helps us to understand the age-old concern about a 'flight from reason' and the recent flare-ups and attendant scapegoating between some parts of the natural and social science communities referred to as the Science Wars (Gross and Levitt, 1994; Hilgartner, 1996).

Regime changes and a possible lock-in

The scientific establishment's views and behaviours are shaped by a regime of science organization and science policy which was dominant in the 1950s and 1960s, and is still a strong force in the institutions of science. Funding of basic science without ties attached, under the overall legitimization of *Science as the Endless Frontier* (after Vannevar Bush's 1945 report to the US President),[3] created a macro-protected space for science, as if it were a social contract between science and society (Guston and Kenniston, 1994). Of course, much more was happening than the funding of basic science; but this is a *locus* to identify key rules of the regime that governs expectations and interactions.

Pressures for relevance of scientific research, and, in general, new linkages with, and interference by, the 'outside' world, have opened up the earlier protected space for science. The 1971 Brooks Report is a landmark in the shift toward (civilian) relevance (OECD, 1971). The other key feature is the more critical attitude towards science, in society as well as with politicians (in the USA, this is the continuation of a hallowed Congressional tradition). From the 1980s onwards, one also sees a renewed hope that science and technology will fuel economic growth and help us to understand and solve environmental problems.

The contextual changes, and the uncertain combination of reluctant responses to change and exploitation of new opportunities by (other) scientists, create ambivalence. If these are (even only partially) resolved, a new regime might emerge. In fact, this may well be happening now, and 'strategic science' is an appropriate name for the regime-to-be. This is a label proposed by me as an analyst, but also a diagnosis of the secular changes towards relevance and public scrutiny of science, and of the responses, including mimesis and resource mobilization games.

The idea of 'strategic research' is taken up increasingly, and Irvine and Martin's definition, which locates strategic research at the side of basic research (with 'pure or curiosity-orientated research' as the other sub-division) has become authoritative. They define strategic research as 'Basic research carried out with the expectation that it will produce a broad base of knowledge likely to form the background to the solution of recognised current or future practical problems' (Irvine and Martin, 1984: 4). Scientists have internalized the pressure for relevance, but at the same time have captured it for their own purposes by claiming a division of labour. Typical stories emphasize strategic research as the hero at the core of one or more 'innovation chains', but with a gap in the chain where the switch from open-ended research to implementation would occur.

Thus, strategic science, as a label for the present situation, covers a basic ambivalence (Rip, 1997). On the one hand, it creates a space for scientists to do their own thing, even if constrained by the credibility pressures deriving from promise-requirement cycles (Van Lente and Rip, 1998). On the other hand, it claims a productive relation between science and society, where society is content to accept promises. The label of strategic science can, then, also cover up spurious claims of relevance, not too different from the claims made under the regime of Science as the Endless Frontier.

To assess these developments, three points are important. First, the contrast between fundamental (and scientifically excellent) research on the one hand and relevant research on the other hand is not a principled contrast. It has more to do with the institutional division of labour than with the nature of scientific research. The combination of scientifically excellent and relevant research occurs again and again, in history and in present-day science. This combination is not present in all disciplines and scientific fields in the same way, but it occurs sufficiently often to justify the claim that a new category like strategic research that embraces both is a realistic option.

Second, the reassertion of the purity of science is more dangerous for the long-term development of science than its exploitation in the quest for relevance, and the possible fragmentation of science-as-we-know-it. This is not to say that policy makers and sponsors of science can do as they wish. Socio-cognitive dynamics leading to quality assurance must be maintained and stimulated, but this cannot be achieved by invoking purity and creating a 'reserve' for science. This challenge is addressed in concrete contexts, but there is also another trend. One can see signs of a 'monstrous alliance' between politicians and science policy makers, and the new elite in science promising to contribute to wealth creation as well as sustainability. A new 'reserve' is created, even if the composition of the establishments changes.

In this respect – and this is the third point – it is important (though also ambivalent in terms of effects) that science is under public scrutiny.

While public and political scrutiny may sometimes be disconcerting, it is not really a threat (except for physicists and biologists fighting science wars). New accountability relations and interactions with the public may stabilize. At the moment, one sees scientists (but also funding agencies, which have their own credibility cycles to care for; see Rip, 1994) exploiting opportunities to mobilize resources through press conferences and other publicity efforts. Such activities increase the variety of linkages between science and society, which appears to be a good thing to counteract a too rapid lock-in into a protected regime of strategic science.

In praise of heterogeneity

Why is a variety of linkages so important? Gibbons *et al.* (1994) argue that Mode 2 is characterized by an increasing heterogeneity in the cultivation of science. While they sometimes recognize that Mode 1 will continue (and be important, for example, as one way of implementing quality control), in their more triumphant moments Mode 2 has the future to itself, especially when more and more people listen to their diagnosis.

My concern is with the basic richness of knowledge production, and the way various 'modes' impinge on it. Regimes and modes discipline the richness, unavoidably so, but will (it is hoped) not completely contain it. Thomas Kuhn has highlighted the ambivalent role of intellectual discipline and dogma in science. Michael Polanyi made the same point using an intriguing experience of his own (Polanyi, 1963). I suggest that a similar ambivalence occurs at the meso- and macro-levels of science organization and science policy. This may not always be visible to insiders; in this respect it is useful to try to create some distance, culturally and/or historically.[4] Such a possibly disrespectful, but necessarily hard-nosed, distance is especially important when one considers capacity building. Given the time necessary to build up capacity and competence, one has to ask what kind of science and mode of knowledge production will prevail in 2010, rather than now.

My answer: emphasizing heterogeneity, rather than reducing it, can be seen as a hedging strategy. Under conditions of uncertainty, it is only prudent to maintain variety. Heterogeneity is valuable in its own right, however. In the example of capacity building in knowledge production in South Africa, the two lines of thought come together – with the help of a historical detour through the European Renaissance.

Embracing variety versus controlling the world

A traditional but certainly strong argument for entertaining heterogeneity derives from the importance of novelty creation and the paradoxical situation that one cannot simply organize for it. The challenge is to maintain the richness of knowledge production, as well as its quality. Knowledge production is disciplined by experimental conditions and by ideals of explanation in a scientific community. Scientific revolutions, to use Thomas Kuhn's storyline, break through the bounds set by 'normal' science (Kuhn, 1970). In addition to this general dialectic of continuity and innovation, there is a secular change in knowledge production in Europe and in the Western world: from embracing variety, in what one could call the 'natural history' approach, to an emphasis on restricted, controllable conditions in a laboratory (Rip, 1982). This typically 'Western' science approach has led to major achievements, but on condition that the complexities of the real world could be reduced to something equivalent to the laboratory conditions of scientific experiments.

What we see now is how well intentioned action based on such knowledge can backfire, when the 'restrictions' under which the knowledge is valid do not apply. The messy world strikes back, not out of vengefulness but because the action did not have the requisite variety. We can think of 'Western', i.e. experiment-based, knowledge production as one variety, visible from the sixteenth and seventeenth centuries onward, and becoming dominant with the rise of discipline-oriented scientific professions. Another variety, natural history (observational and embracing the complexities rather than disciplining them in experiments), was kept alive in the exploring approaches, and is coming into its own again in earth and environmental sciences. Measuring, mapping and modelling the world, rather than (re-) constructing it, is the form that the new natural history appears to take. These '3M' approaches are particularly relevant to decision making, which introduces special dynamics, up to advocacy science. The practitioners of the new natural history play increasingly important roles in science.

The melting pot of the Renaissance

Reasoning from the premise that the seventeenth century represented an unprecedented shift in the socio-cognitive history of science, it is important to look more closely into the nature of knowledge production before that time, in the European Renaissance. Whatever else fourteenth- to sixteenth-century Europe was, it was a melting pot. For a birthplace of Western science as we know it, it looked messy, unruly and without clear boundaries between various knowledges. There were the (medieval) universities, and travelling humanists, artists and engineers. There were also almanac makers, astrologers, mountebanks and *ciarlatani* performing tricks at the fairs. Princes and wealthy persons were sought as sponsors, and the scholarly and craft work to be done was defined in terms of their wishes and aspirations, as well as for the market place. One intriguing variety of knowledge production was through so-called 'professors of secrets'. They collected recipes from different crafts and some of their own experience, and sold them on the fairs or to sponsors. The ambivalence in their position is curiously similar to that of biotechnologists and other scientists in commercially important areas. They had to advertise themselves and their knowledge in order to create some visibility. However, at the same time they had to keep their secrets in order to maintain a competitive advantage over other such 'professors' operating on the same market or for the same sponsors (Eamon, 1985).

The so-called scientific revolution of the seventeenth century replaced unruliness with proper procedure (in scientific academies), and started to create boundaries between mechanical philosophy and the crafts. Whether one sees this as an achievement or as dehumanization (Toulmin, 1990), the rationalistic mode of knowledge production which eventually emerged has grown out of the fertile soil of the Renaissance. The richness, variety and openness of knowledge production of the time were important. And I would argue that richness, variety and openness remained, and remain, important as a backdrop to high science, and as a source of renewal. When Gibbons *et al.* identify heterogeneity and transdisciplinarity as features of present-day knowledge production, they should not be taken at their own words, that this is a new Mode 2. It is the 'old' backdrop to whatever is the dominant mode of knowledge production. One might prefer to exploit the potential of this rich, variegated backdrop further, rather than try to specify a new mode.

Capacity building for the twenty-first century

Developing and exploiting this potential raises questions of capacity and competence building. Very visibly so in developing countries, but implicitly as well in developed countries, capacity building (and maintenance) in science is an issue. Capacity built now will become productive in another five to ten years and subsequently mature (and thus introduce inertia). The common mimetic route is to define the nature of capacity building in terms of what is now seen as important. This may well be a recipe for becoming obsolete before one's time.

In countries like the Republic of South Africa, this question is particularly pressing. There is a tendency to take top scientific institutions in the USA, and sometimes also in Europe and Japan, as the exemplars. But the world (of science, and more generally) may well evolve in such a way that present-day exemplars will be left behind. So developing countries should set their sights on what is important in 2010, rather than what appears to be important now – however difficult this will be politically.

Capacity building for 2010 is difficult politically, because the longer term tends to lose out to the short term. The substantial difficulty relates to the unpredictability of the evolution of knowledge production and research institutions. One way to address this difficulty is to nurture variety, and monitor developments so as to be able to make wise choices – a reflexive version of the European Renaissance.

This line of thinking links up with the idea of African Renaissance, without bowing to its ideological thrust. The idea of an African Renaissance has a certain duality to it: on the one hand, it emphasizes openness to African inputs which introduce variety into the research system. On the other hand, particular kinds of variety and 'indigenous' ways of knowledge production are given priority. 'Africa' becomes a constraint rather than an opportunity. This tension is visible in otherwise sensible diagnoses of African universities (Adedeji, 1998), and is all the more striking because of the modernist thrust of the argument in such articles. The reference to the Renaissance, with its richness, openness and variety, should be taken as more important than its being specifically African. The latter might create a lock-in which will be, in its own way, as limiting as the seventeenth-century scientific revolution. It is a new Renaissance, rather than an African Renaissance, which must shape knowledge production in the twenty-first century.[5]

Conclusion

By creating an alternative (analyst's) storyline (empirically grounded, but still a storyline in terms of its implications for the future), I have created distance from the Mode 2 argument and other dichotomous histories. This distance is important, not because of their being presently fashionable – fashions are there for a reason, and have effects – but because a non-triumphant version of the Mode 2 message must be developed, grounded in history and sociology of science. It is in this way that one can transcend the monolithic histories of maintaining the status quo.

My storyline emphasized a three-level dynamic: (a) varying, heterogeneous practices of knowledge production; (b) meso-level organizations, with their inertia and openings for change; and (c) possible lock-ins when interactions with environments are (re-) structured as a protected space. I then argued for opening up such protected spaces, or at least being reflexive about them and keeping their boundaries permeable.

There are other attempts to open up a space for science policy articulation linked to diagnoses of ongoing evolution/transformation, without immediately conferring one or another label. But to communicate one apparently needs labels, whether a numbered mode or an alternative notion of 'post-normal science' (Ravetz, 1992).

The reader need not share my concern about too rapid a lock-in, but the analysis I offer of trends and indications of a lock-in remains important. It depends on the nature of such a lock-in whether one should be concerned or not. Heterogeneity (disturbing and innovative) is important, and that is why I have (diffidently, because as an outsider) criticized the African Renaissance movement for its monolithic character. The melting pot of Renaissance Europe, out of which modern science emerged, appears as important as the academies and societies of the incipient institutionalization of science in the seventeenth and eighteenth centuries. Present-day multiculturalism, if taken seriously, might be the key to the new science of the twenty-first century.

Notes

1. De Solla Price refers to Alvin Weinberg (1961). Weinberg himself mentions that he owes the term to Fred Hoyle, the British astronomer.
2. *Grossbetrieb* was a common term at the time used to refer to national scientific enterprises such as the German one, dominated by large research institutes, division of labour and heavy investments in material and equipment. Germany had 'progressed' further on the road to big science – British and French prize winners found the Nobel prize an important research subsidy, while for the Germans it did not make much difference. The importance of German science at the time made it difficult to discount *Grossbetrieb* as a deviation from good science.
3. In 1990, the US National Science Foundation republished the report on the occasion of its fortieth anniversary. This can be seen as a claim of continuity and relevance for Bush's arguments for the 1990s, but also as an indication of a romantic harking back to the past.
4. In his recent book, Steve Fuller (1997) devotes a very interesting chapter to the view on Western science 'from the outside in', from Islam and from Japan.
5. I have presented these ideas in South Africa. See, for example, Rip (1998). I have drawn on the text of this publication for the present chapter.

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