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Science under Pressure

Proceedings

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Foreword

Why a seminar on Science under pressure?

Science never operated in a vacuum, no matter how much its practitioners might have strived for it. Nor was the meaning of science ever, even from its own perspective, a given thing: Copernicus was a renegade rather than a heretic and heliocentric cosmology a treason rather than a sacrilege. The history of science and its place in historical contexts of culture, economy, and politics is a tale of tension as well as complexity. To be sure, different paradigms of science and research traditions as well as different fields of scientific knowledge production have conceived of the double contingency of science and society in very different ways, empirically as well as normatively. Thus, answers to the question of how to conceive of the relationship between science and society, and of how to assess it, abound - they have done so for centuries and will continue to do so. And the same holds for the continuous, but no less troubled, attempt made by scientists themselves to depict what science is and, pace The Science Wars, what it is not. Yet, in a time where science, as faithless to itself as ever, is facing growing demands and, equally important, a growing diversity of demands from the surrounding society, the proposition that science is, indeed, a contingent endeavor can hardly be questioned. And, at the turn of the century, Science and Technology Studies, long since emerged from the obscurity of Academic exotica, have left the hopeful ranks of burgeoning fields of inquiry and become a discipline in its own right.

In September 2000, The Danish Institute for Studies in Research and Research Policy had the pleasure of hosting a European seminar whose objective was to attack the question of scientific contingency head on by asking whether science is under pressure - or just challenged - and if so: in what sense. A range of scholars from different countries, with different disciplinary backgrounds, and with different experiences in regard to the relationship between science and society accepted our invitation and took part in a two-day discussion of the pressures and challenges facing science today. The contributions to this discussion from the speakers, with the addenda of Stefan Hermann's closing reply, are collected in the following proceedings.

Loet Leydesdorff provided input to a lively discussion about the complex, i.e. non-linear, dynamics of the interplay between universities, industry and government from the perspective of a system-theoretically informed Triple Helix model.

Steve Fuller, on the other hand, looked at the governance of science from the vantage point of normative republican political theory: How can citizens win influence on science, given that monastic truth seeking and hyper capitalist commercialism do not exhaust the alternatives open for modern-day universities?

Pressure on science rises and challenges emanate also from within the practice of science itself. Science can be controversial, and its insights and methods are susceptible to continuous critique and challenge, not least from within the echelons of its practioners. But scientists will immediately agree that such critique and challenge is quintessential, not only to their own integrity and to the integrity of their respective disciplines, but also to the very quality of their insights.

Moving from the general, yet very different, system modeling perspectives of Leydesdorff and Fuller, Helge Kragh, partly in response to propositions made previously by Steve Fuller, discussed science and non-science from the perspective of physical science. In particular, he challenged, the view that post-World War II science is unique and radically different from pre-World War II science. Trained in another discipline, Andreas Roepstorff presented some conclusion from an ethnographic case study on a science institution, challenging the dichotomous insider-outsider approach to understanding the meaning, role, and function of science. Science, so Roepstorff argues, must be conceived integratively as a culture-, social order-, and cosmology- producing practice. As could be expected, Kragh's and Roepstorff's presentations sparked an engaged discussion of disciplinary differences as a challenge to science practice.

Whereas Kragh and even Roepstorff set out to discuss the internal workings of scientific practice, Claus Emmeche's analysis brought awareness to the societal challenges that scientists are met by. Emmeche illustrated his general observations on challenges from outside Academia by way of a comment on the pressure that biotechnology-related areas of scientific research are experiencing these years. Transcending the outside-in perspective, Emmeche concluded his presentation by suggesting that science has returned the favor by producing knowledge of a nature that, more than ever, forces society to reflect on the terms and objectives of scientific knowledge production. At the same time, the new research agenda seems to have dealt a final blow to the scholastic isolationism of the »positivist ethos«, in effect making considerations of a political and ethical nature part and parcel of »good science«.

Acknowledging the, by now, almost trivial importance of the international community, the seminar's attention was, finally, directed towards the regional and the global level. Diana Wolff-Albers presented her views on the challenges that face science and technology in Europe as a result of increasing globalization, among them the scientific and technological infrastructure and the quality of human resources. But does increasing globalization make for the biggest pressure against science? Yes, says Thomas Whiston, but not »in the conventional resource allocation meaning of the term«. In his presentation, he argued that the pressure, which is really a challenge, is to contribute to »solutions to the most urgent global environmental problems and basic need of the global economy«. Thus, the urgency of environmental and socio-economic problems on a global scale calls for a »new global scientific agenda which involves all nations as equal partners«.

»Flipping the coin«, Stefan Hermann concludes these proceedings by challenging what he considers to be an underlying premise, of all the seminar presentations (with the exception of Roepstorff and, in part, Fuller), namely the confidence in science as such. Hermann urges us to reconsider, by way of critical sociology, this confidence. Not only is science, according to Hermann, a highly contingent social-cum-discursive practice, it is also a highly effective vehicle of social domination and normalization, which, however, so the Foucauldian argument goes, should not be contrasted to the pursuit of truth.

Among the responsibilities of The Danish Institute for Studies in Research and Research Policy is the task of providing decisionmakers and the public with knowledge of the contextual conditions of science and research and the effect with which science and research exert an influence on society.
I believe that Science under pressure makes a contribution in this regard.

The seminar was organized by researcher Kristian Kindtler in collaboration with research director Elisabeth Vestergaard. And the proceedings were edited by Anne-Mette Pedersen. On behalf of the institute, I want to thank all who participated in the seminar

Aarhus, January 2001
Karen Siune, Director

Content

A Triple Helix of University-Industry-Government Relations: »Mode 2« and the Globalization of »National« Systems of Innovation[1]

Loet Leydesdorff[2] and Henry Etzkowitz[3]

Abstract
The Triple Helix of University-Industry-Government Relations is compared with alternative models for explaining the current transitions in the research system in their social contexts. Communications and negotiations between institutional partners generate a reflexive overlay that increasingly reorganizes the underlying arrangements. This process of interactive codification makes the economy knowledge-based. The institutional layer can be considered as the retention mechanism of the evolutionarily developing system. »National« organization of the systems of innovation has historically been important in determining competition, but reorganizations across industrial sectors and nation states are induced by new technologies (biotechnology, ICT). University research provides a locus of exploration in these knowledge-intensive network transitions.

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1 *This is the English version of a paper originally published in French: Loet Leydesdorff and Henry Etzkowitz, »Le Mode 2« et la globalisation des systèmes d'innovation »nationaux«: le modèle à triple hélice des relations entre université, industrie et gouvernement,* Sociologie et Société, *32(1) (2000) pp. 135-156; at <http://www.erudite.org/erudite/socsoc/v32n01/leydesd/leydesd.pdf>.*

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1. Introduction

Three models have recently been proposed for the explanation of the socio-economic relations of the knowledge base: (1) the model of national systems of innovations (e.g., Lundvall, 1988 and 1992; Nelson, 1993), (2) the model of an emerging »Mode 2« of the production of scientific knowledge (Gibbons et al. 1994), and (3) the model of a Triple Helix of university-industry-government relations (Etzkowitz and Leydesdorff, 1995, 1997, and 2000).

The Triple Helix model improves on the national systems of innovation model by declaring »governance« as a variable. Thus, the different levels of government (European, national, regional, and local) can be studied in a coherent framework. The Triple Helix shares with the »Mode 2« model a focus on a dynamic overlay of negotiations and alliances between and among the institutional carriers of the overlay. Information is continuously codified into new knowledge when communicated reflexively, both within each of the helices recursively and interactively among them. Knowledge and information flows function as an emerging coordination mechanism of society in parallel and in interaction with existing (economic) exchange relations and (political) control mechanisms. Thus, the political economy tends to become increasingly knowledge-based.

Three interacting dynamics (knowledge production, markets-i.e., diffusion-and control) can be expected to generate non-linear patterns which contain all the species of chaotic behaviour (crises, trajectory formation, bifurcations, etc.). One needs an algorithmic model for studying such complex dynamics. Furthermore, the »Triple Helix« thesis states that the university can play an enhanced role in innovation in a knowledge-based economy. The model is analytically different from the National Systems of Innovation (NSI) approach which considers the firm as having the leading role in innovation, and from the »Triangle« model of Sábato (1975), in which the state is privileged (cf. Sábato and Mackenzie 1982). We focus on the *network overlay of communications and expectations* that reshape the institutional arrangements among universities, industries, and governmental agencies. We hypothesize a potentially salient role of the university since the knowledge production function is increasingly made part of the knowledge infrastructure (Godin and Gingras, 2000).

2. The Salience of University Research

As the role of the military has decreased and academia has risen in the institutional structures of contemporary societies, the network of relationships among academia, industry, and government have also been transformed, displacing the Cold-War »Power Elite« trilateral mode of Wright Mills (1958) with an overlay of reflexive communications. Because the university-industry-government relations are no longer hierarchically organized given a pluriform society, the effects of the transformations are the subject of an international debate over the appropriate role of the university in technology and knowledge transfer.

For example, the *Swedish Research 2000 Report* recommended the withdrawal of the universities from the envisaged »third mission« of direct contributions to industry. Instead, the university should return to research and teaching tasks, as traditionally conceptualized (see Benner and Sandström, 2000). However, it can be expected that proponents of the third mission from the new universities and regional colleges, which have based their research programmes on its premises, will continue to make their case. Science and technology have become important to regional developments (e.g., Saxenian, 1994; Cooke et al., 1997; Braczyk et al. 1998). Both RandD and higher education can be analyzed also in terms of markets (Dasgupta and David, 1994).

The issues in the Swedish debate are echoed in the critique of academic technology transfer in the U.S.A. by several economists (e.g., Rosenberg and Nelson, 1994). The argument is that academic technology transfer mechanisms may create unnecessary transaction costs by encapsulating knowledge in patents that might otherwise flow freely to industry. But would the knowledge be efficiently transferred to industry without the series of mechanisms for identifying and enhancing the applicability of research findings? How are development processes to be carried further, through special grants for this purpose or in new firms formed on campus and in university incubator facilities?

The institutional innovations aim to promote closer relations between faculties and firms. »The Endless Frontier« of basic research funded as an end in itself, with only long-term practical results expected, is being replaced by an »Endless Transition« model in which basic research is linked to utilization through a series of intermediate processes (Callon 1998), often stimulated by government. The linear model either expressed in terms of »market pull« or »technology push« was insufficient to induce transfer of knowledge and technology. Publication and patenting assume different systems of reference both from each other and with reference to the transformation of knowledge and technology into marketable products. The rules and regulations had to be reshaped and an interface strategy invented in order to integrate »market pull« and »technology push« through new organizational mechanisms (e.g., OECD 1980; Rothwell and Zegveld 1981).

In the U.S.A., these programs include the Small Business Innovation Research program (SBIR) and the Small Bussiness Technology Transfer Program (STTR) of the Department of Defense, the Industry/University Cooperative Research Centers (IUCRC) and Engineering Research Centers (ERC) of the National Science Foundation, etc. (Etzkowitz et al., 2000). In Sweden, the Knowledge Competency Foundation, the Technology Bridge Foundation were established as public venture capital source, utilizing the Wage Earners Fund, originally intended to buy stock in established firms on behalf of the public. The beginnings of a Swedish movement to involve academia more closely in this direction has occasioned a debate similar to the one that took place in the U.S. in the early 1980s. At that time, Harvard University sought to establish a firm jointly with one of its professors, based on his research results.

Can academia encompass a third mission of economic development in addition to research and teaching? How can each of these various tasks contribute to the mission of the university? The late nineteenth century witnessed an academic revolution in which research was introduced into the university mission and made more or less compatible with teaching, at least at the graduate level. Many universities in the U.S.A. and worldwide are still undergoing this transformation of purpose. The increased salience of knowledge and research to social and economic development has opened up a third mission: the role of the university in socio-economic development.
A »Second Academic Revolution« seems under way since W.W. II, but more visibly since the end of the Cold War (Etzkowitz, forthcoming).

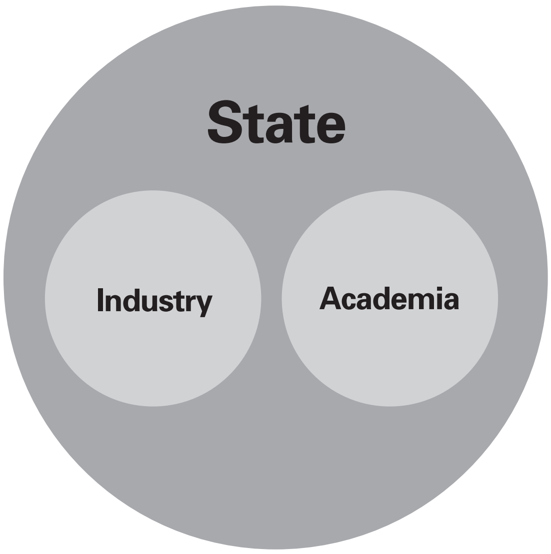
In the U.S.A. in the 1970s, in various Western European countries during the 1980s, and in Sweden at present, this transition has led to a reevaluation of the mission and role of the university in society. Similar controversies have taken place in Latin America, Asia, and elsewhere in Europe. The »Triple Helix« series of conferences (Amsterdam, 1996; Purchase, New York, 1998; and Rio de Janeiro, 2000) have provided a venue for the discussion of theoretical and empirical issues by academics and policy analysts (Leydesdorff and Etzkowitz, 1996 and 1998). Different possible resolutions of the relations among the institutional spheres of university, industry, and government can help to generate alternative strategies for economic growth and social transformation.

3. Three Triple Helix Perspectives

The evolution of innovation systems, and the current conflict over which path should be taken in university-industry relations, are reflected in the different perspectives on institutional arrangements of university-industry-government relations. The various configurations can analytically be considered as »instantiations« (Giddens 1984) of the more general Triple Helix model. These policy models are entertained for the purpose of normative orientation and policy guidance.

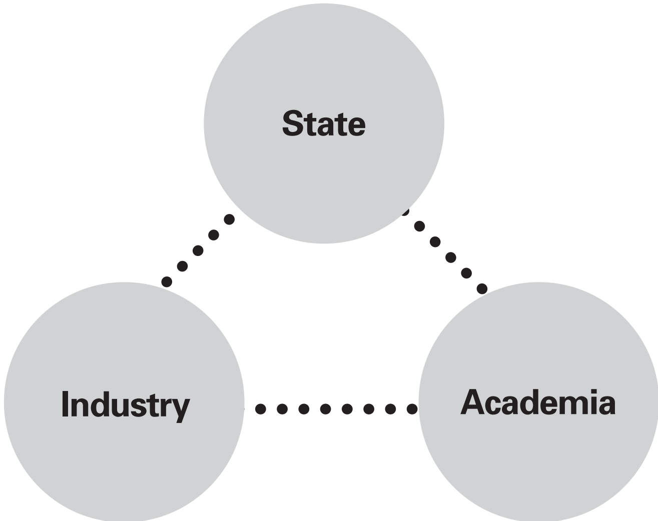
First, a specific historical situation can be distinguished which we label »Triple Helix I.« In this configuration the nation state encompasses academia and industry and directs the relations between them (Figure 1). The strong version of this model could be found in the former Soviet Union and in Eastern European countries under »existing socialism.« Weaker versions were formulated in the policies of many Latin American countries and to some extent in European countries such as Norway.

Figure 1. An Etatistic Model of University-Industry-Government Relations



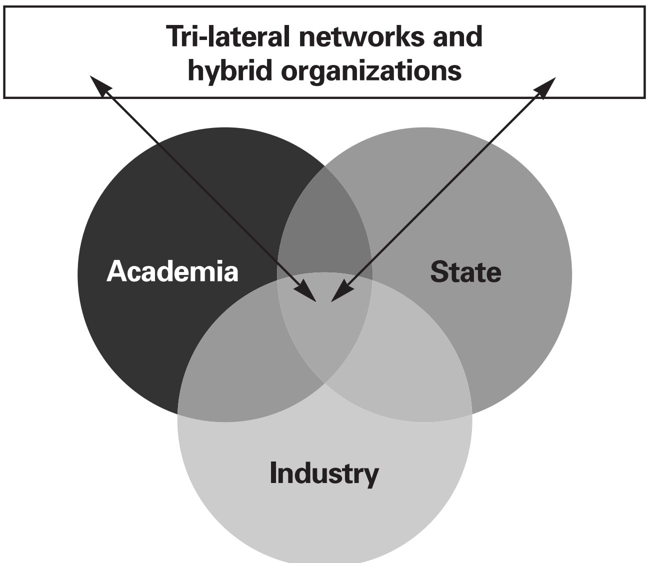
A second policy model (Figure 2) consists of separate institutional spheres with strong borders dividing them and highly circumscribed relations among the spheres, exemplified in Sweden by the noted *Research 2000 Report* and in the U.S.A. in opposition to the various reports of the Government-University-Industry Research Roundtable (GUIRR) of the National Research Council (MacLane 1996; cf. GUIRR 1998).

Figure 2. A »laissez-faire« Model of University-Industry-Government Relations



Finally, the further developed »Triple Helix III« model can be expected to generate a knowledge infrastructure in terms of overlapping institutional spheres, with each taking the role of the other and with hybrid organizations emerging at the interfaces (Figure 3). The overlay partially integrates the underlying arrangements, but in a distributed network mode. Thus, the system remains incomplete and therefore in flux.

Figure 3. The Triple Helix Model of University-Industry-Government relations



The differences between the latter two configurations of university-industry-government relations currently generate normative interest. »Triple Helix I« is largely viewed as a failed developmental model. With too little room for »bottom up« initiatives, innovation was discouraged rather than encouraged. »Triple Helix II« entails a laissez-faire policy, nowadays also advocated as shock therapy to reduce the role of the state in »Triple Helix I.« In one form or another, most countries and regions are presently trying to attain some form of the fully fledged »Triple Helix III« model.

The common objective is to realize an innovative environment consisting of university spin-off firms, tri-lateral initiatives for knowledge-based economic development, and strategic alliances among firms (large and small, operating in different areas, and with different levels of technology), government laboratories, and academic research groups. These arrangements are often encouraged, but not controlled, by government, whether through new »rules of the game,« direct or indirect financial assistance, or through the Bayh-Dole Act in the U.S.A. or new actors such as the above mentioned foundations to promote innovation in Sweden.

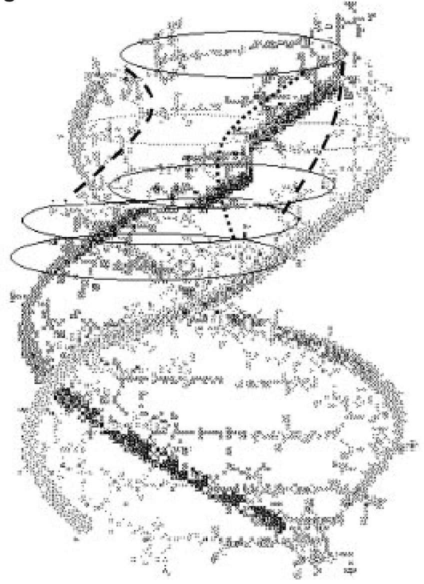
4. The Triple Helix Model of Innovations

The Triple Helix as an analytical model adds to the description of the variety of institutional arrangements and policy models an explanation of their dynamics. What are the units of operation that interact when a system of innovation is formed? How can such a complex system be specified?

In our opinion, typifications in terms of »national systems of innovation« (Lundvall 1988; Nelson 1993); »research systems in transition« (Cozzens et al., 1990; Ziman 1994), »Mode 2« (Gibbons et al., 1994) or »the post modern research system« (Rip and Van der Meulen 1996) are indicative of flux, reorganization, and the enhanced role of knowledge in the economy and society. In order to explain these observable reorganizations in university-industry-government relations, one needs to transform the sociological theories of institutional retention, recombinatorial innovation, and reflexive controls. Each theory can be expected to appreciate a different subdynamic (Leydesdorff 1997).

In contrast to a double helix (or a coevolution between two dynamics), a triple helix cannot be expected to generate long-term stability. The biological metaphor does no longer work because of the difference between cultural and biological evolutions. Biological evolution theory assumes variation as a driver and selection to be naturally given. Cultural evolution, however, is driven by individuals and groups who make conscious decisions as well as the appearance of unintended consequences. A Triple Helix in which each strand may relate to the other two can be expected to develop a reflexive overlay of communications, networks, and organizations among the helices (Figure 4). This result can also be considered as an unintended consequence of a societal development which no longer requires central alignment by a central government (like in times of war).

Figure 4. The overlay of communications and expectations at the network level guides the reconstruction of institutional arrangements



The sources of innovation in a Triple Helix configuration can no longer be considered as synchronized a priori. They do not fit together in a pregiven order, but they generate puzzles for participants, analysts, and policy-makers to solve. This network of relations generates a feedback among intentions, strategies, and projects that adds surplus value by reorganizing and harmonizing continuously the underlying infrastructure in order to achieve at least an approximation of the variety of goals. The issue of how much we are in control or non-control of these dynamics specifies a research program on innovation.

Innovation systems, and the relationships among them, are apparent at the organizational, local, regional, national, and multi-national levels. The interacting subdynamics, that is, specific operations like markets and technological innovations, are continuously reconstructed like commerce on the Internet, yet differently at different levels. The subdynamics and the levels are also reflexively reconstructed through discussions and negotiations. What is considered as »industry«, what as »market« cannot be taken for granted and should not be reified. Each »system« is defined and can be redefined as the research project is designed.

For example, »national systems of innovation« can be more or less systemic. The extent of systemness, however, remains an empirical question (see Leydesdorff and Oomes (1999) for a test on »systemness«). The dynamic »system(s) of innovation« may consist of increasingly complex collaborations across national borders and among researchers and users of research from various institutional spheres. There may be different dynamics among regions (Cooke 1998; Riba-Vilanova and Leydesdorff, 1999). The systems of reference have to be specified analytically, that is, as theoretically informed hypotheses. The Triple Helix hypothesis adds that the arrangements can be expected to remain in transition. The observations provide an opportunity to update the analytical expectations.

5. An Endless Transition

The infrastructure of knowledge-intensive economies implies an Endless Transition. Marx's great vision that »all that is solid, melts into air« (Berman 1982) underestimated the importance of seemingly volatile communications and interactions in recoding the (complex) network system. Particularly, when knowledge is increasingly utilized as a resource for the production and distribution system, reconstruction may come to prevail as a mode of »creative destruction« (Schumpeter 1939 and 1966; Luhmann 1984).

Can the reconstructing forces be specified? One mode of specification is provided by evolutionary economics in which the three functional mechanisms are: technological innovation provides the variation, markets are the prevailing selectors, and the institutional structures provide the system with retention and reflexive control (Nelson 1994). In advanced and pluriform societies, the mechanisms of institutional control are further differentiated into public and private domains. Thus, a complex system is developed that is continuously integrated and differentiated, both locally and globally.

Innovation can be defined at different levels and from different perspectives within this complex dynamics. For example, evolutionary economists have argued that one should consider firms as the units of analysis, since they carry the innovations and they have to compete in markets (Nelson and Winter 1982; cf. Andersen 1994). From a policy perspective, one may wish to define »national systems of innovation« as a relevant frame of reference for government interventions. Others have argued in favour of networks as more abstract units of analysis: the semi-autonomous dynamics of the networks may exhibit lock-ins, segmentation, etc. (e.g., David and Foray 1994). Furthermore, the evolving networks may change in terms of relevant boundaries while developing (Maturana 1978).

In our opinion, these various perspectives open windows of appreciation on the dynamic and complex processes of innovation, but from specific angles. The complex dynamics is composed of subdynamics like market forces, political power, institutional control, social movements, technological trajectories and regimes. The operations can be expected to be nested and interacting. Integration, for example, within a corporation or within a nation state, cannot be taken for granted. Technological innovation may also require the reshaping of an organization or a community (Freeman and Perez 1988). But the system is not deterministic: in some phases intentional actions may be more succesful in shaping the direction of technological change than in others (Hughes 1983).

The dynamics are non-linear while both the interaction terms and the recursive terms have to be declared. First, there are ongoing transformations within each of the helices. These reconstructions can be considered as a level of continuous innovations under pressure of changing environments. When two helices are increasingly shaping each other mutually, co-evolution may lead to a stabilization along a trajectory. If more than a single interface is stabilized, the formation of a globalized regime can be expected. At each level, cycles are generated which guide the phasing of the developments. The higher-order transformations (longer-term) are induced by the lower-order ones, but the latter can seriously be disturbed by events at a next-order system's level (Schumpeter 1939; Kampmann et al. 1994).

Although this model is abstract, it enables us to specify the various windows of theoretical appreciation in terms of their constitutive subdynamics (cf. Leydesdorff and Van den Besselaar 1997). The different subdynamics can be expected to select upon each other asymmetrically, as in processes of negotiation, by using their specific codes. For example, the markets and networks select upon technological feasibilities, whereas the options for technological developments can also be specified in terms of market forces. Governments can intervene by helping create a new market or otherwise changing the rules of the game.

When the selections »lock-in« upon each other, next-order systems may become relevant. For example, airplane development at the level of firms generates trajectories at the level of the industry in coevolutions between selected technologies and markets (McKelvey 1996). Nowadays, the development of a new technological trajectory invokes the support of national governments and even international levels (like the EU), using increasingly a Triple Helix regime (Frenken and Leydesdorff, 2000). Our approach enables us to pursue the analysis at the network level and then to compare among configurations of relations. For example, both industries and governments are entrained in institutional transformations, while the institutional transformations themselves change under the pressure of information and communication technologies (ICT) or government policies. Before explaining the organization of the Triple Helix model in terms of these implications, however, we wish to turn once more to the analytical position of the Triple Helix model in relation to other non-linear models of innovation, like »Mode 2« and »national systems of innovation.« How does the algorithmic model which operates in terms of relational changes and expectations differ from the geometrical metaphors based on historical observations that hitherto prevail in the literature?

6. Non-linear models of innovation

Non-linear models of innovation extend upon linear models by taking interactive and recursive terms into account. These non-linear terms are expected to change the causal relations between input and output. The production rules in the systems under study, for example, can be expected to change with the further development of the input/output relations (e.g., because of economies of scale). Thus, the unit of operation may be transformed, as is typical when a pilot plant in the chemical industry is scaled up to a production facility.

By changing the unit of analysis into a unit of operation at the reflexive level, one obtains a different perspective on the system under study. But the system itself is also evolving. In terms of methodologies, this challenges our conceptual apparatus, since one has to be able to distinguish whether a variable has changed or merely the value of the variable. The discursive analysis contains a snapshot, while the reality provides a moving picture. One needs metaphors to reduce the complexity for the discursive understanding. Geometrical metaphors can be stabilized by higher-order codifications as in the case of paradigms. The understanding in terms of fluxes (that is, how the variables as well as the value may change over time), however, calls for the use of algorithmic simulations. The observables can then be considered as special cases which inform the expectations (Leydesdorff 1995).

Innovation, in particular, can be defined only in terms of an operation. Both the innovator(s) and the innovated system(s) are expected to be changed by the innovation. Furthermore, one is able to be both a participant and an observer, and one is also able to change perspectives. In the analysis, however, the various roles are distinguished although they can sometimes be fused in »real life« events. Langton (1989) proposed to distinguish between the »phenotypical« level of the observables and the »genotypical« level of analytical theorizing. The »phenotypes« remain to be explained and the various explanations compete in terms of their clarity and usefulness for updating the expectations. Confusion, however, is difficult to avoid given the pressure to jump to normative conclusions, while different perspectives are continuously competing, both normatively and analytically.

Let us first focus on the problem of the unit of analysis and the unit of operation. In addition to extending the linear (input/output) models of neo-classical and business economics with a recursive perspective, evolutionary economists also changed the unit of analysis. Whereas neo-classical economics focused on markets as networks in terms of input/output relations among informed and rational agents, evolutionary economists have tended to focus on firms as the specific and bounded carriers of an innovation process. Both the unit of analysis and the unit of operation were thus changed (Andersen 1994; cf. Alchian 1950).

Lundvall (1988, at p. 357) noted that the *interactive* terms between demand and supply in user-producer relations assume a system of reference in addition to the market at each moment in time. The classical dispute in innovation theory had, in his opinion, referred to the role of demand and supply, that is, market forces, in determining the rate and direction of the process of innovation (cf. Mowery and Rosenberg, 1979; Freeman, 1982, p. 211). If, however, the dynamics of innovation (e.g., product competition) are expected to be different from the dynamics of the market (i.e., price competition), an additional system of reference for the selection should also be specified with reference to the time axis.

Lundvall (1988) proposed »to take the national system of production as a starting point when defining a system of innovation« (p. 362). However, he added that the national system of production should not be considered as a closed system: »the specific degree and form of openness determines the dynamics of each national system of production.« In our opinion, as a first step, innovation systems should be considered as the dynamics of *change* in systems of both production and distribution. From this perspective, national systems compete in terms of the adaptability of their knowledge infrastructure. How are competences distributed for solving »the production puzzle« which is generated by uneven technological developments across sectors (Nelson and Winter 1975; Nelson 1982)?

The infrastructure conditions the processes of innovation which are possible within and among the sectors. In particular, the distribution of relevant actors contains an heuristic potential which can be made reflexive by a strategic analysis of specific strengths and weaknesses (Pavitt 1984). The solution of the production puzzle typically brings government into the picture shifting the dynamics from a double to a triple helix. The consequent processes of negotiation are both complex and dynamic: one expects that the (institutional) actors will be reproduced and changed by the interactions. Trilateral networks and hybrid organzations are created for resolving social and economic crises. The actors from the different spheres negotiate and define new projects, such as the invention of the venture capital firm in New England in the early post-war era (Etzkowitz, forthcoming). Thus, a Triple Helix dynamics of University-Industry-Government Relations is generated endogeneously.

Gibbons et al. (1994) argued that this »new mode of the production of scientific knowledge« has now become manifest. But: how can these dynamics in the network arrangements between industries, governments, and academia be considered as a consequence of the user-producer interactions foregrounded by Lundvall (1988)? Are national systems still a relevant unit of analysis? Since the new mode of knowledge production (»Mode 2«) is characterized as an outcome, it should, in our opinion, be considered as an emerging system. The emerging system rests like a hyper-network on the networks on which it builds (such as the disciplines, the industries, and the national governments), but the knowledge-economy transforms »the ship while a storm is raging on the open sea« (Neurath et al., 1929).

Science has always been organized through networks, and to pursue practical as well as theoretical interests. Centuries before »Mersenne«, was transmogrified into an Internet site, he was an individual, who by visits and letters, knitted the European scientific community together. The Academies of Science played a similar role in local and national contexts from the 16th century. The practical impetus to scientific discovery is long-standing. Robert K. Merton's (1938) dissertation reported that between 40-60% of discoveries in the 17th century could be classified as having their origins in trying to solve problems in navigation, mining, etc. Conversely, solution of practical problems through scientific means has been an important factor in scientific development, whether in German pharmaceutical science in the 17th century (Gustin 1975) or in the British sponsored competition to provide a secure basis for navigation (Sobel, 1995).

The so-called »Mode 2« is not new; it is the original format of science before its academic institutionalization in the nineteenth century. Another question to be answered is why »Mode 1« has arisen after »Mode 2«: the original organizational and institutional basis of science, consisting of networks and invisible colleges (cf. Weingart, 1997; Godin, 1998). Where have these ideas, of the scientist as the isolated individual and of science separated from the interests of society, come from? »Mode 2« represents the material base of science, how it actually operates. »Mode 1« is a construct, built upon that base in order to justify autonomy for science, especially in an earlier era when it was still a fragile institution and needed all the help it could get.

In the U.S.A., during the late 19th century, large fortunes were given to found new universities, and expand old ones. There were grave concerns among many academics that the industrialists making these gifts would try to directly influence the universities, by claiming rights to hire and fire professors as well as well as to decide what topics were acceptable for research and instruction (Storr, 1953). To carve out an independent space for science, beyond the control of economic interests, a physicist, Henry Rowland, propounded the doctrine that if anyone with external interests tried to intervene, it would harm the conduct of science. As President of the American Association for the Advancement of Science, he promoted the ideology of pure research in the late 19th century. Of course, at the same time as liberal arts universities oriented toward pure research were being founded, land grant universities, including MIT, pursued more practical research strategies. These two contrasting academic modes existed in parallel for many years.

Decades hence, Robert K. Merton posited the normative structure of science in 1942 and strengthened the ideology of »pure science.« His emphasis on universalism and skepticism was a response to a particular historical situation, the need to defend science from corruption by the Nazi doctrine of a racial basis for science and from Lysenko's attack on genetics in the Soviet Union. Merton's formulation of a set of norms to protect the free space of science was accepted as the basis for an empirical sociology of science for many years.

The third element in establishing the ideology of pure science was, of course, the Bush Report of 1945. The huge success of science in supplying practical results during World War II in one sense supplied its own legitimation for science. But with the end of the war at hand and wanting to insure that science was funded in peacetime, a rationale was needed in 1944 when Bush persuaded President Roosevelt to write a letter commissioning the report (Bush 1980).

In the first draft of his report, Bush proposed to follow the then current British method of funding science at universities. It would be distributed on a per capita basis according to the number of students at each school. In the contemporary British system of a small number of universities, the funds automatically went to an elite. However, if that model had been followed in the U.S., even in the early post war era, the flow of funds would have taken a different course. The funding would not only have flowed primarily to a bi-coastal academic elite but would have been much more broadly distributed across the academic spectrum, especially to the large state universities in the Midwest.

In the time between the draft and the final report, the mechanism for distribution of government funds to academic research was revised and »peer review« was introduced. Adapted from Foundation practices in the 1920s and 30s, it could be expected that »the peers,« the leading scientists who would most surely be on those committees, would distribute the funds primarily to a scientific elite. The status system of U.S. universities that had been in place from the 1920s was reinforced.

This model of »best science« is no longer acceptable to many as the sole basis for distribution of public research funds. Congresspersons who represent regions with universities that are not significant recipients of research funds have disregarded peer review and distributed research funds by direct appropriation, much as roads and bridges are often sited through »log rolling« and »pork barrel« processes. Nevertheless, these politically directed funds support also serious scientific research and instrumentation projects. Even when received by schools with little or no previous research experience, these »one time funds« are typically used to rapidly build up competencies in order to compete within the peer review system.

Indeed, when a leading school, Columbia University, needed to renew the infrastructure of its chemistry department, it contracted with the same lobbying firm in Washington DC as less well-known schools. Through public relations advice, Columbia relabeled its chemistry department »The National Center for Excellence in Chemistry.« A special federal appropriation was made and the research facilities were renovated and expanded. To hold its faculty, the university could not afford to wait for the slower route of peer review, and likely smaller amounts of funding.

Increasing competition for research funds among new and old actors has caused an incipient breakdown of »peer review«, a system that could best adjudicate within a moderate level of competition. As competition for research funds continues to expand, how should the strain be adjusted? Some propose shrinking the research system; others suggest linking science to new sources of legitimation such as regional development.

7. The Future Legitimation of Science

It is nowadays apparent that the development of science provides much of the basis for future industrial development. These connections, however, have been present from the creation of science as an organized activity in the 17th century. Marx pointed them out again in the mid-19th century in connection with the development of chemical industry in Germany. At the time, he developed a thesis of the growth of science-based industry on the basis of a single empirical example: Perkins researches on dyestuffs in the UK leading to the development of an industry in Germany.

The potential of science to contribute to economic development has become a source of regional and international competition at the turn of the millenium. Until recently, the location of research was of little concern. The relationship between the site where knowledge is produced and its eventual utilization was not seen to be tightly linked, even as a first mover advantage. This view has changed dramatically in recent years, as has the notion that high-tech conurbations, like Route 128 and Silicon Valley, are unique instances that can not be replicated. The more recent emergence of Austin, Texas, for example, is based in part on the expansion of research at the University of Texas, aided by state as well as industry and federal funds.

Less research intensive regions are by now well aware that science, applied to local resources, is the basis of much of their future potential for economic and social development. In the U.S.A., it is no longer acceptable for research funds to primarily go to the east and west coasts with a few places in between in the Midwest. The reason why funding is awarded on bases other than the peer review system, is that all regions want a share of research funding. The classic legitimation for scientific research as a contribution to culture still holds and military and health objectives also remain a strong stimulus to research funding. Nevertheless, the future legitimation for scientific research, which will keep funding at a high level, is that it is increasingly the source of new lines of economic development.

Newly created disciplines are often the basis for these heightened expectations. Such disciplines do not arise only from the subdivision of new disciplines from old ones, as in the 19th century (Ben David and Collins, 1966). New disciplines have arisen, more recently, through syntheses of practical and theoretical interests. For example, computer science grew out of elements of older disciplines such as electrical engineering, psychology, philosophy, and a machine. Materials science and other fields such as nano-technology that are on every nation's critical technology list were similarly created.

The university can be expected to remain the core institution of the knowledge sector as long as it retains its original educational mission. Teaching is the university's comparative advantage, especially when linked to research and economic development. Students are also potential inventors. They represent a dynamic flow-through of »human capital« in academic research groups, as opposed to more static industrial laboratories and research institutes. Although they are sometimes considered a necessary distraction, the turnover of students insures the primacy of the university as a source of innovation.

The university may be compared to other recently proposed contenders for knowledge leadership, such as the consulting firm. A consulting company draws together widely dispersed personnel for individual projects and then disperses them again after a project, solving a client's particular problem, is completed. Such firms lack the organizational ability to pursue a cumulative research program as a matter of course. The university's unique comparative advantages is that it combines continuity with change, organizational and research memory with new persons and new ideas, through the passage of student generations. When there is a break in the generations, typically caused by a loss of research funding, one academic research group disappears and can be replaced by another.

Of course, as firms organize increasingly higher level training programs (e.g., Applied Global University at the Applied Materials Devices Corporation, a semi-conductor equipment manufacturer in Silicon Valley) they might in the future also, individually or jointly, attempt to give out degrees. Companies often draw upon personnel in their research units, as well as external consultants, to do some of the teaching in their corporate universities. Nevertheless, with a few notable exceptions, such as the RAND Corporation, they have not yet systematically drawn together research and training into a single framework. However, as the need for life-long learning increases, a university tied to the workplace becomes more salient.

8. Implications of the Triple Helix Model

The Triple Helix denotes not only the relationship of university, industry and government, but also internal transformation within each of these spheres. The university has been transformed from a teaching institution into one which combines teaching with research, a revolution that is still ongoing, not only in the U.S.A., but in many other countries. There is a tension between the two activities but nevertheless they co-exist in a more or less compatible relationship with each other because it has been found to be both more productive and cost effective to combine the two functions.

The Triple Helix overlay provides a model at the level of social structure for the explanation of »Mode 2« as an historically emerging structure for the production of scientific knowledge, and its relation to »Mode 1.« First, the arrangements between industry and government no longer need to be conceptualized as exclusively between national governments and specific industrial sectors. Strategic alliances cut across traditional sector divides; governments can act at national, regional, or increasingly also at international levels. Corporations adopt »global« postures either within a formal corporate structure or by alliance. Trade blocks like the EU, NAFTA, and Mercosul provide new options for breaking »lock-ins,« without the sacrifice of competitive advantages from previous constellations. For example, the Airbus can be considered as an interactive opportunity for recombination at the supra-national level (Frenken, 2000).

Second, the driving force of the interactions can be specified as the expectation of profits. »Profit« may mean different things to the various actors involved. A leading edge consumer, for example, provides firms and engineers with opportunities to perceive »reverse salients« in current product lines and software. Thus, opportunities for improvements and puzzle-solving trajectories can be defined. Note that analytically the drivers are no longer conceptualized as *ex ante* causes, but in terms of expectations that can be evaluated only ex post. From the evolutionary perspective, selection *(ex post)* is structure determined, while variation may be random (Arthur 1988; Leydesdorff and Van den Besselaar 1998).

Third, the foundation of the model in terms of expectations leaves room for uncertainties and chance processes. The institutional carriers are expected to be reproduced as far as they have been functional hitherto, but the negotiations can be expected to lead to experiments which may thereafter also be institutionalized. Thus, a stage model of innovation can be specified. The stages of this model do not need to correspond with product life cycle theory. Barras (1990), for example, noted that in ICT »a reverse product life« cycle seems to be dominant. Bruckner et al. (1994) proposed niche-creation as the mechanism of potential lock-out in the case of competing technologies. A successful innovation changes the landscape, that is, the opportunity structure for the institutional actors involved. Structural changes in turn are expected to change the dynamics.

Fourth, the expansion of the higher-education and academic research sector has provided society with a realm in which different representations can be entertained and recombined in a systematic manner. Kaghan and Barett (1997) have used in this context the term »desktop innovation« as different from the laboratory model (cf. Etzkowitz, 1999). Knowledge-intensive economies can no longer be based on simple measures of profit maximization: utility functions have to be matched with opportunity structures. Over time, opportunity structures are recursively driven by the contingencies of prevailing and possible technologies. A laboratory of knowledge-intensive developments is socially available and can be improved upon (Etzkowitz and Leydesdorff 1995). As this helix operates, the human capital factor is further developed along the learning curves and as an antidote to the risk of technological unemployment (Pasinetti, 1981).

Fifth, the model also explains why the tensions need not to be resolved. A resolution would hinder the dynamics of a system which lives from the perturbations and interactions among its subsystems. Thus, the subsystems are expected to be reproduced. When one opens the black-box one finds »Mode 1« within »Mode 2,« and »Mode 2« within »Mode 1.« The system is neither integrated nor completely differentiated, but it performs on the edges of fractional differentiations and local integrations. Using this model, one can begin to understand why the global regime exhibits itself in progressive instances, while the local instances inform us about global developments in terms of the exceptions which are replicated and built upon.

Case materials enable us to specify the negative selection mechanisms reflexively. Selection mechanisms, however, remain constructs. Over time, the inference can be corroborated. At this end, the function of reflexive inferencing based on available and new theories moves the system forward by drawing attention to possibilities for change.

Sixth, the crucial question of the exchange media -economic expectations (in terms of profit and growth), theoretical expectations, assessment of what can be realized given institutional and geographic constraints- have to be related and converted into one another. The helices communicate recursively over time in terms of each one's own code. Reflexively, they can also take the role of each other, to a certain extent. While the discourses are able to interact at the interfaces, the frequency of the external interaction is (at least initially) lower than the frequency within each helix. Over time and with the availability of ICT, this relation may change.

The balance between spatial and virtual relations is contingent upon the availability of the exchange media and their codifications. Codified media provide the system with opportunities to change the meaning of a communication (given another context) while maintaining its substance (Cowan and Foray 1997). Despite the »virtuality« of the overlay, this system is not »on the fly«: it is grounded in a culture which it has to reproduce (Giddens 1984). The retention mechanism is no longer given, but »on the move«: it is reconstructed as the system is reconstructed, that is, as one of its subdynamics. As the technological culture provides options for recombination, the boundaries of communities can be reconstituted. The price may be felt as a loss of traditional identities or alienation, or as a concern with the sustainability of the reconstruction, but the reverse of »creative destruction« is the option of increasing development. The new mode of knowledge production generates an Endless Transition that continuously redefines the borders of the Endless Frontier.

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Republicanism as a Theory of Science Governance

Steve Fuller*

If the idea of »science governance« is to be taken seriously, then it needs to be attached to an appropriate political theory. In contrast to the communitarian and liberal theories implicitly used to justify the governance of science, I draw on the resources of republicanism. After explaining the historical and philosophical contours of the theory, I argue that the university remains the ideal site for republican governance. Nevertheless, there are various obstacles, both historical and contemporary, to the realization of republicanism. I discuss these in consecutive sections, ending with an agenda to revive republicanism in today's universities.

1. The Republican Motivation in Historical and Philosophical Terms

Republicanism is ultimately a theory of liberty. (See Philip Pettit, *Republicanism,* Oxford University Press, 1997, for the most comprehensive recent statement.) The theory states that liberty is freedom from domination by another's will, where »domination« is understood mainly as a counterfactual condition - that is, not actual interference but the perceived threat of interference that inhibits people from doing and saying what they would like. The republican sense of liberty is enforced by a constitutional agreement to submit to what the 17th century English philosopher, James Harrington, originally called an »empire of laws, not men.« Republicans regard the actions taken by either a solitary tyrant or a democratic majority as equally the product of an »arbitrary will« that knows no law other than its own desire. In this sense, there is nothing »natural« about republican liberty: it must always be socially constructed; hence, the significance attached to a written constitution and the need for regular elections in democracies to decide the fate of existing policies, *even when no one is complaining.* In this respect, republicanism incorporates the reflectiveness that warrants the label (associated with the US Constitution): »philosophically designed order.«

Because republican liberty is the explicit product of a legal system, citizens are obliged to uphold and test (for purposes of improving) the system. This

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implies significant civic participation. Most fundamentally, people agree to couch their political disagreements in terms amenable to resolution by legally recognized procedures; hence, republicans have placed great store by an education in rhetoric. Republicans are loath to refer to their personal interest in the outcome of a debate, since the long-term value of appearing honorable in the polity outweighs any short-term personal advantage. The way Bush and Gore handled themselves in the aftermath of the disputed 2000 US presidential election — ever translating personal feelings into legal arguments - epitomized republicanism in action. Thus, when republican theorists speak of the value of »civility,« they are not referring to some outmoded ideal of courtliness, but rather to what the political theorist Jon Elster has called the »civilizing force of hypocrisy.« People come to believe the reasons they use to justify their actions. This means that, for republicans, political rationality is tantamount to mass hypocrisy. Put a bit more floridly, republicanism's much vaunted »civil religion« is a belief in the transubstantiation of private vices into public virtue via the administrative alchemy of the law.

But the republican sense of obligation goes beyond the norms governing free public expression. While republican regimes have been opposed to the existence of standing armies (or police), they have endorsed universal military training and rotating service in the armed forces. Also, republicans tend to oppose the idea of career politicians — especially that a life in politics can be a vehicle of upward social mobility. (There lies the corruption of the individual and the captivity of the polity.) Instead, they have supported the random selection of civil officeholders, akin to the selection of jurors in trials, combined with compulsory voting in elections. The presupposition here is that regardless of their other differences, citizens are peers in matters of public affairs (what the Romans meant by *res publica*) and, more to the point, are obliged to act that way. In that sense, the obligation to exercise one's freedom is a democratized version of *noblesse oblige.*

Also presupposed in this compulsory conception of freedom is that the competences involved in managing one's own and society's affairs are micro- and macro-versions of the same skills. Thus, proven competence in the management of one's estate establishes the sense of social responsibility necessary for holding political office, and compulsory military training enables one to stand up for oneself in the public forum, especially in the face of majoritarian intimidation. It is often forgotten that before the introduction of state-mandated mass education in the late 19th century, the only clear grounds for compulsory training was defense of the commonwealth. Not surprisingly, the strongest republican voice in the last two centuries, John Stuart Mill, upset the »pure democrats« of his day by arguing that extending the right to vote was useless unless people were capable to perform the duties entailed by the right. Mill was thus quite happy to couple the right to vote with an obligation to be educated.

Given such qualifications for citizenship, it is unsurprising that republicanism has held an ambivalent place in democratic political theory. If liberty is a social achievement, and not a natural entitlement, then it must be earned. Thus, classical republican regimes fiercely ensured the maintenance of equality among those already free, but they did precious little to extend the circle of liberty. Stories of the founding of republics typically begin by recounting the revolutionary overthrow of a tyrant by an indignant middle class. But republics often end up either becoming empires that support a complacent middle class on the backs of a far-flung and stratified social order (e.g. Rome and arguably the US) or allowing their volatility and divisiveness to expose them to an external stabilizing force (e.g. the fate of classical Athens and Weimar Germany).

To be sure, many republican practices anticipated socialist ones, most notably the redistribution of inherited wealth and agricultural surpluses. This reflects the concrete situation of traditional republican regimes, namely, that liberty adhered to land owners in a specific region who were ipso facto equal stakeholders in the region's future, which was the domain of public affairs. Thus, redistributionist policies were necessary to ensure that economic power could not turn into political power over time. This often meant that the first-born male inherited his father's livelihood but relatively little of the wealth he accumulated in its practice. Moreover, the rotating nature of civil and military service anticipated the Maoist strategy for acquainting freed people with the full range of societal tasks so as to prevent the subversion of egalitarianism by certain jobs acquiring either too much expertise or too little status.

Nevertheless, as citizenship was extended beyond a small middle class, liberty tended to get either »thinned« or »thickened.« In Isaiah Berlin's terms, the thinned version of republicanism is negative liberty, the thickened version positive liberty - or, respectively, *liberalism* and *communitarianism,* as they are ideologically defined in the first chapter of *The Governance of Science* (Open University Press, 2000). Interestingly, Berlin saw these two kinds of liberties as pure types, rather than alternative degradations of republican liberty, as I would urge. This helps us to understand the alien character of republicanism in our time. Republicans would regard as an historical Fall from Grace what modern political theorists would see as the progressive clarification of polar ideals.

Negative liberty has supported unlimited freedom of contract in democratic societies, including the »freedom« to contract oneself into wage slavery and other forms of servitude, in which self-worth is effectively exchanged for money. Thus, in liberal regimes, non-domination is reduced to non-interference, a point originally urged by Thomas Hobbes in his demystified definition of the law as the state's monopoly of force in society. When Jeremy Bentham declared that the republican appeal to »rights« amounted to »nonsense on stilts,« he was reinventing Hobbes in a world where mass enfranchisement meant that the state would have to meddle objectionably in people's lives in order to regain the kind of self-determination vaunted in republican regimes.

Indeed, this Hobbesian reluctance to have the state interfere more than is absolutely necessary has been even carried over into the welfare state. The benchmark is John Rawls' »difference principle,« whereby inequalities in »wealth« (understood broadly to include both money and talent) are justified if the rich are in a better position to benefit the poor than the poor themselves are. Such a policy licenses the »spontaneous« emergence over time of a paternalist social order that would be anathema to republican sensibilities. However, it sets the groundwork for a form of positive liberty that would »free« people from entertaining false expectations about their own life chances. Thus, the state would ascribe roles that enable people to participate »appropriately« in reproducing the social order. An updated version of this strategy is the use of aptitude tests or genetic screening to delimit people's career horizons.

Republicans oppose what negative and positive liberty jointly uphold, namely, the allowance of non-coercive forms of domination justified in terms of either the explicit choice or the »best interests« of the people concerned - that is, the republican diagnosis of negative and positive liberty, respectively. Republicans use the legal system to foreclose these undesirable possibilities. On the one hand, labor-management legislation and a guaranteed citizens' income counteract the desperation and abuses associated with unlimited freedom of contract; on the other, affirmative action and antitrust legislation counteract the advantage accumulated by wealthy groups and corporations over time. This distinctive legislative posture epitomizes republicanism's adherence to three propositions:

(1) There need not be a tradeoff between individual and collective well-being. Indeed, promotion of the individual may be the best way to promote the collective.
(2) Individual and collective well-being should not be identified, respectively, with short and long term benefit. Rather, individuals become better people by thinking in terms of what is best for society in the long term.
(3) Collectively minded individuals need not aim for an absolute consensus of opinion, only for the reversible resolutions of differences whose proliferation is always encouraged.

Those who relish paradoxes may sum up the republican position as favoring the »artifice« of state intervention to prevent domination over any »natural« emergence of domination in the absence of state intervention. Historically, this amounts to a view of politics that combines »liberty before liberalism,« in Quentin Skinner's apt phrase, and what I call »social responsibility after socialism« - at least in the sense that the disagreements fostered in a republican environment are meant ultimately to serve the larger good in a way communitarians could not envisage. A more prosaic account of the differences in conceptions of liberty is presented in figure 1 below:

Fig. 1. Republican Liberty and its Devolutions

Types Of Liberty	Republican Liberty	Positive Liberty (Communitarianism)	Negative Liberty (Liberalism)
Slogan	The Right to be Wrong	Freedom is the Recognition of Necessity	Anything Not Prohibited is Permitted
Liberty's Antithesis	No Legal Protection from Implicit Threats	Anomie Bred by False Self-Understanding	Actual Interference from Other People
Function Of Law	Guarantee Rights	Define Identity	Prevent Harm
Deformation	Liberalism or Communitarianism	Duty To Perform a Role	Right to be a Slave

2. In Search of Republican Vehicles for the Governance of Science

In what respects do the history and philosophy of republican politics provide a model for the governance of science?

First of all, republicanism highlights the ever-present need to constitute the object of governance. There is nothing »natural« about the polity that dictates how it should be governed. Rather, the »who« and »how« of political participation must be formally defined, continuously enacted, and subject to periodic review. This explicitly artificial (or »constructivist«) approach to politics is focused more on the quality of the political process than the quantity of its products - less on the actual number of voters and more on the character of their participation in the electoral process.

Transferred to the scientific realm, republicanism offers a welcomed counterbalance to current discussions of the »governance of science,« which presume that »science« exists simply as a body of systematic knowledge that may be institutionalized by any number of means, ranging from individual minds through state-funded universities to privately funded »research parks.« All one looks at, then, are knowledge-bearing indicators or »outputs,« such as the number of scientific papers, patents, or university graduates. Serious study has yet to be given to the relationship among these indicators and the institutions generating them, wherein lie hidden hierarchies and suppressed perspectives. In this respect, science policy treats knowledge as a spontaneously self-organizing system.

Nevertheless, whenever attempts have been made to study science from the standpoint of its participants rather than its products, the results have revealed that the aggregate measures do not reflect the interests of most of those who are presumed to have contributed to them. A simple case in point is the survey that the American Physical Society conducted of its membership in 1991 on their degree of support for the Superconducting Supercollider, which was strongly pushed on Congress by various Nobel Prize winning physicists. It turns out that the Supercollider was ranked only third, with many expressing reservations that Congressional funding of the world's largest particle accelerator would undermine the support received for less glamorous, but more populous, branches of the discipline, as well as skew the teaching of physics so as to inflate the larger social significance attached to particle physics.

Even more to the point was a survey conducted in 1990 by the US Carnegie Foundation for the Advancement of Teaching in response to perceived pressure on faculty at all tertiary educational institutions to emulate the high-publication model of research universities. Published under the title of »Scholarship Reconsidered«, the survey revealed that despite the perception that all faculty are pressured to publish a lot, only institutions strong in graduate training required it for tenure and promotion. Nevertheless, except for the most elite universities, most faculty (70% overall) saw their »scholarship« better expressed through teaching than academic publications. Moreover, strong graduate-oriented institutions tended to perceive a tradeoff in quality between teaching and publication that was not perceived in the rest of the tertiary sector. And while there remained considerable disagreement over scholarly standards across tertiary institutions, nevertheless there was virtual unanimity that »exciting developments« were taking place in one's own discipline. In the end, more than two-thirds held that means other than publication needed to be developed to evaluate scholarly performance.

What all this suggests is that a republican approach to the governance of science would need to settle upon an institution as the unit of governance, in terms of which criteria for participation and standards of accountability can be developed. There are three possibilities: the *professional association,* the *consensus conference,* and the *university.* None of these institutional sites should be seen as excluding the others, but their historical viability as vehicles of republicanism varies significantly, as indicated below.

The first possible unit of governance is Marxist-inspired. It involves taking discipline-based professional associations as the basis for unionizing the sciences. In that case, science policy would become a form of labor-management relations. Aside from the Marxist baggage, the main obstacle to realizing this possibility nowadays is that the work conditions of scientists trained in a particular discipline have become so diversified as to undermine a plausible case for the kind of »class consciousness« that unions are supposed to foster. This may be seen as a negative unintended consequence of our inhabiting a post-industrial »knowledge society,« a point brought out well by the Carnegie Foundation Report. Yet, there is a silver lining in this cloud. Insofar as the diverse work conditions of scientists reflect those of society at large, then a democratically governed professional association can function as the launchpad for a social movement within which standing conflicts in the society can be expressed, elaborated, and to some extent, resolved. For example, the full range of public attitudes can be found within the community of professional biologists whose internal debates are a microcosm of larger societal struggles.

A second possibility is that academics should be seeding republican projects at large as an extension of the idea of the »open society« that is essential to the spirit of critical inquiry. Too often when we speak of the need for society to become »scientifically literate« or »knowledgeable,« we think purely in terms of increasing the amount of formal training people receive. Here, however, the emphasis would be placed on expanding the opportunities people have to participate in defining the scientific agenda. This may be seen as a continuation of the »extension agency« model of academic public service pioneered by the land-grant colleges in the US in the 19th century. (For a discussion of this prospect, in relation to *The Governance of Science,* see the review essay by James Collier in *Minerva,* vol. 38, no. 1 (2000).) However, in this case, outreach would focus on ideological, rather than technological, innovation. The ideal vehicle is the consensus conference, or so-called citizens jury, whereby a cross-section of the public takes testimony from various experts and interested parties in order to draft policy guidelines for legislation governing a science- or technology-based issue of widespread social concern. (Typically, these have been related to health or the environment.) The citizen-jurors do not themselves make policy — that remains in the hands of popularly elected representatives — but in effect they draft the constitutional framework within which the representatives should make policy. (A recipe for organizing a consensus conference is provided at http://www.medinfo.cam.ac.uk/phgu/info_database/Testing_etc/citizens'_jury.asp)

From the standpoint of lay interest in democratizing science, consensus conferences have continued the contribution of academics to political movements in the larger society. Yet, consensus conferences have also generally produced results that leave the experts »pleasantly surprised,« especially in terms of the public's diligence and fairness in drafting a workable policy framework. (This has been especially true in Japan, which does not have a strong tradition of participatory democracy.) It would seem that politicians are alone in objecting to the insertion of consensus conferences as an institutional »wild card« into the policy process. These are encapsulated in three objections, which I list and answer below:

1) Consensus conferences would seem to usurp the authority of the legislature, indeed, by interpolating academics (who are the typical organizers of these conferences) to act as convenors of a mini-constitutional convention. I respond that the appropriate way of viewing these conferences is as a counterbalance to the »de-republicanizing« tendencies of representative democracy, whereby people come to believe (often with the help of their

elected officials!) that their duty is done once they have voted for their representatives. The long term effect of such de-republicanization has been that parties come to stereotype what is politically possible, and voting comes to be seen as little more than a symbolic ratification of one's vague faith in the political system as a whole. (It is worth recalling that such complacency on the part of the electorate has been defended, under the name of »plebiscitarianism,« as the appropriate response in a complex world best left to expert politicians.) In this respect, consensus conferences ensure that the democratic polity is republican, not just in the legislative chamber, but »all the way down.«

2) Consensus conferences cannot get around the problem of what counts as an adequate »cross-section of the public,« the perennial problem of representative democracies in general. I respond that this objection misses the point of the exercise. There is nothing intrinsically valuable about having citizens juries constituted in certain proportions of the population. A »cross-section of the public« is valuable only insofar as it results in a framework that adequately distinguishes between the personal interests of the jurors and the diverse interests of the larger society, with the understanding that the full range of those interests can never be represented by a single jury. There is evidence from Japan that this is the case, namely, that while jurors do not start by distinguishing their own interests from the collective good, they do so by the end of the consensus conference. In fact, often they became clearer about their own reasons for, say, refusing gene therapy for themselves yet allowing others to receive such treatment if they wish.

3) By what authority do academics - as opposed to anyone else - claim to conduct consensus conferences? What makes them the unique keepers of republican democracy? I respond that first one needs to look at the competition. In Tony Blair's Britain, it is common to conduct »focus groups« before legislation is proposed. Marketing consultants and think-tank dwellers (often armed with sociology degrees) are employed for this purpose. Their basic strategy is to start with the government's general policy aim and then adjust its expression according to the focus group's response, so as to assure maximum agreement. In other words, the public is not offered the chance to make an independent assessment or to resolve internal disagreements in non-consensualist terms. In contrast, the relatively detached situation of academics (i.e. their jobs do not depend on the outcomes of the conference) enables them to avoid this pattern. Moreover, the perennial pedagogical concern of academics with the next generation

of societal leaders means that their sense of »policy« is bound to be broader than that of any particular government, or consultancy working on its behalf. The breadth of the academics' temporal horizon is significant, since any major legislative initiative is likely to have consequences beyond the voters in the next election.

This last point raises the third possibility, namely, the university as the principal site of republican governance of science, which will be the focus of the remainder of this piece.

3. The University as the Ultimate Republican Institution

Universities were originally chartered in medieval Europe as politically autonomous, self-funding, limited liability »corporations,« with many of the same characteristics of the ancient republics. Unlike the often much larger academies of Islam, India, and China, the medieval universities were not directly beholden to state or private benefactors and hence could survive changing conditions in the larger society. Indeed, universities were often encouraged to fill vacuums in political and economic leadership, as long as they did not attempt to subvert the keepers of the legal system that enabled their existence. Thus, universities came to perform at least three functions: they completed the family's role in educating the next generation of elites, they offered professional training for civil and ecclesiastical posts, and they continued the ongoing project of synthesizing disparate forms of knowledge into a common cultural inheritance. As we shall see in section 4, in the modern period, these three functions have come to be differentiated and set against each other, representing what, after Bjorn Wittrock, I call the *British, French,* and *German* models of the university, respectively.

Both universities and classical republics are predicated on the presence of an external enemy that threatens all their members equally. Regardless of their other differences, academics and citizens, respectively, can always focus their political energies on how to deal with this foe. In the history of republics, the foe has tended to be a larger empire or political entity that threatens to obliterate the republic's autonomy and hence the liberties enjoyed by its citizens. The common foe that has confronted the university has been alternatively called »error,« »falsehood,« »prejudice,« or »ignorance.« The university has traditionally tackled this common foe through *curriculum design,* which functions much as »foreign affairs« does in republics. In both cases, it provides the backbone of governance, underwriting both the autonomy and dynamism of republics and universities. Deft curriculum design has prepared the conditions for the wider reception of the innovative and often controversial research done by the faculty. Even during the Scientific and Industrial Revolutions, when the universities were rarely the source of new discoveries or inventions, they nevertheless helped to normalize those developments as part of the public knowledge of the day — if only by immunizing the next generation of elites against a knee-jerk negative reaction to innovation and controversy.

Thus, rather than being a burden on the free spirit of research, teaching has conferred legitimacy and relevance on ideas and techniques that would otherwise fall victim to either benign neglect or charges of blasphemy. Toward this end, curriculum design has compelled the maintenance of a lingua franca for a single academic subject and sometimes even the entire university. (In *Three Rival Versions of Moral Inquiry,* Duckworth, 1990, Alasdair MacIntyre has claimed as much with respect to the discourse of Christian doctrine as the university's lingua franca before the Enlightenment.) This has enabled the expression of intellectual disagreement in ways that have had larger societal import. Indeed, one should not underestimate the long-term role that the scholastic artifice of reducing complex differences of opinion to binary oppositions has played in fueling the political imagination. The intellectual basis of virtually every modern social movement is a »us-versus-them« dichotomy such as »nature versus nurture,« »rulers versus ruled,« or simply »traditional versus modern.«

Moreover, just as republicans would have it, universities have been traditionally run according to a system of checks and balances in which faculty and students exercised mutually countervailing powers. Faculty determined the curriculum, but students could voice their objections by refusing to pay for lectures, which would ultimately - albeit indirectly - affect course content. In the medieval period, the university's corporate character was modeled on the trade guilds, where students were positioned as apprentices in their field of study. However, the British settlers in 17th and 18th century America innovated the idea of the university as an *independent church* that, lacking either a state monopoly or a natural clientele, had to actively solicit its student base as a constituency that would support the institution even after graduation. This »evangelical« attitude has anchored higher education policy in the US more generally, even in the public sector, most notably through the philanthropic mission of university alumni associations.

This point raises the important issue of the political economy under which universities have been able to retain their autonomy as republican institutions - especially in the face of a changing environment. We have already seen that the republican sense of liberty is typically lost under these circumstances, hence the rise of positive and negative liberty as alternative ideals. In the history of the university, there have been two basic autonomy strategies, which have involved academics in, respectively, a *priestly* and a *monastic* mission.

In the priestly mode, universities *expand* to become sovereign states in their own right, or at least to acquire many of the powers typically held by states. During the politically disorganized Middle Ages, this was a common path for universities to take, given that they were in the business of training the next generation of civil and religious officials. In the 19th century, this strategy was updated for a modern secular society in the land-grant mission of American universities. In the monastic mode, universities contract so as to be free of benefactors who might try to pervert the path of inquiry with their donations. In chapter six of *The Governance of Science,* I proposed an updated version of this ascetic orientation, namely, that universities would refuse big corporate grants in favor of functioning as critics, quality control checks, and »reverse engineers« - all in the aid of removing barriers to the free flow of knowledge. In this respect, the innovative function of universities would be limited to opening up channels for the distribution of already existing knowledge.

These autonomy strategies are traceable to the period immediately preceding the founding of the first universities. Two 12th century French clerics, Bernard of Clairvaux and Peter Abelard, are the »patron saints« of the priestly and monastic strategies, respectively. Bernard's academics would stamp out heresy and consolidate religious orthodoxy as part of the proselytizing mission of the Church, whereas Abelard's would relentlessly pursue dialectics to bring out the strengths and weaknesses of both sides of any issue, so as to drive home the fallibility of any humanly designed orthodoxy. To be sure, both the priestly and monastic modes are susceptible to pitfalls analogous to those that befell the religious orders on which they are modeled. In theological terms, the priestly mode is susceptible to *simony,* the monastic mode to *acedia.* On the one hand, an expanding university is open to corruption as potential benefactors try to buy their way into the research and teaching agenda; on the other, a contracting university may withdraw into itself in a cynical spirit that doubts the ultimate efficacy of the university's mission, which many today associate with the »postmodern condition«. The existence of these pitfalls points to the sorts of environments in which the priestly and monastic modes tend to operate. The former requires a permeable boundary between the public and private spheres, whereas the latter requires a much harder boundary. I believe that much of the ideological content of the »culture wars« in US universities over the past quarter century can be captured in these starkly polarized terms, if we think of St. Bernard as the patron saint of the »Right« and Peter Abelard as that of the »Left«.

However, matters elsewhere have not been so clear-cut, and so more complex models might be welcomed. In chapter two of Thomas Kuhn: *A Philosophical History for Our Times* (University of Chicago Press, 2000), I wrote about the Faustian bargain struck in the early 20th century by Max Planck and other defenders of the German universities, in the face of growing public demands, coupled with a growing dependency of universities on public resources to support their own activities. Basically, the bargain - one replayed in all the other major national university systems - consisted of academics agreeing to set examinations and curricula that provided the new (»democratic«) basis for societal reproduction and stratification, in return for a largely free research culture. In other words, the dynamic function of the university was contained to an increasingly esoteric arena (the research culture) that was removed from the stabilizing function that the curriculum now had to make its public face. This bargain remained intact throughout most of the 20th century, and indeed inspired the sociological side of paradigms presented in Kuhn's *The Structure of Scientific Revolutions* (University of Chicago Press, 1962). However, with the end of the Cold War and the divestment of what Alvin Gouldner originally called »the welfare-warfare state,« the Faustian bargain is unraveling. It is to this that we now turn.

4. Historical Threats to the Republican Constitution of the University

However, the status of the university as a regulative ideal of institutionalized inquiry is in danger of being lost today, as academic culture comes to be divided between what European science policy gurus, after Michael Gibbons et al. (in *The New Production of Knowledge,* Sage, 1994), have dubbed »Mode 1« and »Mode 2« knowledge production. These two modes effectively re-enact, respectively, the communitarian and liberal devolutions of republican liberty previously outlined in figure 1. The full array of differences among the three modes of science governance is captured in figure 2.

Fig. 2. Three Modes of Science Governance (Based on *The Governance of Science*)

Scientific polity	Republican	Communitarian	Liberal
Mode of knowledge production	Mode 0 or 3?	Mode 1	Mode 2
Motto	"Right to be Wrong"	"Safety in numbers"	"Survival of the fittest"
Instantiation	Dynamic University	Fixed paradigm	Temporary network
Defining virtue of science	Inclusive reliable access	Certified reliable knowledge	Increasingly effective action
Intrinsic value of knowledge	"Public good" (non-excludable and non-positional good)	Consensus (excludable good)	Utility (positional good)
Extrinsic value of knowledge	Critique (inquiry serves by changing social practices)	Ideology (inquiry constrains other social practices)	Technology (inquiry subserved to other social practices)
Metascience	Institution-centered social epistemology	Domain-centered epistemology	Individual-centered research ethics
Attitude to risk	Collectively encouraged	Collectively avoided	Individually absorbed
Cumulative advantage	Regularly redistributed	Academic lineage	Track record
Source of disadvantage	None	Ideological (political correctness)	Financial (unfunded grants)

In the history of the university, the difference between communitarianism and liberalism is most felt in the difference between *too much* and *too little* concern about the disruptive consequences of innovation for society. Kuhn's conception of »paradigm« as resistant to innovation until a research tradition has run its course is perhaps the clearest example of the conservatism of Mode 1, to which Mode 2 is supposed to provide welcomed relief by opening up knowledge production to a wider range of influences. Indeed, most interdisciplinary fields in the natural sciences (and even some in the social sciences, including cybernetics and some area studies) owe their existence to academics frustrated with the disciplinary boundaries of their universities and hence susceptible to the lure of strategic research initiatives from both the public and private sectors. However, while the innovations may have been made outside the academy, their institutionalization ultimately depended on the establishment of degree-granting programs at universities.

Thus, Mode 2 is at most a *temporary* antidote to Mode 1. This is a point that we are in serious danger losing, as universities are increasingly forced to compete on a »leveled playing field« with other institutions that attempt to fulfill only one of the university's many traditional functions, such as science parks (for research) and online degree courses (for teaching). The 19th century is the source for the three modern models of the university, each associated with the historical experience of a European nation: Britain, France and Germany. Each tradition evokes a totemic figure: Cardinal John Henry Newman, Napoleon Bonaparte and Wilhelm von Humboldt. I shall review each type and then comment on more recent developments that have altered (perverted?) the ideal it represents. The three types are summarized in figure 3.

Fig. 3. The Post-19TH c. Deformations of the University's Republican Spirit

Ideal type	Britain	France	Germany
Totemic figure	Cardinal Newman	Napoleon Bonaparte	Wilhelm von Humboldt
What is promoted?	Development of the whole person	The interests of clients	The advancement of the discipline
Student commitment	Lifelong	Temporary	Either or both
Faculty commitment	Collegial	Contractual	Departmental
Quality control	Entrance exams	Licensing board	Peer review
Economic model	Fiduciary institution	Monopoly	Multi-product Firm
Income source	Rent (of reputation)	Profit (from contracts)	Wage (for work)
Recent ideology	Principal-Agent Theory	Credential Society	Economies of Scope and Scale
Recent theorist	Arthur Stinchcombe	Peter Drucker	William Baumol
Achilles heel	Academic Bullionism	Poor Accountability	Competing Standards

These three ideal types basically accentuate aspects that had been integrated together in the training of elites in the ancient Greek academies and original medieval universities. However, Europe's political reorganization around a system of competitive nation-states undermined this pre-modern unity of mission. Interestingly, within its own borders, the United States has permitted each type to flourish, respectively, as liberal arts colleges, land-grant colleges, and research universities. But then again, as a federally constituted republic, the US has never pretended to have a coordinated national system of education and research.

According to the British ideal, the university (think Oxbridge) is located on a campus and is governed »collegially«, which is to say, on the basis of academics inhabiting a common space without necessarily having common intellectual interests. These colleagues relate to students as an extended family *(»in loco parentis«)* through a personalized tutorial system. Students are evaluated as much for their »good character« as for their academic performance. The pursuit of knowledge is clearly a means for creating the well-rounded person. In less flattering terms, the university is a glorified »finishing school«. Thus, students typically come from wealthy backgrounds and are expected to assume leadership positions upon graduation.

According to the French ideal, higher education is led by glorified polytechnic institutes that are explicitly entrusted with advancing the fortunes of the nation that subsidizes their existence. Thus, faculties of these *grandes ecoles* may be organized around, say, »agriculture« and »mining«, as opposed to such conceptually defined object domains as »physics« and »chemistry«. Value-for-money at the level of teaching and research translates to a system of clients and contracts. Here we see the greatest concern with demonstrating an overall increase in »human capital« (»credentials«, for short) as determined by state licensing boards.

According to the German ideal, the university is dedicated primarily to the pursuit of knowledge for its own sake. This explains why its institutional structure appears arcane to non-academics, organized as it is around disciplines and not social functions. Yet, at least at the level of rhetoric, this model seems to have had the most lasting impact. Here the university is officially detached from the concerns of state and regulated primarily by »peer review« mechanisms. This autonomy is justified on the grounds that policy would benefit most from knowledge that has undergone specialist scrutiny, resulting in its collective authorization by a specialist journal. Not surprisingly, Ph.D. training turns out to be the cornerstone of this model.

Each model implies a distinctive political economy, which by the end of the 20th century has exhibited its own distinctive »deformation«.

The British model envisages the university as a fiduciary institution, in which students pay academics to make choices on their behalf that they themselves do not feel competent to make — with incompetence potentially carrying a heavy toll. (See Arthur Stinchcombe, *Information and Organizations,* University of California Press, 1990.) The choices concern the sort of knowledge that students need to succeed in life. As the students' entrusted agent, the university then engages in informed speculative investments, namely, the hiring of the most distinguished faculty. However, this process can be deformed, if there is no clear relationship between the accumulation of »big names« and the quality of education that students receive. A good example is the sharp distinction in performance standards used in teaching and research, with the latter carrying greater symbolic and financial value. The result is what Adam Smith might have recognized as »academic bullionism«, that is, the possession of big names who look good on university brochures but do little to increase the competence of those who pay their salaries.

On the French model, higher education is a state-sanctioned monopoly that licenses the practice of the liberal professions: law, medicine, engineering, and more recently something called »business«. The attractiveness of this model lay in the provision of quality control in areas where academic knowledge interfaces with public and private interests. Yet, interestingly, the person most closely associated with the idea that ours is a »knowledge society«, Peter Drucker, has long denounced the deformation attending this model: namely, the university becomes a place where excessive fear of charlatanry and the jealous guarding of guild privileges stifle the spirit of experimentation and innovation (see, e.g. Drucker's *Post-Capitalist Society,* Harpercollins, 1993).

Finally, the German model of the university is that of a multi-product firm - in this case, one devoted to both teaching and research. This multiplicity of functions constitutes the »scope« of the university. In theory, the university should become better in performing both functions as it gets larger, since that would allow for »synergies« between teaching and research. The exposure of more students to the latest research should produce at least a more supportive public environment for new research and perhaps even new recruits to the ranks of researchers. Thus, as William Baumol has argued, an increase in the scale and scope of the university should be positively correlated. (See Baumol's »On the proper cost tests for a natural monopoly in a multi-product industry«, *American Economic Review 67* (1977): 809-22.) But, once again, this rosy picture is deformed by the divisive performance standards for teaching and research, in which the former pulls toward ever more credentially enhanced students and the latter toward ever more multiply cited publications. Consequently, academics and students face each other more as obstacles than enablers of their respective ends.

5. The Challenge of Contract Academic Workers to the University's Republican Constitution

The suboptimal academic worlds generated by the British, French, and German schemes may be summarized as follows. The British university retains its autonomy by mystifying its relationship to society, while the French university loses its autonomy by becoming an extension of society. The German university arrives at a hybrid (»Faustian«) conception of autonomy, whereby it agrees to provide the basis of societal reproduction in a democratized polity, while it abdicates all responsibility for the circulation of its knowledge products. Thus, research autonomy comes to be equated with the freedom to ignore the consequences of one's research. This mentality probably reached its apotheosis in US Cold War academia. However, with the end of the Cold War and the gradual devolution of traditional academic functions to the private sector, we see the rise of »contract academic workers« who are temporarily funded by some combination of academic and non-academic interests.
To Mode 2 devotees, these workers are the wave of the future - but the future of what?

Here are three relevant facts:

1) As an increasing percentage of new researchers (or Ph.D. recipients) are unable to get regular academic posts immediately, they must rely on some sort of external funding for their livelihoods. How can the interest of such contract-based researchers be sustained in the peer review processes by which academics have traditionally decided which potential contributions to knowledge are worth funding and publicizing? These itinerant workers are typically equipped with the most up-to-date knowledge and skills in their fields, yet the conditions under which they work are not conducive to the support of peer review and allied academic practices.

2) Most government-based research councils require that a grant's »principal investigator« hold a regular academic post, which rules out anyone employed on a temporary basis. Most articles submitted for publication to academic journals are refereed by established academics on whom the editor has previously relied. These people are not only distinguished in their fields but also employed in environments that recognize the significance of their participation in such reviews. Contract-based researchers do not enjoy this indulgence: any time spent refereeing a colleague's article is time taken away from completing one's own time-dependent project or writing a proposal to ensure one's continued employment.

3) Most universities will not accord the same welfare and legal coverage to temporary staff as they do to regular staff. This is an open invitation to worker exploitation, not only in terms of the number of hours that may be demanded of contract researchers but more importantly the appropriation of such researchers' intellectual property by the universities that temporarily house them. Peer review is thus not unreasonably regarded as a euphemism for the proverbial »old-boys network«, where the old get richer, the young poorer.

Given these three conditions, universities may soon become the site of class conflict of a magnitude that was previously confined to the industrial sector of the economy. However, this conflict is unlikely to be manifested in work stoppages, property damage, and strikes. Instead, it will be marked by a gradual withdrawal of services from the universities by many of the most academically talented people of the current generation. Their contract-based status will encourage them to see teaching in opposition to research, and hence the university as little more than a necessary evil for the proper conduct of their work. Indeed, these people may be amongst the first to call for greater public accountability for universities in ways that may eventually serve to undermine the autonomy of academic institutions altogether.

Unfortunately, academia continues to harbor considerable snobbery toward contract-based research, including a widely held folk psychology of contract researchers as more interested in the pursuit of profit than knowledge. We often presume the inherent attractiveness of contemporary academic life to such an extent that we can only ascribe base motives to those who might resist its charms. Yet, these charms are largely byproducts of the very thing that contract-based researchers lack, namely, secure employment with clear career prospects. Absent those conditions, one is simply presented with an indeterminate labor market where one must live by his or her wits by taking advantage of opportunities as they arise. By those standards, it is not surprising that the dynamic environment of corporate culture increasingly lures people whose ties to academia have become tenuous.

Moreover, when academics try to defend themselves from the onslaught of corporate culture, they rarely seek to protect the university as such; rather, they more parochially aim to shore up the integrity of discipline-based inquiry, as against the interdisciplinary and (allegedly) methodologically sloppy work produced by client-driven, contract-based research. In short, Mode 2 is met with Mode 1 - not a more republican mode 0 or 3. Here we see the most pernicious feature of the Myth of the Modes: The two modes are seen as not merely mutually exclusive, but also jointly exhaustive - that is, not admitting of other possibilities. In particular, missing is the idea of the university as an autonomous site for knowledge production that constitutes a whole that is greater than the sum of its disciplinary parts. This idea, the foundation of liberal education, kept disciplinary proliferation in check by regularly forcing academics to consider how their disparate research agendas contributed to a common curriculum that would be imparted to the incoming cohort of students. This curriculum, in turn, provided a basis for the university to present a unified image of itself as a critical force in society's self-reproductive processes.

As I showed in the earlier sections, the historical realization of this ideal has been always less than perfect, but at least one could rely on the presence of some academics to defend the ideal and provide institutional buffers to its complete corruption. Such was the case when academic administrators were recruited from the ranks of the teaching and research staff. However, increasingly academic administration is itself seen as requiring skills akin to those needed to manage business firms with a similar range of personnel and organizational structure. (As usual, the US is ahead of the curve here.) Not surprisingly, these administrators actually encourage academics to conceptualize their work in terms of the discipline-based criteria associated with Mode 1 knowledge production, partly because that is the easiest way to get academics to mimic the productivity-orientation that governs Mode 2 knowledge production.

But can this demystified strategy work in the long term? Specifically, as universities shed the ideological and institutional trappings that traditionally distinguished them from business, can they be reasonably expected to compete with business in attracting and keeping the best research talent? Academia continues to send mixed messages about the need to publish in peer-reviewed journals while, at the same time, encouraging the pursuit of intellectual property that typically takes research out of the public domain and may even mask the contributions of the original researchers. In business, the situation is more brutally straightforward: Research is primarily aimed at intellectual property, and researchers are paid sufficiently well that the reversion of patent rights to the company is not regarded as an injustice.

This problem here is traceable to the terms on which the experimental natural sciences were incorporated into the universities in the late 19th century, originally in Germany. The natural sciences were not part of the original framework for the constitution of liberal education that justified the university as an autonomous institution of higher learning. Not surprisingly, Thomas Kuhn found physics and chemistry to be most conducive to his conception of scientific inquiry as paradigm-driven, which approximates the insular ideal of Mode 1 knowledge production. Yet, within a generation of their admission as proper subjects for academic training, the natural sciences were involved in large-scale projects collaboratively sponsored by government and industry. In Germany, this was epitomized by the Kaiser Wilhelm Institutes, the precursors of today's Max Planck Institutes. They marked the institutional dawn of Mode 2 knowledge production. In short, Mode 1 and Mode 2 were born largely as opposite sides of the same coin, which was the recognition of experimental natural science as proper academic subjects.

The humanities, which up to that point had dominated the universities, were never as narrowly insular as Mode 1 implies but neither as readily adaptive to external pressures as Mode 2 implies. Self-declared »postmodernists« in the humanities too often forget that common to the multiple coinages of the expression »the postmodern condition« in the 1970s by Daniel Bell, Stephen Toulmin, and Jean-Francois Lyotard was a belief that 20th century developments in the natural sciences (broadly construed to include mathematics and computer science) had irreversibly destroyed the unity of knowledge ideal, which Lyotard quite explicitly associated with the constitution of the university.

It is important to recall this history because the distinctive voice of the university can be easily lost in this »brave new world« of »knowledge management« and »knowledge societies« but in practice little more than the ultimate extension of the labor policies of industrial capitalism. So far the »partnerships« forged between academics and their clients in government and especially business have had rather asymmetrical consequences. Academics have adopted many of the performance standards of their partners, including measures of productivity, efficiency and even customer satisfaction, all of which have served to alter substantially how academic work is conducted and evaluated. But what have government and business learned from academia? It would seem not much yet. Nevertheless, contract researchers potentially function as academic ambassadors in stressing the values associated with rigorous long-term thinking, which courts counter-intuitive conclusions and takes into account a wider range of »stakeholders« than simply those who are able to shout the loudest or pay cash upfront. The fact that several firms have begun to invest in the funding of doctoral programmes for their middle management personnel suggests that they have begun to see that certain intellectual traits can be gained only through an immersion in traditional university values. (An account of an important Swedish initiative, Fenix, may be found in M. Jacob and T. Hellstrom, eds., *The Future of Knowledge Production in the Academy,* Open University Press, 2000.)

6. Conclusion: A Republican Agenda for the Academic CEO of Tomorrow

The days of appointing a university's chief executive officer (»vice-chancellor« in the UK, »president« in the US) from the ranks of senior academics is quickly drawing to a close. There is no reason to think that the average successful department chair, or even faculty dean, has the competence needed to defend the university's autonomy in the face of competing institutions and pressure from both the public and private sectors. However, this does not mean turning over the reins of the university to a professional manager (of the Harvard Business School variety) who thinks that a university can be run like any other organization of comparable scale and scope. Rather, it means that academic administration requires its own course of study and accreditation, which would in turn be the new breeding ground for the republican spirit.

The victory of both liberalism and communitarianism over republicanism in higher education today can be measured by the »organizational dumbness« of universities, a fact much remarked by professional knowledge managers. In the UK, this dumbness is most clearly manifested in the enforcement of mutually exclusive, even competing, standards for the evaluation of teaching and research, thereby ensuring that the university will appear to be a suboptimally performing — if not downright incoherent — institution. Not surprisingly, academic administrators seem to be little more than glorified custodians who make sure that the university's space is efficiently managed and that staff are sufficiently happy to remain in their allotted places. The republican antidote to this pervasive mentality is a reassertion of the university's corporate identity, above not only non-academic interests (Mode 2 liberalism) but also narrowly disciplinary ones (Mode 1 communitarianism). Toward this end, I propose the following three point agenda:

1) Universities must treat expansion or contraction as genuinely alternative modes of survival. Bigger is better only if there is no state support, and hence universities must turn to the private sector. This state-of-affairs would probably indicate a contraction of state control more generally, and hence universities should enter into an expansionist mode as a state-like corporation, providing services to the larger society that are not likely to be provided by a purely market-driven corporate entity. However, if there is strong state support, then universities best retain their autonomy by remaining small and critical of the dominant market-driven forms of knowledge. Both possibilities have the advantage of avoiding the Faustian bargain that universities have struck in the 20th century, which I discussed briefly at the end of section 3.

2) Assuming, in our post-welfare-state world, that university expansion is the more realistic alternative, there are two radically different models for conceptualizing the »private sector« corporation. One is the fast-food franchise based on satisfying specific but transient consumer demand; the other is the independent church based on servicing vaguer but deeper dispositions in people's lives. Despite the dominance of the former model, I would strongly urge the latter. This means inter alia shifting the burden of funding from market-driven student tuition fees to lifelong alumni support (which is then used to finance scholarships). To motivate this shift, consider that a church that devoted all its resources to baptism in the faith and schooling in its catechism but ignored the interests of adult members would be always struggling to make ends meet and probably deemed dysfunctional. Here the US enjoys a definite historical advantage. Even strong state-subsidized universities in the US have long flourished as de facto churches (where football games function as Mass). However, recent calls across the world to privatize state-run universities typically aim to reduce the utility curves for teaching and research to the short-sighted ones associated with fast food. The »religiosity« of churches seems to blind people to their organizational virtues.

3) Finally, to regain their republican potential, universities must take a principled stance against the inheritance of academic advantage. To be sure, affirmative action and positive discrimination in the selection of students and staff contribute to this end. However, the principle must be applied thoroughly, so as to prevent the emergence of academic dynasties whose overthrow is justified only when the dynastic heirs have failed in their own terms. (This is basically the conservative theory of revolution as caused by the need to restore natural order, the scientific version of which is Kuhn's theory of paradigm change.) In The Governance of Science, I make a number of proposals designed to subvert inherited advantage. These are summed up in the word »fungibility,« namely, that no form of inquiry should command so many resources as to crowd out most competitors. There are many ways to satisfy the fungibility condition, not least of which involve regularly scheduled elections to determine the distribution of research and teaching resources - and, as illustrated in the German academic tradition, this may extend to the selection of the university's own chief executive officers.

Insiders and Outsiders Unite!

Science and Science Studies in/of the 21. Century

Andreas Roepstorff*

Abstract

In the current discussion of science, one finds a struggle between the outside description of science conducted by those studying the sciences without being (natural) scientists, and the inside understanding by those who undertake science. Based on an ethnographic study of a scientific institution, it is argued that this distinction is not a fruitful one. It fails to capture that science is not only a mundane practice for scientists, but that it is also plays a general and important role as part of a modern social order. An order, that presently appears to undergo rapid transformations The outsider-insider distinction therefore fails to correctly identify the current pressures on science and it is useless for the important discussion on which role science ought to play in the future.

Introduction: Imagining Science and Pressure

We learn from the heading of this seminar that Science is under Pressure. That is indeed a very strong image that, as all versatile metaphors, in itself is empty of any real content since it is applicable to all sorts of understandings of science, from the day-to-day interactions of concrete persons, to the high-polished metaphysical entity of Science. As a scientist sometimes studying other scientists it is my experience that neither the 'pressure' nor the 'science' notion has an unequivocal content. However, this does not imply that science and pressure can never be given any meaningful interpretation. Try to conceptualise a metaphorical co-ordinate system where the different understandings of Science is on one axis, and the different understandings of Pressure on the other. This generates a two-dimensional semantic landscape of science-pressure. This landscape is not uniform, rather it appears two have two main attractors, each containing a science-pressure complex with particular meanings condensing around each of the concepts. This was at least how I experienced it as I worked through my material with the 'science under pressure' metaphor in mind. In order to outline which understanding of science occurs in these attractors, I will in the following recapitulate the process of semantic condensation as I experienced it. Through four encirclements, that bring us to

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Russian biologists, Martian anthropologists, European brain mappers, and Insiders and Outsiders, I will give shape to the two science-pressure attractors and eventually connect them.

First encirclement: a statue and a Russian biologist

At first the heading Science under Pressure evoked to me a recurring, vivid picture: A bright monolithic instance, Science, consisting of a sculpture-like statue of ever changing contours that was being squeezed by a heavy weight in a scenario somewhat similar to an old, used car being transformed into a cubic piece of scrap metal.

My next mental flash of imagery was evoked memories from doing research with and among scientists in the former Soviet Union. They condensed around an e-mail I had received a few days earlier. It was written by a Russian biologist, AT, and it reached me through an extended network of researchers. The Russian scientist introduced himself in the following way:

> »I am a Russian marine biologist. Among other things like marine ecology, invertebrate taxonomy & morphology, I am interested in the history and recent state of traditional coastal fishery along the Russian European North and Far East. During 1997 - 1998 I headed a study of the recent state of coastal villages and a study of recent state of traditional modes of nature use in these villages.
>
> To my mind, traditional coastal fishery [in the XVI - XX centuries in this area] is an interesting example of self-governance of local resources. Now, after 70 years of total State regulations of fishery (and of course the same time of every-day poaching) fishermen in the coastal villages are intuitively trying to work out some self-regulations. On this way they face with misunderstanding of local authorities, and State Fishery inspection......
>
> No doubt I am a dilettante in social sciences. I am looking for colleagues, who will be interested in discussing problems of history and recent state of coastal fishery, and in particular coastal fishery in Russia....

> Now I am trying to find funds to continue the work, By various reasons, local authorities in the moment still are not going to support the project (I think they need few more years, to realize that they are not able to solve conflicts and frictions in this field, and of course they just have not money even for more actually needs). That's why I look for additional funding. Probably you can advice me the foundations, which could be interested in such studies.«

It was not initially obvious to me why this first encirclement of the Science under Pressure theme would connect a statue, a car breaker's machinery and a Russian biologist turned sociologist, but I was probably experiencing the power of metaphors in creating vivid imagery in so-called blended mental spaces. If one asks experts on the mechanisms of semantics, such as the Danish semiotician Per Age Brandt (e.g. 2000), this is how metaphors build up complicated meanings from more concrete elementary semantic relationships. By backtracking the elements involved in this mental blend, it is, therefore, possible to get a grip of the meaning of the notions that went into the metaphor.

In the first imagery, the abstract notions of science and pressure were being transformed into physically embedded representations: a massive, brutal piece of metal threatening to impose its weight by the force of gravity on a beautiful, refined transformable object of human construction, presumably a representation of science. The next imagery was simultaneously much more concrete and much further removed from the *Science under Pressure* metaphor. It did not directly embody abstract notions like science and pressure. Rather it evoked a concrete person: a Russian scientist trained in one field, but broadly interested in all sorts of other things. Working against the ideas of local authorities and on behalf of a good cause, he was sending messages out in the world asking to be embedded in a network of other researchers hopefully with access to the necessary material resources to allow for a continuation of his work.

Apparently this stereotype of the post-Soviet condition for researchers was another archetypal example of Science under Pressure. On second thought, the biologist TA epitomised a situation experienced by many of my ex-Soviet colleagues. They are well-educated broad-minded individuals who have been trained to value the accumulation of knowledge in 'Science', understood as a transcendental field not directly under local political control, as an important field of human endeavour. An endeavour they continue to contribute to and identify with although the conditions are now very difficult and the personal costs immense. The situation for many scientists in Russia the last 10 years thereby appears to express in an almost cosmological manner central conflicts within (West)European metaphysics surrounding the strange notion of Science.

This is not the place to discuss why Russia continuously appears to produce 'doomsday hyperversions' of European metaphysical conflicts. It is neither the place for a hagiographic description of how science after all is made in Russia in spite of all sorts of eternal pressures. It was, however, striking that the loose line of associations, which the title for this seminar kept condensing around, led me to seemingly unrelated images of cartoon-like representations of a human construction being crushed and to distant networks of people doing the impossible.

This tension between two images: On one hand the abstract Science, capital S, threatened by Pressure, capital P, and the much more diffuse and seemingly unrelated image of networks of interacting people busily involved in conducting science reflects a tension inherent in my understanding of the concept that appears to point to more general properties of this strange entity. It is namely similar to the distinction employed by Steven Fuller (1997: 25-26) between a *philosophical, substantive* definition of science as a general institution on one hand and a *sociological, functional* definition on the other describing actual scientists involved in actual work. Although Science, capital S, is the title of the quoted book, the sociologist Fuller does not appear to believe in the internal consistency of this notion. He is, rather, busy debunking the whole concept, and in that he finds support in a strange manuscript apparently made by some radical outsiders observing the scientific community: a group of Martians presenting a provocative analysis of the Earthlings' relation to science. We will, for the second encirclement, follow their arguments in some detail.

Second encirclement: a Martian outsider's perspective on Science

In *Science* (Fuller 1997) there is a chapter called *Science as Superstition: A Lost Martian Chronicle* (op. cit: 40-79) allegedly paraphrasing *The Martian Ultraviolet Paper on that Distinctively Superstition Called Science,* an essay written by a group of Martian anthropologists studying Earthly devotional rituals. The paper presents a summary of an outsider's view on this strange multi-facetted set of phenomena that earthlings claim to group under the common heading of Science.

In struggling with the intricacies of science, the Martian anthropologists decide that the central research question must be a classical anthropological one: The determination of whether humanity's faith in science is superstitious (op. cit. p. 47). In order to answer that question, they structure their analysis around five classical notions from the sociology of religion: *mystery, soteriology, saintliness, magic causation* and *theodicy* (Weber 1993).

The Martians aptly demonstrate that just as these five categories were useful for Max Weber in understanding, as a non-believer, the sociological role and function of religion, they can be used to organise a sociology of science that will not take the sayings of the insiders of science for granted. The Earthling's understanding of Science appears to the Martians to be full of superstition in Malinowski's sense (1954), since they appear to see 'science' as a magical entity that in itself has an intrinsic causality. To the Martian observer this causality does not emanate from Science itself. Science as an institution should rather be understood in relation to other causally relevant factors of a social nature.

This is best exemplified in the two appendices to the paper. Here the Martians demonstrate how the famous four norms describing the modern scientific ethos for the scientific insider *Universalism, Communism, Disinterestedness,* and *Organised Scepticism* (Merton 1942) to the Martian outsiders have a completely different make-up. They appear like *Cultural Imperialism, Mafiosism, Opportunism,* and *Collective Irresponsibility* respectively. Finally the Martians demonstrate how the Science Citation Index, the most widely used evaluative standard both within and outside of the scientific community, is not just a neutral representation of the activities of scientists and their long-term impact. It is, the Martians claim, an actively used battlefield where individual scientists strategically use their choice of citations in articles as a ballot system »inflating the citation counts of their colleagues who are regarded as even marginally powerful« (op. cit. p. 71). In epistemological terms, a figure in the citation index is therefore not just a *natural* kind that passively reflects reality, it is an *interactive* kind (Hacking 1999; Roepstorff 1999) that, since it feeds back on reality, is actively being shaped and used by the involved scientists.

The analysis of the Martian Outsiders does, according to Fuller, cast serious doubts on the actual content of science as described by the scientific Insider. The obvious conclusion is therefore that there is no 'nature' to Science in general. Along the lines of the Religion-Science comparison, the Martian anthropologists therefore suggest that the best parallel to an understanding of the future of Science is to study the history of the World Religions. In particular they emphasise the period when »Christendom was secularized when the emerging nation-states of Europe in the seventeenth century refused to grant a single Church special economic and political privileges. This led to a period of evangelism, in which religious believers competed to attract believers who would materially sustain their effort« (op. cit. p. 60). In the current science funding situation, the Martians see a similar process of de-coupling between the (nation)state and science as an institution, and they envisage a future of »humans embarking on a second Enlightenment, one in which science continues to enjoy popular support even after, like Christendom, its sacred status and state support have been removed« (op. cit. 62).

Being myself an earthling anthropologist studying earthling scientists, I have been creatively provoked by the Martian perspective. I would therefore in the following like to examine the extent to which the Martian analysis appears to be applicable to my recent research. The third encirclement will therefore take us to a leading brain-imaging laboratory.

Third encirclement: the laboratory as a temple

As part of a research project on the ethnography of mind-brain researchers, I recently spent half a year at a scientific institution that specialises in examining the neural basis of cognitive functions in humans. The institute had been set up in the middle of the 1990's by a substantial grant from a tremendously rich medical research charity. This had allowed for the construction of a purpose-built house in the middle of one of the best research environments in Britain which was the workplace for an international, mainly European group of scientists from several disciplines (neurology, psychiatry, psychology, physics, mathematics, biology and others).

Although the institute was formally part of a university structure, being funded by the research charity, informally known as *The Trust,* gave the department some advantages not usually found in publicly funded research institutes in Britain. It had for some years been the official policy of the Trust to pay its researchers at a higher level than in comparable state funded research positions, and the department had relatively more money for running costs. Although the financial details, as so often in anthropological work, were difficult to oversee, it appeared that there were sufficient resources to do, what needed be done: The physical space was well-designed and -equipped, and computer networks and brain scanners were continuously updated and of a very high standard. During the last decade, the government policy in Britain as in most of Europe has been to actively promote co-operation between industry and universities thereby encouraging the formation of a government-industry-university triple helix (Etzkowitz & Leydesdorff 1997). Such formation was, however, not encouraged by the Trust. The agreement was, rather, that the Trust sponsored the institute through a five year renewable contract. During that period they were to be the main provider of resources to the institute which should, then, pay back in excellent research.

But how does one measure success? Judging by the reports of the institute and by the things valorised in the daily interactions, one standard appeared to be the most important: peer-recognition in its various forms. The most important aspect of this was the track record of published papers, but there were also other symbolic markers of recognition such as invitations to give special, honourable lectures, prizes from various committees and invitations to membership of various scientific organisations. These measurement of productivity and success were very visible in the day-to-day interactions of the place. Each time a paper was submitted for publication, the abstract was circulated on the internal e-mail, people receiving prices were publicly mentioned and lauded, and when one of the senior researchers became a member of the highly prestigious Royal Society, a small reception was held for all members of staff. On the third floor, where the principal investigators resided, emblems of success were furthermore made publicly visible in the form of framed pre-prints of papers published in *Science* and *Nature,* two journals widely accepted as the most important journals for any scientist to publish in. Other walls would spot posters announcing important scientific talks given by researchers from the institute.

By most conceivable standards the centre was doing very well. According to the Science Citation Index, the senior scientists had thousands of citations of their work, and rumours had it that at least two of them were in the British top 10 of most quoted scientists. Sceptics both within and outside the department claimed, however, that these impressing results not only expressed the long-term scientific importance of the work done. According to their analysis of the Science Citation Index numbers, which ran along the same lines as the one conducted by the Martian anthropologists, the impressive amount of citations was partly due to the enormous hype (McCrone 1999) that surrounded the field of brain-imaging in the 1990's. A hype which almost automatically secured a higher rate of citation than in any other related field, and this gave the leading scientists in the field many opportunities to collectively boost citation ranks by quoting among themselves.

The workings of the Science Citation Index system as outlined by the Martians appeared, in other words, to be a generally known and accepted fact. This definitely put some pressure on the younger researchers about to establish themselves. It was seen as important to think out experiments in such a way that they could be published in 'good' journals. Before conducting an experiment, one would discuss which journals could potentially publish the expected results. In this process Impact Factor tables, that analyse the average amount of citations generated by publications in various journals, were an important tool. In my interviews, many young scientists would express a feeling of tension between these 'mundane' aspects of gaining credentials in a scientific career on one hand, and the larger project of adding novel, important knowledge to Science on the other. The latter project was a norm that many would explicitly and without questioning claim to identify with.

As everywhere else, norms were rarely discussed publicly. The four norms of the scientific ethos outlined by Merton (1942) *Universalism, Communism, Disinterestedness, Organised Scepticism* did, however, appear as useful notions around which important aspects of the internal logic of the place could be condensed. One of the important outputs of the institute is a software package that has become one of the most used frameworks for the manipulation and interpretation of the highly complex brain-imaging data. It elegantly integrates a couple of well-known statistical methods and it allows for the generation of results whose statistical significance is supported by a long tradition of scientific knowledge and practice. Over the last 10 years the software package, along with the methodologies and conceptualisations inscribed in its use, has become central in setting the standard for how findings worthy of publication can and should be extracted from the massive arrays of raw data generated by brain scanners. The software is therefore a practical implementation of the *Universalist* idea »that truth-claims, whatever their source, are to be subjected to *preestablished impersonal criteria*« (op. cit. p. 270). Although the development and maintenance of the package requires a substantial amount of man-power, it is made publicly available via the World Wide Web, and free support is offered through the e-mail based help-line frequented by the community of researchers using the software. This way of sharing and distributing results of scientific work is, in other words, fully in line with Merton's norm of *Communism* which implies that »the substantive findings of science are product of a social collaboration and are assigned to the community. They constitute a common heritage in which the equity of the individual producer is severely limited« (op. cit. p 273).

Due to the explicit policy of the Trust, a level of institutional *Disinterestedness* was inherent in the very funding structure of the department and contrary to many other current brain-research centres, there were hardly any signs of a close co-operation between science and industry that could pollute the 'pure science' aspect. On the contrary, this institutional attitude appeared to enjoin disinterested activity to the extent that »it is to the interest of scientists to conform on pain of sanctions and, insofar as the norm has been internalized, on pain of psychological conflict« (p. 274). As described by Merton, this norm of disinterestedness was not blindly internalised, it was rather an ongoing focal point for the interpretation and evaluation of actions conducted by people both within and outside the department. Finally, the *Organised scepticism* was not only a concurrent tone of the informal interactions, it was also the theme underlying most of the recurring ritualised meetings attended by most of the staff. In the weekly *Journal Club,* for instance, a recent, allegedly important scientific paper would usually be discussed and torn apart by the community of researchers. Similarly, in the weekly *Project Presentations* proposed experiments were made subject to tough discussions and scrutiny that would often change the design and outlay of the experimental paradigm.

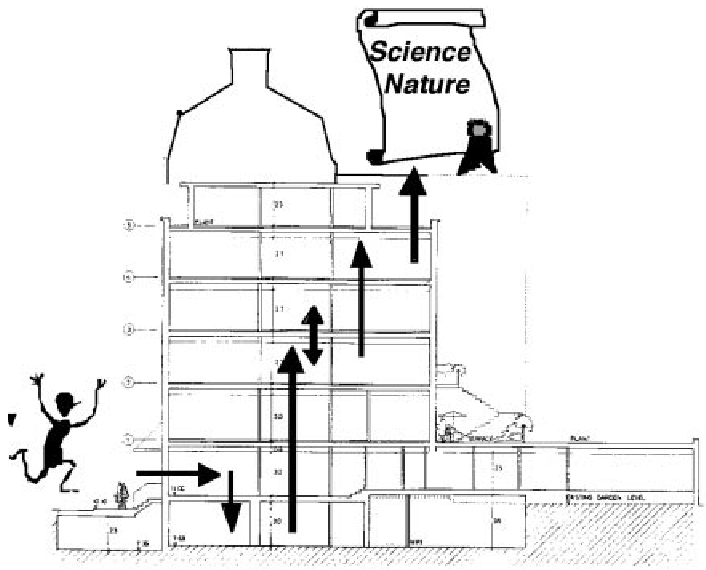
The Martian outsider observed something similar to the Mertonian norms organising and attracting the behaviour of scientists. They did, however, diabolically suggest that if one shifted the frame of reference from the community of scientists to the society in which this community was embedded, the four norms could be given the radically different headings of *Cultural Imperialism, Mafiosism, Opportunism,* and *Collective Irresponsibility* respectively. They interpreted this finding as a normative underdetermination of science since the same set of norms can be given alternatively valued spin according to the context in which they are regarded (op. cit. 62-67). We shall return to the Martian interpretation later, but for now it suffices to say that at a descriptive, as opposed to an evaluative, level there appeared to be a strong correlation between the findings of the Martians and my field-work experiences.

The importance of the Science Citation Index and the rediscovery of the Mertonian norms were, however, not the only findings that our Martian anthropologists appeared to get right. They claimed in their analysis that Science, as an institution, be best analysed as a special instance of the general religious structure. Institutions are, however, not only abstract entities, they are also situated in concrete locations, workplaces (Hacking 1988) where the abstract logic of the tradition is being turned into practice. That implies that if the Martian analogy is correct, then an analysis of the actual site of science should identify structures, functions and meanings comparable to a workplace of Religion such as a temple or a monastery.

An analysis of the symbolic meaning of the physical lay-out of the institute appeared, indeed, to confirm the finding of the Martian anthropologists. Although the building was purpose-built in the beginning of the 1990's it had inherited the name, St. John's House, and the physical face from the previous building at the site which since the beginning of the 20'th Century had housed a convent. Standing high over the entrance door, a statue of St. John still symbolically baptised everybody walking into the building. In its contemporary function, the institute had a strictly hierarchical organisation of physical space. Individual volunteers, called *subjects* in the vernacular of the place, would walk under St. John, into the house and down to the scanners in the basement where they underwent a number of examinations. As an outcome of this, the subjects, or more precisely representations of activity in their brains, were transformed into data in the form of mathematically described objects. In the ideal description these data would flow up through the house to the second floor where young scientists, the fellows, would subject them to the first analysis. The transformed data would then be discussed with the senior scientists *the principals* residing on the third floor. Finally they would be presented in the seminar room on the top floor before they could leave into the global circulation of scientific knowledge presented in conferences and solidified and inscribed in leading journals (Roepstorff forthcoming).

The house is therefore in cosmological terms a site of transformation: A black box (Latour 1987) where subjects entering under the baptising hand of St. John in a vertical flow through the house are being turned into objectivity ready to be inscribed in the immortal annals of *Science* and *Nature* (Figure 1). As the old convent, the modern laboratory thereby connects a profane world of day-to-day interactions to a sacred transcendental world of meaning above time and space.

Figure 1: The laboratory as a black-box transforming subjects into objectivity in a vertical flow through the house.



Apparently we see in the structure and function of my site of research an archetypal embodiment of the mainstream scientific project as outlined by the Martian Anthropologists. On an institutional level, Science apparently shares with Religion a cosmological role in establishing what is true and eternal as opposed to just arbitrary, profane and politically motivated. On a functional level, science appears, however, to be somewhat more mundane: It is about publishing, disseminating, and writing; about being cited and getting recognised.

It may appear that I have been overly emphasising elements that would fit into this picture. Obviously it is difficult for me to prove that I have not been misled by my own analysis, but even on close reflection I don't think that I have systematically committed that error. Rather, the remarkable coherence between the structure of the building, the day-to-day activities and the archetypal Mertonian norms of science should properly be understood in relation to the on-going reconstruction of the scientific field as described by the Martians. The change in research-funding during the 1990's in Britain meant that, judged by Mertonian standards, much of public, state-finance research funding have become polluted by the need to stress commercial application and align the research with short-term policy plans. The Trust appears in the economical-ideological landscape of science policy as a non nation-state source of substantial economic and symbolic capital, explicitly holding a particular set of traditional (Mertonian) norms in high esteem. It thereby supports and reinforces an orthodox interpretation and representation of the scientific norms and values that explicitly and demonstratively works against much of the current public research policy which, relative to the Mertonian standards, favour a heterodox alliance with commercial interests and short-term political priorities. As always when one identifies a shift from doxic unquestioned values, such as the Mertonian norms in their first formulation, to an explicit presentation of orthodox and heterodox interpretations, this suggest that a reconfiguration of the surrounding political and social field is ongoing (Bourdieu 1977; Strathern 1995). That reconfiguration is very likely due the decoupling of the nation-states and Science foreseen by the Martians. A decoupling that takes apart institutions which appears to have been connected at least since the modern nation-states arose sometime in the middle of the 19. Century. In this ongoing process of change the Trust appears to emphasise the idealised norms and values of the scientific identity.

Whereas the Martians interpreted this decoupling as the coming of a second Enlightenment (Fuller, 1997, p. 60-62), the results from my fieldwork could, however, just as well point to a different scenario: A situation similar to the pre-nation-state medieval Europe where rich monastic orders were working alongside state institutions of the pre-nation political landscape, sometimes co-operating, sometimes struggling against them. Perhaps the Trust and other similar entities will in the future be seen as acting in a similar way in the post-national political landscape. They set up symbolic monasteries that not only act as a framework for the practical doings of scientists, but also as cathedrals acting as sources of identification and sites of maintenance for transcendental values of a Mertonian Science. Rather than a 'new' enlightenment modernity, we therefore appear to see an amodern scenario. Amodern because two of the most forceful elements of modernity, a conceptual nature-culture dichotomy (Latour 1993) and a political nation-state unity (Greenfeld 1992) both appear to be undergoing major reconfigurations.

With this analysis at hand it suddenly makes sense why my initial chain of associations to *Science under Pressure* led me from the mutilation of a beautiful piece of human construction to a Russian biologist. Most of the former Soviet Union have, namely, in the most brutal way experienced a decoupling between the (nation)state and the financing of science. In that part of the world it appears, however, to be absolutely uncertain which instances will fill out this gap and how the coming reconfiguration will affect not only the practice of science, but also the norms and values it will follow and/or identify with.

Fourth encirclement: the Outsider and the Insider perspectives revisited

On a descriptive level, the Martian analysis appears to resonate well with my results. There is, however, another more evaluative level, where the Martians, or perhaps Fuller's reading of them, in my opinion misconstrue central aspects of science. Fuller claims, namely, that the Martians in paralleling Science and Religion make a serious debunking of the whole scientific project. This is, however, only possible because the Martians, properly unknowingly, reproduce epistemological problems concerning knowledge generated by Outsiders and Insiders that have for some time been a major concern to earthling anthropologists studying knowledge in general and scientific knowledge in particular.

The Martian paper represents, according to Fuller, the view of the ultimate Outsider designed for Outsider consumption and this perspective is in Fuller's representation ideal for secularising and de-mystifying the notion of Science that to ordinary Earthlings are surrounded by an air of the sacred. Properly unknowingly the Martians thereby reproduce the strategy commonly applied by Earthling critical intellectuals (no mentioning of Fuller here) who see it as a noble pursuit to demonstrate that behind any apparently stable and solid representation lies nothing but fetishism and false consciousness (Latour 1999: 276-280). The difference between the knowledge claims of Insiders and Outsiders was, as so much else of relevance for the contemporary discussion of science, discussed by Robert Merton, this time in one of his later articles on the sociology of knowledge »The Perspective of Insiders and Outsiders«

(Merton 1972). The paper is fuelled by a concern about the rise of the so-called *Insider Doctrine* of knowledge which in its strong form claims »the epistemological principle that particular groups in each moment of history have *monopolistic* access to certain kinds of knowledge« (p. 102).

Tracing the doctrine in the recent history of ideas from Marx to the German nazis, Merton discusses at length the version of it propagated by his contemporary black intellectuals who claim that »as a matter of social epistemology, *only* black historians can truly understand black history, *only* black ethnologists can understand black culture, only black sociologists can understand the social life of the black and so on« (p. 103). This analysis demonstrates that the epistemological and ontological claim of the Insider Doctrine develops in social groups 'on the way up' fuelled by an impetus to gain control over their social and political environment (p. 110).

In Merton's material, the Insider doctrine does not apply to the natural sciences since it«does not argue for a Black Physics, Black Chemistry, Black Biology or Black Technology for the new will to control their fate deals with the social environment, not the environment of nature....[and there is moreover] nothing in the life experiences of Negroes that is said to sensitize them to the subject matters and problematics of the physical and life sciences« (op. cit.). In the current discussion on the natural sciences, we see, however, a clash between Insider and Outsider perspectives. The Outsiders, often arguing along the lines of Fuller's Martians, claim to debunk the whole notion of science and they are being confronted by the so-called Science Warriors that claim only Insiders, that is scientists and a few respectable philosophers of science (Weinberg 2000), should speak on behalf of Science. The argument of the scientific insiders run very much along the lines of Merton's analysis. As opposed to the Black intellectuals in Merton's case, scientists do have something in their personal and professional training and life experience that could be said to sensitise them to the physical and life sciences. They are therefore in a position where they can claim a particular Insider-knowledge of the sciences. In contrast to the Black intellectuals discussed by Merton, this application of the Insider doctrine does, however, appear to be a response to a feeling of pressure (Sokal 1996)[4], rather than as a tool employed by people on the way up the social ladder.

4 The most famous battle in the 'Science War' between what one could call Radical Realists and Universal Constructionists was the well-known Sokal affair, a battle beautifully won by the science warriors. It might, however, be a Pyrrhus victory that did not at all confront the real problems underlying the current pressure on science and scientists (for a discussion see Kjærgaard 1997; Roepstorff 1999).

In ethnography and anthropology, the methodological debate has continuously been nourished, sometimes in an almost navel-contemplating way (Clifford & Marcus 1986), by an examination of the possibilities and problems of the insider-outsider distinction. This has indeed been the case ever since field-work-based research became *the* methodology in ethnography in the beginning of the 20th Century with the explicit aim of grasping, 'the native's point of view, his relation to life, to realise *his* vision of *his* world« (Malinowski 1922: 25). The traditional field-work method relied on (a myth of) the possibility to reconstruct from the outside the native's point of view, so that this Insider perspective and all its limitations could be dissected and communicated. The epistemological and phenomenological movement underlying this strategy has, however, been increasingly difficult to uphold once anthropologists turned the attention to their own societies. This radically problematised the very existence of an 'outsider' platform from where the Insider perspective could be constructed.

This problem is arguably nowhere more potent than in the emerging field of 'anthropology of science' (Latour 1990). It can be argued that none of the emerging bulk of 'outsider' studies of science represents a truly outside perspective. As the anthropologist and his or her subjects are immersed in and have emerged from rather similar life-worlds, it is, rather, studies conducted among like-minded people with whom one shares professional, educational and personal experiences. This means that it is almost impossible in a consistent way to treat the more or less obscure abstract concepts, that one encounters, as free-floating arbitrary notions. This has otherwise been *the* treatment of notions from magic to *kula,* that in the hands of the anthropologist can be made to point to a real reality invisible to the poor insiders that are trapped in their own notions. A classical strategy applied when studying knowledge of 'the natives', applied by Lévy-Bruhl (1966), Lévi-Strauss (1962), Malinowski (1954) and Atran (1998) alike for varying purposes, has been to judge the knowledge of the natives by the (supposedly) universal standards of scientific knowledge. This conceptualisation only made sense because these anthropologists did not feel a need for explaining science, which was simply taken for granted as a universal measurement. This observation was already made by the Polish microbiologist and epistemologist Ludwik Fleck in 1935 (1979) but nobody paid any attention to him in his lifetime. It was only when anthropologists began studying the actual work of scientists, which proved no more straight forward than the doings of the natives, that it became obvious that there no longer was a stable, outside ground that one could compare to. (Latour 1990).

The Martians did, as many modern so-called critical social scientists (Latour 1999: 276-280), try to ground the epistemology of their natives, the scientists, in their societal structure. That is, however, not a feasible method either, since the very content of the knowledge generated within a particular tradition is not a simple derivative of the social. On the contrary, knowledge is an integral part of the social since, to paraphrase Bruno Latour, »a society that collides particles inside gigantic accelerators, [or a society that study the nature of the mind in brain scanners, AR] is not the same as one that does not« (Latour 1990). This implies that it is not, as the Martians attempted, possible to use 'the social' as a neutral ground onto which one can analytically project science. The interesting question is, rather, how nature, society and 'cosmology' is made in one simultaneously movement.

This is well exemplified by the institute that I visited. The Mertonian *idealistic* interpretation would be that they were simply following the silent norms of proper science. A Martian anthropologist would against this propose a *critical* interpretation, namely that they are indeed just a religious institution in disguise, hiding earthly human ambition under the veil of the magic of science. The *realist* earthling anthropologist would be able to recognise both perspectives, but he would not focus on the evaluation of the norms as such. He would rather be stricken by the fact that in the institution, knowledge and cosmology appear to go hand in hand. This is not because the two reflect each other, for there is no direct connection between the norms, structures and social organisation of the laboratory on one hand (Roepstorff forthcoming), and the actual content of the knowledge being produced on the other. There is, however, apparently no problem about being at the same time a producer of knowledge at the very highest level and being an almost explicit temple to *Science* and *Nature* understood both as abstract entities and as concrete sites of publication. This shifts the analytical focus away from an normative evaluation of the norms to an analysis of how they intermingle with the social organisation. The Insider interpretation, that the Outsider anthropologist would not directly disagree with, is that it is precisely because the institute adheres to Mertonian norms and because one takes on the responsibility and the possibilities that goes with them, that one may create that very special environment which is needed for knowledge to be made creatively and scrutinised properly.

Conclusion

Once one has relegated from the artificial outsider-insider dichotomy, the Martian/Fuller question of whether the belief in science is superstitious and whether science is nothing but a religion-substitute becomes relatively ridiculous. The anthropologist would claim, although he may on this point part with his informants, that obviously science is not just a passive reflection of a given reality. It is an active process of making knowledge (Fleck 1979). Through this activity other aspects are generated as well: a social order that stages how, why and by whom knowledge should be made, truths established and controversies settled. In that perspective it is important to know what are the explicit and implicit rules of the game: Are we playing by and judging each other by Mertonian norms, or do we follow other rules? Or, to paraphrase a recent case from Denmark who should we trust when we decide whether chewing gum is good for our health? What is at stake is, in other words, how science is to be placed in the social order (Merton 1938), and what types of institutions are set up to decide what is right and wrong (Vestergaard 2000).

In the Science-Pressure metaphorical landscape, that I outlined in the introduction, this question resides somewhere near the attractor-site where the beautiful sculpture-being-crushed resides. The two cases of the Russian biologist and the European brain mappers apparently reside near the other attractor where science is a practical activity undertaken by concrete people. There is, however, a common link between these two cases that sends an extension right across the science-pressure landscape to the other attractor of the beautiful human construction threatened by brutal forces. The Russian biologist, that asks to be embedded in new networks, and the Trust, that in an orthodox movement supports and imposes the importance of old norms, are both active responses to common global changes in the current role of science, both in terms of the funding structure and in terms of the place of science in the social order and in the public image. These changes appear to imply that a special role for science in the social order can no longer be taken granted, and that in the future the part played by science may be very different from the present one (Vestergaard, op. cit.). As a response, both parties appear to be consciously striving to connect the day-to-day activities of making science with an abstract idea of science as a beautiful and privileged sphere of human activity. This is part of an active process of making cosmology (Barth 1987) and social order and of establishing criteria for who and what should be trusted in settling what is right and wrong.

In this process, there are neither specially privileged outsiders nor insiders. On the contrary, the issue is much too important to end up as a clash between self-proclaimed Insiders insisting on the special role of scientists to talk for science confronting rhetorically constructed Outsiders setting out on the don quixoteian mission of de-mystifying and de-bunking.

The take-home message from this science study lies, therefore, somewhere else: There is no inherent contradiction between science, culture and cosmology. On the contrary, when making science people do, as when they do all sorts of other things, also make culture, cosmology and social order. The challenge lies, therefore, in transcending the banal dichotomy between the Insider and the Outsider perspectives where the Insider claims »pure science is not to be touched by outsiders« while the Outsider claims » what you call science is nothing but religion which is nothing but the social expressing itself«. As the *bon mot* for this diplomatic mission it is worth recapitulating how Merton ended his little treatise on Insiders and Outsiders:

»Insiders and Outsiders unite. You have nothing to loose but your claims. You have a world of understanding to win (Merton 1972).«

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Trends, Perspectives and Problems in the Physical Sciences

Helge Kragh*

Steve Fuller's view of modern science, as he discusses it in his recent book [1], includes several interesting key notions that invite further discussion and, as I see it, opposition. Among his leading themes is the claim that post-World War II science is radically different from earlier science. The difference, Fuller argues, is particularly reflected in, and exemplified by, the big-science phenomenon. I do not share Fuller's view and shall briefly state my reasons. In the latter part of this paper I offer my general view on the nature and prospects of science at the turn of the century, including some comments on what is known as the crisis of science.

Fuller speaks of science in its very broad meaning, in the sense of *Wissenschaft* or knowledge production in general. Contrary to this notion, which includes practically all the academic disciplines, I have in mind mostly the natural sciences. My comments are in particular related to those sciences that I happen to know best, namely, basic sciences such as physics, chemistry, and astronomy. These are what are often called the physical sciences. My discussion is undoubtedly coloured by a book I wrote last year, entitled *Quantum Generations,* in which I surveyed the entire development of physics during the previous century and in which I formed various general conclusions and suggestions as to the development of this field of science [2]. I shall start with a few general observations with regard to some of the conspicuous trends that can be identified in the development of the physical sciences during the twentieth century. I am well aware that developments within the geo- and biosciences do not mirror those of the physical sciences. Nonetheless, I believe that my discussion is more or less valid also for these branches of science.

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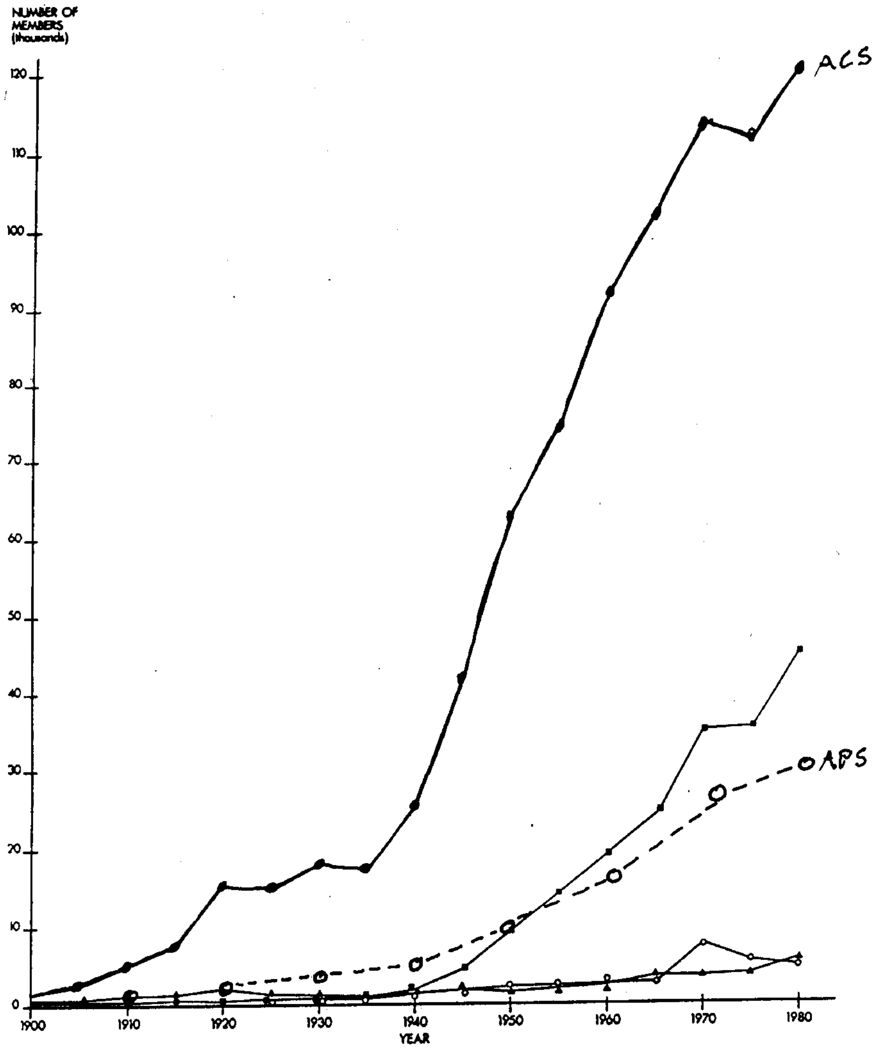
The trends in the modern development of the physical sciences that I find particularly interesting are the following:

1. Growth
2. Industrialisation
3. Big Science
4. Americanisation
5. Collectivisation
6. Internationalisation
7. Specialisation and fragmentation
8. Inter- and trans-disciplinarity

Space does not allow me to comment on all of these trends, but a couple of remarks concerning a few of them will be sufficient for the present purpose.

First and foremost, the *growth* of the sciences during the twentieth century has been truly remarkable (figure 1). To take physics as an example (if not necessarily a representative one), in round numbers this science is today about 100 times as large as it was around 1900. For example, whereas the number of physicists anno 1900 amounted to at most 1500, the number by the year 2000 was close to 15,000. Likewise, the number of physics publications per year has increased from about 2,200 to 240,000. A multiplication factor of 100 is amazing by all standards - recall that in the same period the world's population has increased by a factor of 4, from about 1.5 billion to 6 billion. Between 1920 and 1980 the growth was exponential, by and large, but of course this cannot go on forever. Expectedly, during the last couples of decades, the growth has levelled off and seems now to approach a zero growth rate, on a longer time-scale approximating a logistic growth (figure 2). Although the growth in manpower and publications has been remarkable, the growth in economic support has been even more drastic. Reliable historico-economic figures for world-wide science support do not exist, but it is certain that today the sciences receive considerably more than 100 times as much money as they did a century ago.

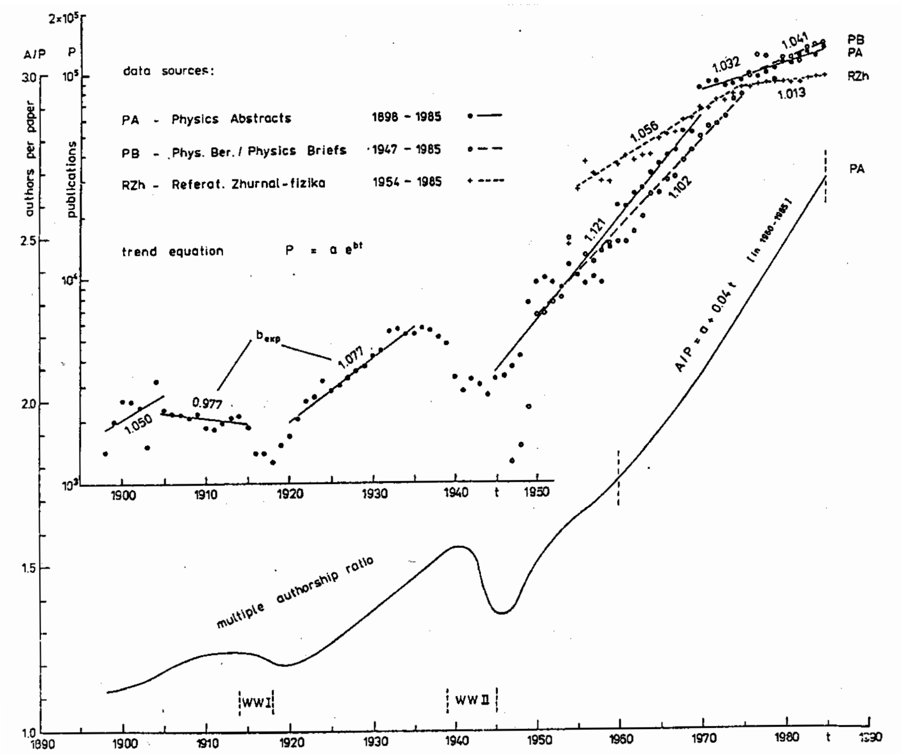
Figure 1.
The growth of science, here exemplified by the rise in membership of American Chemical Society (ACS) and American Physical Society (APS). The other curves refer to membership of other American chemical societies. Adapted from Arnold Thackray et al., *Chemistry in America* 1876-1976. Dordrecht: Reidel, 1985.



The most important change in science policy and science funding has undoubtedly been the great shift that occurred in the decade after World War II, first in the United States. As is well known, the change was, more or less, a reflection of the big war-oriented research projects such as radar, penicillin, the atomic bomb, etc. Under the impact of the military success of science, Vannevar Bush and others formulated a new philosophy of science policy which to a large extent is still the blueprint on which modern science policy is founded. A large number of new institutions were established, and science now became publicly funded by the taxpayers to an extent that was unheard of before 1940. In many countries, before World War II scientific research was often considered not to be a responsibility of the state but was instead funded, if funded at all, by private foundations and wealthy individuals. The massive intervention of governments into research is essentially a phenomenon characteristic of the post-1945 period.

Today, the science sector is of course very expensive, but it should always be kept in mind that costs are relative and open to discussion. There is no such thing as »too expensive« in any absolute sense. Even in the heydays of high-energy physics - the archetypical big and expensive science - the funding for this branch of physics »only« amounted to about 0.025 per cent of the Gross National Product (figure 2). Furthermore, many gross statistical data refer to the money allocated to R&D (Research and Development), which is not a very good measure when it comes to the basic sciences. These sciences, whether organised large-scale or small-scale, absorb only a very small part of the R&D budgets. On the top of that, with what should science expenditures be compared? How outrageous is a $10-billion physics project in comparison with the costs of developing a new nuclear submarine? How is it that expenses for advertising and marketing commercial products greatly exceed those devoted to basic science? And why do we quarrel over science funding, when much larger donations and taxes to religious causes are happily accepted?

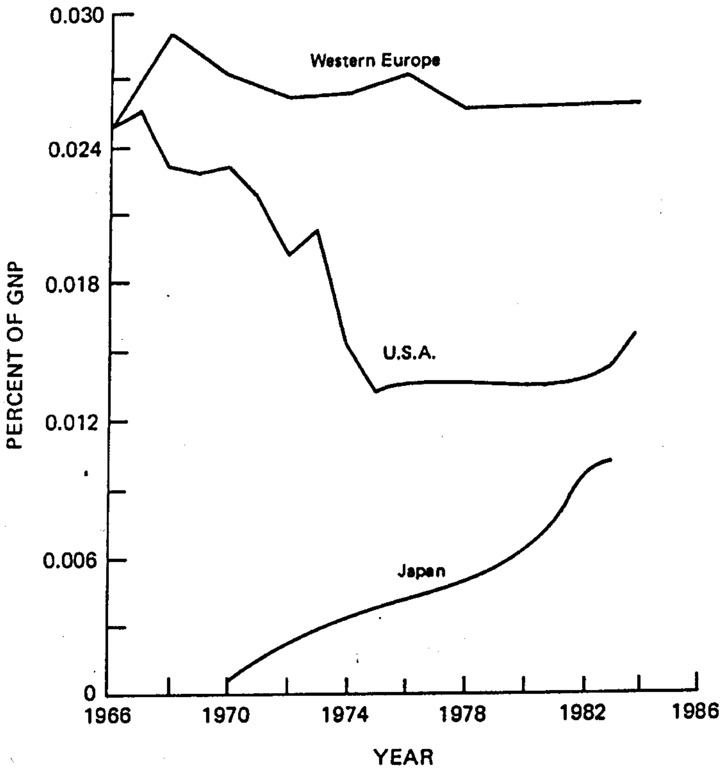
Figure 2.
Funding for high-energy physics as a percentage of the GNP. Source: William F. Brinkman et al., eds., *Physics Through the 1990s: An Overview.* Washington, D.C.: Washington Academy Press, 1986.



Another, no less interesting trend during the last fifty years can be summarised in the term *collectivisation.* There has been a clear development towards teamwork where many researchers work together in groups, often organised in a complex hierarchy. Naturally, the trend has been much more marked in the apparatus-intensive experimental sciences than in the theoretical sciences, and it is also a trend that is particular to the natural (and especially the physical) sciences. Among researchers in the humanities, teamwork is practically unknown. Collectivisation is in part related to the increasing dependence on large-scale apparatus, i.e., the *industrialisation* of science as it occurs most markedly in big science. Among the many relevant aspects of this area of problems, I shall only mention that, traditionally, scientific research has been associated with individuals who engage in investigating nature and, through this work, gains merit that he or she can use for career purposes. In some areas of science, this tradition seems to be seriously jeopardised.

Thus, while in the good old days scientific papers were written by a single author, nowadays many papers are multi-authored. The trend towards multi-authorship has been steady, if not particularly dramatic (figure 3). Again, this is clearly related to the experimental sciences, and especially to very expensive or »big« science. Multi-authorship is very rare in the humanities, and even in theoretical physics and astronomy the average number of authors per paper is close to one (for astronomy, see [3]). In general this is not a great problem, but in extreme cases it may be so. How much satisfaction is there being author number thirty-seven in a scientific paper? Who is responsible for a scientific project in which more than one hundred researchers have participated? As indicated by figure 4, the front page of a 1983 physics communication which the following year resulted in a Nobel prize, the scale of science collaborations may be very large indeed.

Figure 3.
Trends in world publication output (physics) and collaborativeness. The upper curve shows in logarithmic scale the rise in physics publications, whereas the lower one gives the average number of authors per paper. S ource: Jan Vlachy, »Scientometric analyses in physics - where we stand,« Czechoslovakian. *Journal of Physics* B 36 (1986), pp. 1-13.



Collectivisation is typically associated with big-science projects and hence with big, expensive apparatus. Here the danger is that the team, or the apparatus, takes over priority (socially as well as epistemically) from the individual scientist. The machine may become a kind of Frankenstein monster who controls the scientist, rather than the other way around. The potential dangers of big science are encapsulated in a memorandum of 1956, in which the director of an American accelerator laboratory explained to the physicists that »In this new type of work experimental skill must be supplemented by personality traits which enhance and encourage the much needed cooperative loyalty.« Moreover, »Since it is a great privilege to work with the cosmotron [an early particle accelerator] I feel that we now must deny its use to anyone whose emotional build-up might be detrimental to the cooperative spirit, no matter how good a physicist he is. ... I shall reserve the right to refuse experimental work in high energy to any member of my staff whom I deem unfit for group collaboration. I must remind you that it is, after all, not you but the machine that creates the particles and events which you are investigating with such great zeal« [2, p. 308]. Comments are unnecessary.

As to *big science,* this is mostly a post-1945 phenomenon. Although there are several examples before World War II, especially in astronomy, it is only during the latter half of the twentieth century that science has become really big and really expensive. Standard examples from the physical sciences are the highly successful Hubble Space Telescope (an investment of nearly $3 billion) and the Large Hadron Collider, a $6-billion European accelerator to be completed in 2005. The failed American $10-billion project of a superconducting supercollider (SSC) has been much discussed and enters prominently in Fullers's book. It is surely a case worth looking at, but it should not be forgotten that the SSC case is an anomaly, not the rule in science.

Fuller, like many other analysts of science, does not like big science. But what is so bad about being big? Small may be beautiful - as famously claimed by the British economist E. Schumacher [4] - but what is beautiful is not necessarily very effective from a scientific point of view. There are areas of science that simply cannot be done small-scale, such as high-energy physics and parts of astrophysics and observational cosmology. The same is the case with the human genome project which, although not relying on big and expensive apparatus, must be classified as a big-science project as well. We - the scientists, the public, or the politicians - can of course decide that we will have no big-science programmes, but then we also have to recognise that there are parts of nature that we do not want to know about. And these parts are typically those that most people, including non-scientists, will find very attractive and intellectually exciting.

It seems to me that some of the opposition to big science rests on a somewhat romantic and ill-founded notion that true and deep knowledge can only be gained individually or in a small-scale way, in the scientist's own quest for knowledge. Big science, it is often implied, only leads to instrumentalist or irrelevant knowledge, if to proper knowledge at all. According to Fuller, big science may well be a contradiction in terms. He asks, rhetorically, »has the nature of the knowledge enterprise fundamentally changed as a result of its dimensions having exceeded certain limits?« [1, p. 38]. I consider this view to be false, to be a myth. As I see it, there is no contradiction at all between true, fundamental knowledge and big science. On the contrary, much of the progress that has occurred during the last decades within the fundamental sciences has only been possible because of technology-driven big science. Recent progress in observational cosmology is a beautiful example. Again, we may decide politically that knowledge of the early universe or the unification of natural forces is irrelevant, but it cannot be denied that if we want such knowledge we need to go the big science way. Computer simulation is a wonderful thing and used very extensively, but it is a supplement to observations and cannot be used as the only means to get information about nature.

Specialisation and *fragmentation* are other keywords that often appear in discussions of modern science. And, yes, there has undoubtedly been a trend towards numerous special sub-disciplines, not to mention sub-sub-disciplines. Many of these small and highly specialised fields include scientists who only truly understand their own narrow speciality, although of course they apply methods and techniques based on other disciplines. Modern scientists are very different from the versatile »natural philosophers« of the eighteenth century, but then the amount of present scientific knowledge is also very much larger than it was two hundred years ago. It is a long time ago that a scientist could claim to know his entire discipline, let alone all of science. We may regret the specialisation, but we should also realise that it has been a necessary and very powerful element in the progress of science.

Fortunately, specialisation stands not alone and is not identical with narrow-mindedness. The reverse trend is no less marked. Not only have there emerged a large number of new interdisciplinary fields of the traditional type exemplified by, for example, astrophysics, biochemistry and chemical physics. Also, and more interestingly, trans-disciplinary fields that are more than combinations of existing knowledge areas have been formed; these are typically formed naturally, as a way of understanding very complex phenomena and in connection with such studies. Although they are typically project- or purpose-oriented, nonetheless they are basic sciences. Examples may be the neurosciences, climatology, planetary science, chaos theory, and materials science. It is not least within these trans-disciplinary sciences that spectacular progress has occurred during the last decades.

I would like briefly to comment on one of the more radical proposals made by Steve Fuller as part of his wish to democratise science; namely, whereas it should be left to the state to distribute scientific knowledge, it should not produce it. According to Fuller, knowledge production should be left to corporations, charities and unions, and »the government should only fund research where it is required to address a problem in public policy« [1, p. 105]. Now, this seems to me not only a very drastic proposal, but also an unrealistic one that tends to disregard the relationships between science and technology. Incidentally, it is not a particularly novel proposal. Like much else in Fuller's work, it has strong similarities to the science-political ideas of the late Paul Feyerabend [5].

Consider science in the real world. If there is a social problem, say, the greenhouse effect as partially caused by pollution, how do we know what kind of research is needed? Surely, we can not initiate from scratch research programmes and hope that these will solve a major social problem if there is not already a solid basis in non-oriented scientific knowledge that can be pooled together and used for this purpose. If the state shall enter the production of scientific knowledge only after a problem in public policy has been identified, it will in most cases be too late. (Consider the pollution of rivers. How could this problem have been solved, or greatly diminished, if there did not already exist a highly developed chemical and bacteriological science by means of which the pollutants could be identified and measured?).

The question of expert knowledge versus so-called democratic knowledge is closely connected with the specialisation issue, and it is a question that looms high in Fuller's book. I would like to say a few words about it, if for no other reason to make clear that I disagree with him. Fuller warns against the cult of expertise, to leave science to the specialist practitioners of science, to identify scientific knowledge with expert knowledge. He argues for a more democratic kind of science, a »science for the people« or what he calls »citizen science.« This is far from a new vision, of course. I hardly need recall the popularity of the notion in the 1970s [6]. Fuller's version seems to be as little concrete as most earlier versions of the democratic science vision. Frankly, I do not quite understand what citizen science is, more precisely. Are there areas of modern science that already qualify as citizen science, whereas other areas do not? I assume that big science counts as the very opposite of citizen science, but it would have been nice to have just one example of citizen science as Fuller envisages the concept. At any rate, the historical record of the many attempts to formulate an alternative science for the people is not encouraging, whether in its neo-romanticist sense or in its political left-wing sense. Even less encouraging are the few cases where attempts have been made to transform the idea into practice, including such monstrosities as Aryan science, Lysenko's pseudo-biology, and proletarian science in China during the Cultural Revolution.

Scientific knowledge may be said to be democratic in principle - because it is objective and public - but in practise science is certainly not democratic in the ordinary sense of the word. And, I would add, nor should it be. In fact, an important reason for the success of science is that it is cultivated by experts and judged by communities of experts. Contrary to a popular myth, these expert communities are relatively open, not priesthoods or secret fraternities. They are not, as Fuller claims, exponents of a »mafia mentality« [1, p. 21]. Like Feyerabend before him, Fuller wants to free scientific inquiry from devolving into a form of expertise. His citizen science aims »to render a discovery compatible with as many different background assumptions as possible, so as to empower as many different sorts of people« [1, p. 111]. To my mind, this kind of populist vision will not lead to any new science. It will be the death of science. Fuller asks ironically, and rhetorically, if one has to be a card-carrying Darwinian in order to have anything credible to say about biology. According to my view, the answer should in principle be affirmative; by the same token, I maintain that one should in principle be a card-carrying Einsteinian in order to have anything credible to say about physics. After all, Darwinian (or neo-Darwinian) evolution theory and Einsteinian relativity are so well tested that rejection of these theories almost amounts to denying facts.

It is my general view that modern science, in spite of the very important changes that have occurred, is in many ways fundamentally the same as science in late nineteenth century or even earlier [2, pp. 440-51. See also 7, pp. 126-30]. Thus, the basic rules of the game - the methodology of research - have not really changed. With the exception of computer experiments, which now are very important in many sciences, modern scientists rely on basically the same methods that were used a century ago. How to evaluate a knowledge claim, what counts as a good experiment, testing procedures, aesthetic arguments, the use of thought experiments - these and other methodological topics have largely remained the same, both in little science and big science. Big science is bigger than traditional science, but it is no less scientific; nor, for that matter, is it more scientific. Fuller seems to suggest that the very nature of the knowledge enterprise has fundamentally changed as a result of its big dimensions [1, p. 38]. I do not think that this is the case. At any rate, I think that the claim of such a change still needs to be documented or convincingly argued.

This is not to say that there have not been great changes, only that these changes have typically built on existing knowledge and a healthy respect for traditions. It may seem strange that respect for traditions can produce revolutionary changes, but the changes are what I call »conservative revolutions,« not revolutions in the strong sense that Thomas Kuhn suggested, namely, new paradigms incompatible with and totally different from the old ones. At least within the physical sciences, I think one can reasonably claim that there is no insurmountable gap of communication, no deep incommensurability, between the situation in 1900 and that of today. Although science has greatly expanded and resulted in new and much-improved theories, these have been produced largely cumulatively and without a complete break with the past. It has always been important to be able to reproduce the successes of the old theories, and this sensible requirement guarantees a certain continuity in theoretical progress. After all, most experimental facts continue to be facts even in the light of new theories. (The so-called theory-ladenness of facts is generally overrated and often misunderstood.)

It seems to me that the continual progress, which is such a characteristic feature of the modern development of the sciences, is a strong argument that science is indeed epistemically privileged as compared to other ways of obtaining knowledge. There simply is no other kind of knowledge that has a degree of reliability, precision and stability just remotely comparable to that which the sciences are able to produce. Of course this privileged way of gaining knowledge is limited in scope and applicability, because it only refers to the domains of science - typically domains that can be subjected to experimentation and mathematical analysis - but this is another matter which in no way questions the epistemic superiority of scientific knowledge.

It is often claimed, as Fuller does in his book, that the peer review process mostly works conservatively, and perhaps symbolically, in modern science. That it, and the entire culture of modern science, rewards inertial and orthodox knowledge that rests safely within the limits of normal science. »Science,« Fuller says, »is a society designed to suppress conflict rather than resolve it through either peaceful consensus or open warfare« [1, p. 22]. In science of the big type, he and other critics claim, refutation and replication of experiments rarely occur, not only because of the high expenses but also because such work may be detrimental to the career of the scientist. Experimental anomalies, it is sometimes claimed, are excluded in order not to disturb the consensus of normal science. But the picture of science as a society that shuns or suppresses conflict seems to me to be far away from realities [8].

It is true that science in many ways is a system that cultivates conservative virtues, and that aberrations are not easily tolerated. Yet it is not true that scientists normally cling to orthodox theories or suppress data disagreeing with these. So long that data are considered good, they are taken seriously. Scientists routinely feel forced to abandon a cherished theory in light of experimental evidence, in spite of having a vested interest in the theory. In fact, it is often a scientist's hope to find unexpected results that can refute a well-confirmed theory, and if he or she succeeds in doing so - and the results are confirmed - he or she will be rewarded, not punished. The Nobel prize has sometimes been given to scientists who have confirmed beyond doubt an already established theory, but it has also been awarded to scientists who have successfully challenged such a theory and suggested a new theoretical framework. The latter type of scientific work is generally considered more valuable than the former kind of work, not only as reconstructed historically but also at the time the work was recognised to be important.

In the final part of my discussion, I want briefly to address the so-called crisis in science. This is a crisis that is mostly relevant for the exact and natural sciences but is also sometimes said to be important for the very authority that scientific reasoning has traditionally carried with it. If we cannot trust the results of science - if we cannot believe in science - then what can we trust, what can we believe in? There are people who portray science as an activity with a great past but without a great future. They have reasons, if not necessarily convincing ones, to paint a portray in such dark colours. We cannot take it for granted that at the next turn of the century there will exist a science system of the same magnitude, vitality and authority as we know today. As Fuller rightly points out [1, p. 34], nothing in the nature of society demands that it has an institution specifically devoted to the pursuit of knowledge as pure inquiry. Science is historically contingent, it is neither a necessary nor a natural part of human society. Indeed, in many ways it is a most unnatural activity. From this point of view it may not be totally absurd to imagine a future society in which science is given much less priority or does no longer exist in the form we know it today.

I cannot go into the details of this problem area, but at least I can try to demystify it a little by pointing out that we really have two kinds of problems that are quite different, but are unfortunately often mixed up. On the one hand, there is the political problem of peoples' interest and trust in science, their willingness to pay for the production of scientific knowledge that they will, in most cases, not benefit from. It is possible, but in my view unlikely, that political decision-makers will one day reach the conclusion that science is not worth paying for, at least not on the present level. It is possible that they will decide to scale down drastically scientific research. When I consider this scenario to be unlikely, it is mostly because of the role that science plays as a productive force, the recognition that science sometimes results in new technologies and then, supposedly, economic growth.

The other aspect of the crisis of science is not directly related to politics and money but rather to the cognitive dimensions of science. It is closely related to the end-of-science discussion [9]. In several sciences it may look as if we are approaching a kind of complete picture, or at least that we have a basic knowledge that will not change substantially in the future. And if this is the case, how interesting and worthwhile will it then be to go on investigating the finer details, to explore ever more exotic regions of nature, especially when these regions can only be studied by means of very big and very expensive apparatus. It has been argued that there exists in science a »law of diminishing return,« a kind of logaritmic relation between the value of the knowledge and the resources used to obtain this knowledge [10]. The typical example is high-energy physics, where attempts to explore very high energies may require unrealistically large and expensive accelerators. Whether or not one finds a $6-billion accelerator to be justified (the Large Hadron Collider), there is of course a limit for the energies that can be obtained in laboratories. Hence there is a limit for what can be known scientifically, or at least experimentally. But it should not come as a surprise that there are limits to scientific knowledge.

There are different views concerning the end-of-science question, or rather the completeness-of-science question. As to myself, I do not think that science will ever become »complete« in the sense that there will be no more interesting things to explore, no more surprises. But I do think that large parts of science, and the physical sciences in particular, are approaching a kind of stable or »finished« state, in the sense that our fundamental theories will not change drastically in the future, not even in the far future [7, pp. 122-30]. We can smile at the naivité of the fin-de-siècle scientists who believed that physics had already reached its final state, but their failure does not imply that no such final state exists. Because most scientific theories proposed during history have turned out to be wrong, it does not follow that those accepted today are wrong as well and will be replaced by entirely new theories.

In my view, it becomes still more unlikely that scientists have missed some big and important aspect of nature, that new discoveries or theoretical frameworks will force upon science a true revolution of the magnitude and conceptual depth that occurred in the seventeenth century when the new natural philosophy replaced the Aristotelian system. The modern army of scientists and their arsenal of sophisticated high-precision instruments just make it very difficult for important phenomena to remain hidden. Moreover, the best theories we have are so thoroughly tested and so closely bound in a larger network of theories and experiments that I find it difficult to imagine that they can be entirely wrong. So, in my view it is indeed possible that the pattern of progress in science will change, and that many of the most fundamental aspects will remain as they are now known. There will always be exciting work to do and discoveries to make, but it is far from certain that the development of science in the twenty-first century will be as explosive as it has been in the twentieth century.

Many scientists, recalling earlier periods of pessimism in the sciences, will find such relative pessimism unwarranted. I consider it a realistic scenario, but of course it is guesswork. Perhaps we should consider, in a long perspective, the stormy development in the last half of the twentieth century as an aberration rather than the beginning of a new phase of science development. I cannot express this feeling any better than quoting Leo Kadanoff, a leading American physicist, who in 1992 wrote: »In recent decades, science has had high rewards and has been at the center of social interest and concern. We should not be surprised if this anomaly disappears. We will all be disappointed and hurt by this likely development. But if we can back away and look at the situation with some perspective, all of us in science can say that we have been lucky to be part of a worthwhile enterprise« [2, p. 408].

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Scientists, Biotech Science and Society

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First in this note some general observations on pressures on scientists (with in any field whatsoever) is reported, and secondly, more special remarks will be made on pressures on scientists situated within basic research in molecular biology, biochemistry or related areas with potentially or strategically relevance to biotechnology. This leads finally to the issue of rationality and the possibility of a democratic governance of science.

1. What everybody knows about science as a job (if not vocation)

Generally, two sorts of pressures on science may be distinguished; the pressures originating from the shifting demands of the external social-political body of interests, and pressures originating from the internal structure of science as a business, a job, or a vocation.

In principle, if doing science is simply a legitimate job as another, one can argue that the internal pressures of the very 'job culture' of science (compared to other jobs) is the most salient aspect of pressures, at least from the point of view of the practising researcher. To investigate this suggestion, an empirical pilot project was conducted employing the method of qualitative in-depth ('thick') interview about the socio-psychological working environment of scientists. Of course not by interviewing scientists, as their subjective perception of their situation may be much influenced by the still-dominant ideology of science as a vocation and may not adequately represent the real burdens and challenges of their jobs, - but by interviewing relatives who live with scientists in a sphere of life which may not primarily be called scientific (e.g., their spouse, children, mother-in-laws, etceteras). Due to the pilot status of this investigation and the available time, the number of interviewed persons was rather limited[1] but the results were quite conclusive and can be listed as follows (see Table 1):

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1 In fact: one (the author's wife). Details of this study are likely to be published in a technical report (Emmeche, under press).

Table 1: Internal pressures on the scientist

- The wage is to little (for a day and night working job)
- The scientist is married to his/her job
- Scientists have to race for merits all the time
- They have to do both teaching, administration and research (though they may only be qualified for the latter) [poor students !]

To this general scheme one should add a certain cultural estrangement facing at least the newcomers into science. If you are not born into the academic culture, you have to acquire it, as you acquire a non-native language which may never becomes quite as natural as your mother tongue. Language is closely tied to culture, and one visible feature of this phenomenon is the difference between native and non-native speakers in outer performance, rhetorics and style of presentation, that is easily observed on international conferences. Though the linguistic difference may seem rather superficial, it plays, I think, a real role in the total pattern of pressures. One of its effects is that many scholars and social scientists from the non-English world have really a kind of double professional lives, which may complicate their career: They have, sort of, one research and public profile in their home country and another(and often weaker) one as international partaker of their field. The easiest solution is of course to become as much integrated and internationalised as possible (and on this local spot on Earth 'internationalisation' has for long been a catchword in science policy debates), but there may indeed be some national constraints on this move, as you may still be motivated for specific reasons to do some work in your national tongue (for instance: national history) that will remain invisible to your international peers unless translated, a process which is often unlikely to be followed in case of work done in a strictly local context. This tendency is probably most salient for social science and the humanities, while in mathematics, physics, chemistry and molecular biology, almost all work is published in English. Fields like ecology, traditional biology and geography is in between, praising internationalism as a formal value and universality as an epistemic value while being situated most often by their objects of study in a national context.

Another manifestation of the linguistic constraint is that even if you may fully master the English lingo of your own sub field, meeting another one(e.g., in cross-disciplinary work) immediately throws you into bewilderment. But of course, linguistic and cultural estrangement is not the same. Having been in a scientific field for more than 10 years you tend to become blind to its strange rituals and authoritarian aspects, and 10 years is often the amount of time it takes just to get a normal permanent job within that business. Thus, in the next table we can add the following observations:

Table 2: Pressures on the wannabe scientist

- Extreme career uncertainty
- Cultural estrangement (»blind search for a scientific identity«)
- Linguistic handicap (for non-anglophones)
- Academian Angst (»Am I really fit for scholarly fight?«)
- High-risk for slave-like relations to supervisors
- High-risk for self deceit (»My work is indispensable to the field!«)
- High-risk for cynicism (»If these fools can do it, so can I !«)

It will take us too far to scrutinize the details of each of these pressures, but they play no insignificant role in the early career of scientists. In other words, the social psychology enforced upon the coming practitioners of science can be seen as the worst possible case for a formerly normal collective trade union consciousness of a group of workers: Cynical, narcissistic, neurotic, or even quasi-psychopathic personality characters may thrive in the social settings of science career recruitment. If this description depicts some aspects of the scientist as a weberian ideal type, I will hope that there are some serious flaws in my analysis, or I'll pray to God that there still be some distance between the ideal and reality.

Fortunately, looking at the risks mentioned in table 2, you could turn the mon their head and look at them not so much as an exotic science culture whipping its would-be members, but rather as set of psychological incentives (such as respect to the more experienced members, deep devotion, optimism and self-esteem) mediated to an organization's human capital in order to engage into a complex web of commitments, training and skill-development which is typical of any career-oriented form of life (as opposed to a typical wage-earner form of life where the job is just a means for living, and is not supposed to make up the purpose of life). In that sense these psychological pressures on science may not be so different from pressures on any other career-focused way of living.

Summarising what everybody knows, these schemes are not surprising, but when one adds to the picture the fact that most research areas are intensely specialised and enroll their practitioners through a highly competitive selection process, it is fairly obvious that the social psychology of a person who survive and even thrive within a scientific speciality - such as genetics and molecular biology, to which we now turn - is not necessarily one that predestines a research fellow to face in a sensible way the challenges of the 'external' pressures on science. I am not alluding to the mythological characters like Dr. Jekyll/Mr. Hyde; the standard geneticist may simply be a seemingly innocent, busy nerd, trying to keep a place in his own field, and running after new genes and patents like a Red Queen.

2. Biotech science and scientists

To the public, gene technology and biotechnology are words more or less synonymous with 'the new genetics' and other terms that usually denote basic research. I have often heard university researchers in the genetics and biochemistry departments - when confronted with the newspapers' ongoing debates about risks and ethical concerns with biotechnology -express a deep frustration over the inability of politicians, journalists, representative of environmental movements and other NGO's to distinguish clearly between science and technology. They feel that their basic motivations for doing science are misrepresented when their research is seen by the public as a species of biotechnology. This frustration may in part derive from the feeling of not being understood by the public media as a group doing hard but honest work, ultimately searching for a deeper and true understanding of nature. I remember the proud exclamation of a biochemist, having published his recent work on a new regulatory factor in a biosynthesis pathway, saying something like »this finding is a permanent achievement, it will stand the oblivion of times as opposed to anything you find in the newspapers« - for what he could see in the newspapers relating to his own research was typically concerns about its potential ethical problems or environmental risks, and in general a debate dominated by techno-scepticism or what one may call a clumsy version of a hermeneutics of suspicion towards 'techno science', a term he hated as he found it intimidating his real interests and deep motivations.

We can say that the more a society fail to distinguish between science and technology, the more will the individual scientist long for a time and a place, where clear-cut borders between truth and use can be imagined.

Be that as it may, the university microbiologists often tend to ignore in this context that their own institutes and research projects, in a long period from the mid-1980s until quite recently (where the picture gets more diffuse) have prospered, compared to other university specialities, in part because of this metonymic failure of seeing the deep difference between science and technology. (Some scientists have even prospered in a more direct way venturing into their new identity as partakers and stockholders of new biotech firms).

This prosperity is especially visible when the large biotechnology programmes from the mid-1980s were launched by the state's science foundations and suddenly all within biochemistry and microbiology departments began to see the financial advantage of labeling their research as a species of biotechnology. Of course, this is to state the history a little crudely, because many Centres were established, for instance by the Danish Basic Research Foundation ['Grundforskningsfonden'], with the explicit purpose of doing basic, not »applied«, research in subjects like plant genetics, protein chemistry, or bacterial biochemistry, but everybody knew of course that there is often a quite blurred demarcation line between basic science and applied research, even from the point of view of university disciplines: The way from a new discovery in for instance enzyme biochemistry to the breakthrough of a technological application of that knowledge is in many cases relatively short. Biotechnology is generally knowledge-intensive and the knowledge concerned is usually about basic mechanisms of living nature. Knowing what modifies the self-assembly of an enzyme is thus a Janushead of *both* revealing one tiny part of Nature's most fascinating secrets, *and* adding a potential powerful tool to the palette of industrial production mechanisms of pharmaceutics, foods, materials, etceteras.

Thus the researcher within biotechnology is supposed to be a little squint-eyed, being able to focus *both* on 'strategically'(or potentially applicable) research subjects such as those that appear in the headings of the programmes from the official funding agencies - even though nobody may have an idea whether his or her particular discoveries turn out to become 'strategically' relevant - *and* to focus on the paradigmatic puzzles of his basic field, where he is supposed to make excellent basic research as judged by the pure internal standards of his own discipline. In that sense, our molecular biologist cannot live a life within his or her own secluded university territory, but has to be enough willing to engage in cross-disciplinary work-themes (often involving research representatives from private biotech firms) in order to secure a position as a strong applicant to the scarce grants. This tendency is underscored by the planned establishment in Copenhagen of a new big »Biotech Research and Innovation Centre« (BRIC) housing both university departments and biotech laboratories of private companies.

The molecular biologist of tomorrow has to be a cosmopolite both as a scientist and as a science funder, however, this kind of required double sensitivity will not necessarily make him as squint-eyed as Sartre or a kindred intellectual who »interfere with things that are not his own business«, if these 'things' are taken to mean the public debate about the broader impacts and alternative strategies of biotechnology development. Even when a scientist thinks that an issue is his expert business, he can receive a black eye if his superiors or external agencies think he's too squint. As recent case in point is that of Dr. Arpad Pusztai from the Rowett Research Institute who pronounced in a television programme that his own research indicated that genetically engineered food may not be as safe as usually assumed, and who was suspended from his position afterwards (Bøgh-Hansen 2000, Lembcke 2000). Even though normally, and hopefully, it is not quite that dangerous for a researcher to participate in public debates on controversial matters as an 'expert', we don't perceive the degree to which the mixture of self-censure and explicitly imposed muzzles impede the participation of researchers in public debates -we only notice cases of breakdowns of habits. The Pusztai case has emphasized the need for true independent research, not only into the development of more biotechnology, but also into its consequences and especially into various alternative developmental strategies in for instance agriculture for the use of genetically modified organisms (GMO).

Much of the public debate about GMOs seems to indicate a partial failure of translating the innovations of techno science's nature-culture hybrids into politically adequate categories: That an creature is a Genetically Modified Organism need not in itself constitute a problem if it has been established that this organism is also what we might call an ESO: an Ecologically Sustainable Organism. The difference between ESOs and non-ESOs is much more politically crucial than the difference between GMOs and non-GMOs. As a community it is critical that we can distinguish the kinds of organisms and related agricultural strategies that safely can be applied in food production from the kinds of organisms with considerable risks for health and environment. Therefore we need independent research helping to establish good conditions for a societal control that contributes to secure that any GMO is also an ESO. Research like Dr. Pusztai's was a contribution to that.

One may wonder how independent the status of researchers from various areas of biotechnology research will be judged given the tendency to merge university and private company research. In this context it is interesting to note that in a recent pamphlet about the above-mentioned industrial-university

BRIC centre written by two of its initiators (Bock & Brinkner 2000) the authors sensitively state that:

> »... generally new technology today is met with scepticism. Scientific progress is not automatically secured a popular support. This is also true of biotechnology. The possible advantages it can bring in the form of decreased effects of poisons and pollution, are not perceived as obvious but is met by opinions ranging from sound scepticism about the ability of science to predict and control the powers it releases to a principal aversion to manipulate genes and the hereditary material.
>
> Therefore, the research needs to get into dialogue with the rest of society. An important prerequisite for this is that researchers use time and resources to engage into the ethical questions related to their research. Many researchers seemingly think that they can run their research professionally and have their opinions as a private matter. If scientists instead stand forward and acknowledge that they have some attitudes and go into a discussion with the surrounding society about these, then their work will become more understandable and will meet greater respect in the population. Furthermore it appears to be the case that researchers, who have participated in the debate, come out with a more nuanced perception of their own work than they originally had.« *(ibid.)*[2]

Are we then seeing, here at the turn of the century, signs of a break away from one important element of the *positivistic* ethos of science, not only characteristic of the Vienna Circle philosophers of science but traditionally shared by most natural scientists, namely the central idea that the political discussion of the use or misuse of scientific knowledge should be kept completely separate from the discussion of that very knowledge and how to pursue it?

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2 Bock and Brinkner 2000 (my translation):«generelt mødes ny teknologi i dag med skepsis. Videnskabelige fremskridt er ikke automatisk sikret folkelig opbakning. Dette gælder også for bioteknologien. De mulige fordele, som den kan medføre i form af mindre giftpåvirkning og forurening, opfattes ikke som indlysende, men mødes af holdninger, som strækker sig fra sund skepsis over for videnskabens evne til at forudse og kontrollere de kræfter, den slipper løs til en principiel uvilje mod, at der pilles ved generne og arvemassen.
Forskningen har derfor behov for at komme i dialog med det omgivende samfund. En vigtig forudsætning herfor er, at forskerne bruger tid og kræfter på at interessere sig for de etiske spørgsmål som knytter sig til deres forskning. Mange forskere mener tilsyneladende, at de kan drive forskning professionelt og have deres holdninger som en privat sag. Hvis forskerne i stedet står frem og ved gør, at de har nogle holdninger og går ind i en diskussion med det omgivende samfund om dem, så vil deres arbejde blive mere forståeligt og vil nyde større respekt i befolkningen. Desuden viser det sig, at forskere, der tager del i debatten ,kommer ud af den med en mere nuanceret opfattelse af deres eget arbejde, end de oprindeligt havde.«

After 40 years of discussion about science, technology and society - the atom bomb, the population bomb, the green revolution, the environmental crisis, the new eugenics - I think that we finally see some attempts at a radical re-orientation of a scientific ethos involving a break through the wall between inner and outer values of science, where scientists themselves claim some responsibility for their findings and try to influence the system of political decisions to secure a rational and peaceful exploitation of the knowledge goods.[3] Ideally, taken to the limit, it should no longer be possible to uphold the idea of doing good science measured by the internal standards if its consequences measured by the society's standards are bad.

3. Scientific rationality and consumer governance of science

However, in that same movement, we see a radical question about the nature of rationality raised - what is 'rational use'?, to whom should these goods be valuable?, by what means can a democratic society conduct a rational discussion of these matters? In the mass media we see many outspoken concerns about the use of scientific knowledge, but to what extent does it make sense to see this as an instance of a rational debate? Very often it seems as if scientists have one form of rationality, for instance apply some operational concept of risks in the question of GMOs and 'GE food' (genetically engineered food), while the majority of a society's individuals have quite other rational notions of risks, e.g., some dangers you are simply not willing to negotiate about by weighing the costs and benefits, even though the probability for (catastrophic) accidents eventually is infinitely small (Iversen 2000).

It is quite evident that the above mentioned break in the positivistic ethos imposes a new structure on the pressures of science and scientists. Just as a modern private firm can no longer keep its share of the market by just using the usual profit-maximising algorithms of buy, sell and exploit, but has to be sensitive to a whole gallery of new economical factors or 'partners' ('interested parties') such as the internal work environment, the local society, its culture, the ecosystem, and future generations (this is at least what we are told by economists and managers); in the same manner, the recipients of knowledge production are no longer just students, other scientists and engineers, but in a way the same extended gallery of interests that demand a new accountability by the research society.

3 The passage cited is just one sign of such a tendency, however, the author 'side a of scientists' participation in the debate is possibly first and foremost thought of as a means to gain public acceptance of biotechnology generally.

The question about this new relation is of course two-sided: How can scientists be prepared to face this new challenge (will it require a new set of competencies incorporated in their basic research education? - I think the answer is yes: a molecular biologists should have some training in ethics and philosophy of science), and how can society become better equipped to enter into this new dialogue with science (for instance by the use of more scientifically informed lay consensus conferences, which have been done several times in Denmark with some success)?

The debate about the potential dangers of GMOs for the environment and of GE food for people provide an excellent example of the new structure of the pressures of science in a modern risk society.[4] It also shows how difficult it would be to extend the democratic idea of a society's technology assessment (which is in Denmark often exemplified by the engaged public debate in the 1970s and 80s about nuclear power finally resulting in the official refusal of allowing for the use of nuclear power plants in Denmark) to an idea of a society's democratic assessment of science, perhaps something like Fuller's ideal of a republican governance of science (Fuller 2000). One of the differences between nuclear power and gene technology is that the first is a case of 'big technology in one packet', the latter is a case of medium-size technology in a wealth of possible packets. Furthermore, in the atom power debate, you could fairly easily locate the scientific experts, while in biotechnology the range and composition of expertise needed to access the whole spectrum of consequences and potentialities are broader and far more complicated.

Many scientists think that the public reactions to new risks and potentials are short-sighted, irrational, and subject to random shifts in the public political atmosphere. This is illustrated by the field of plant genetics in Denmark (and elsewhere in Europe) which is facing a financial crisis. There are strong plant genetics groups both within the universities (especially Aarhus), in the public sector of non-university research such as 'Agricultural Research in Denmark' [Danmarks Jordbrugsforskning], and within the private sector (at Carlsberg, Danisco and DLF-Trifolium), and all of them are encountering harsh times, partly due to the lack of dedicated plant genetics programmes within the 5th EC frame programme, partly due to the firms' deliberate slowing-down of their research within this field. It is clear that this slowing down is linked to the big uncertainties concerning the use of genetically modified plants for food in Europe, caused by the scepticism about the real value of such foods and their eventual health risks, that led to a three year moratorium in EC for

4 See also Hansson & Horst (2000). I thank Søren Wenneberg for bringing their work to my attention after the conference »Science under Pressure«.

marketing of gene manipulated plants. Research leader at DLF Trifolium Klaus K. Nielsen states that »There are no official statements against development of plant biotechnology from the EC politicians, but the whole research atmosphere is marked by a hidden political agenda against gene-spliced plants« (Ammitzbøll 2000). In the US, research in this area are running in high speed, and many are fearing a drain of researchers from Europe to US. According to John Mundy from the University of Copenhagen it is already a fact that Europe is leaving plant genetics to US where the firms have a much more liberal legislation to work under (ibid.). For the EC part the problem is not just with plant genetics. Rumours will have it that European science funding is in crisis, as expressed recently by Andrew More from EMBO (European Molecular Biology Organization) who proclaimed that

- »European policy makers have increasingly turned their back on free thought and creative brilliance, and concentrated more on a demand- and applicability-driven scientific culture. Although targeted research is, without doubt, necessary to tackle pressing social demands, basic research, driven primarily by a hunger for knowledge, has given rise to products of spectacular usefulness and completely new fields of research. (...) the established EC funding ethos persists, and by over-concentrating on asking what the needs of society are, is liable to
- over-fund demand-led short-term science, hence damaging basic research and restricting the pool of ideas from which applications can spring; (...),
- convert the research programme into an instrument for commercial research, which is best funded and performed in a commercial rather than an academic context« (Moore 2000).

Returning to plant genetics, could we not see this as a beautiful example of the democratic governance of science? Here we have a society - of European consumers - who does not like the knowledge products that a scientific discipline manufactures, because the consumers simply do not like the idea of eating genes in such food as tomatoes - of course manipulated genes, but a recent survey of the lay people's information level about genetics have shown that about 50% of the population don't think they eat genes when they don't eat GE food. No wonder that scientists think people are irrational. However, the consumers' choice may be completely rational seen from a traditional economic or even ethical point of view: When I can buy nice natural tomatoes today that satisfy my basic needs, why should I be willing to choose GE foods - or even choose a situation where my choices become more complicated? It's completely rational to say nay!

Nevertheless, it is even not that simple. In addition to a my opic natural science rationality and a seemingly opposed lebenswelt or everyday rationality, there are some global issues at stake. Is it really advisable to let the rich world's republic of consumers rule the republic of science?[5] Could the close down of non-popular basic research be short-sighted? The plant geneticist Birger Lindberg Møller - who work in the 'Cassava-project' that aims at understanding the basic biochemistry of the poisonous cyanogene glucosids that today in many countries of the third world has to be removed manually by a very work-demanding process before the cassava roots can serve as food - pose the question in this way:

> »How are the right decisions made about priorities in this area in a democratic society, when a large part of the population (...) have no real interest in natural science and therefore not always understand the cultural import of science? Or, when we instead of an enlightened democracy have an 'a shifting atmosphere democracy' in which the lack of real visions is replaced by a discussion of random single cases, directed by the media and interest organizations with low membership numbers. What is needed is specialist considerations based on a solid scientific background combined with common sense and ethical reflections. (...) The debate is muddled as long as we don't politically decide priority on the basis of a stand to the general problems and as long as we, in the West, do not to a higher extent take responsibility and help the developing countries« (Møller 2000: 46).[6]

By this I only want to conclude by saying that governance of science today is not only a challenge for each country's public and their scientists, it is an international challenge that demands increased power to democratic international (but hardly yet existing) agencies to help re-direct the development of applied science toward the needs for humanity, which is not just identical with the need for American or European consumers.

5 Sorry for the phrase, I am not implying that this should be what Fuller (2000) calls the 'governance of science'.

6 My translation, CE (»Hvordan træffes de rigtige beslutninger vedrørende prioritering på dette område i et demokratisk samfund, når en stor del af befolkningen ikke har nogen særlig interesse i naturvidenskab og derfor heller ikke altid helt forstår naturvidenskabens kulturelle betydning? Eller når vi i stedet for et oplyst demokrati har et stemningsdemokrati, hvor manglen på egentlige visioner erstattes med en diskussion af tilfældigt udvalgte enkeltsager styret af medier og interesseorganisationer med ringe medlemstal? Det, der kræves, er faglige overvejelser baseret på en solid naturvidenskabelig baggrund kombineret med sund fornuft og etiske overvejelser. (...) Debatten forplumres så længe der fra politisk hold ikke bliver prioriteret på basis af en holdning til de overordnede problemstillinger, og så længe vi i Vesten ikke i større grad påtager os det ansvar at hjælpe udviklingslandene.«, Møller 2000).

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Science under pressure?
- the challenge of the global environment.

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Abstract:
It is posited that »science« is *not* under pressure in the conventional resource allocation meaning of the term. Rather *if* science is under pressure, then it is a pressure to respond to, and assist in delivering solutions to the most urgent global environmental problems and basic needs of the global economy.

The pressure is therefore one of challenge rather than one of contraction of opportunity or remit.

1. Introduction
It is not so long ago that in the United Kingdom, at least, the social sciences were under pressure: this was of a sociological, ideological and political nature. Sir Keith Joseph in the Mrs. Thatcher political administration called for a review (later undertaken by Lord Rothschild) of the Social Science Research Council (one of the then five main Research Councils which funded research in British Universities). Some thought (and perhaps hoped) that the SSRC would not survive the review. However, Lord Rothschild gave it a clean bill of health in his extensive review and the SSRC did survive much to the relief of many social scientists, albeit with it's new name as the Economic and Social Research Council. The term »science« was dropped from the name of the Council - an ominous development. Nevertheless the SSRC (ESRC) was seen by Lord Rothschild as highly productive, serving a very useful and relevant social purpose for society at large and for the development of the social sciences in general.

We can see, here, that a whole academic arena *can* come under pressure, under attack if you will, for a variety of reasons. Science, »true science«(?), the natural- physical and engineering sciences felt secure for many, many, years. The science budgets, the »science vote »allocated by the State grew decade-by-decade.... most especially during the post-Second World War period of 1950 - 1980. To some degree, however, as economies faltered, as the pro-

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mise of science became perhaps more circumspect, as »big industry« developed its own scientific research infrastructure and momentum, and as »big science« (eg. particle physic's accelerators, space research etc) became ever more gargantuanly expensive...and perhaps offered a sort of law of diminishing returns in the gains to be made..... then to some extent various science-policy analysts have suggested that the science vote, the science budget, faltered, if not plateaued... that indeed science had reached a sort of »steady-state« (see Ziman,1987). Various international statistics question that conclusion - for many countries the science budget is as big as ever...albeit concentrated in fewer organisations as a consequence of different forms of tiering and restructuring of academe. (see Whiston 1988).

At the same time, however, society at large (viz. the political and corporate process) seemed to demand, on the one hand, greater »relevance« as to the direction or trajectory that science might take and the areas that it might explore and contribute towards. Whilst it is debateable whether or not one can plan a science agenda (though scientific planning was very much the case during the War Years) nevertheless in an effort to do such a thing, many nations moved towards a much greater emphasis upon »directive mode funding« (see Whiston 1988) away from the more laissez-faire, freer »responsive mode« research funding of earlier years whereby scientists themselves and the academic community in general set their own research agenda.. Indeed, in the UK, in the earlier years, with the existence of the University Grants Committee (the UGC) then that body saw its role in many ways as to defend the universities from overdue governmental (State) interference in a bid to maintain academic freedom. The composition of the UGC (which included many university vice-chancellors) helped considerably in that respect. However the later transition to the Higher Education Funding Council and the University Funding Council structures with much smaller university representation and much greater industrial representation did not any longer so strongly resist the role of the State in defining the role and remit of science.

Various other means were explored by many countries as a means of achieving greater central planning and control and accountability of scientific endeavour in the universities. (See Whiston 1988 and Whiston and Geiger 1992 for a review of the techniques explored.). Many of the approaches - for example restructuring of universities, concentration policies, big is beautiful, interdisciplinary laboratories, were essentially exploratory with little empirical evidence as to the extent to which scientific productivity would be changed or improved (Whiston 1988). Even so, with the new emphasis upon larger research teams, research evaluations (RAEs), restructuring and selectivity exercises, and the ever greater emphasis upon publications (which is not the same as wider dissemination), plus increasing reliance upon »foresight approaches« with regard to the definition of the science-portfolio (borrowed from earlier attempts in Japan - see Irvine and Martin 1984) then many scientists *did* feel under pressure and threatened. This was not necessarily in relation to the amount of funds available, rather than the control and direction of the funding schemata. In that total sense, then, science was perhaps under pressure. Whether or not it was under the *right sort* of pressure is however a moot point. Thus, some would suggest that the »correct« pressure would not to be to seek to maximise publications - scientists are clever and they soon learnt how to play that game - but rather to maximise the social effectiveness and utility function of science. Directive mode funding, national plans, do not necessarily guarantee that greater social utility is adhered to. Thus a more specific emphasis, more refined targeting of pressure points, might be more appropriate if the real aim of control of science were upon the seeking of solutions to wider societal problems and the facilitation of the applicative.

Having made those points there are many ways in which science *could* be seen to be under pressure, other than the above. Thus there could be:

(I) A scepticism as to the value of science and what it can deliver. Funding does not have to be viewed in absolute terms but rather in relative terms viewed against the various competing calls upon the public purse.

(II) There can be a recruitment problem, an »image problem« whereby the young - the new cohorts of students both entering and leaving the university- no longer wish to enrol in the earlier scientific agenda of yesteryear, they do not wish to take science courses or undertake science careers. Indeed academe itself may no longer be so attractive to them as it was to their forefathers.

(III) Related to (ii), many may see a career in academe as no longer a glittering prize for many reasons. And they may see, in financial and security terms, a more robust career path in other professions: banking, law, accountancy, applied medicine, commerce and other professions.

(IV) Also with the questioning of the »linear model« of science toward a much more messy, organic, interwoven networking (see Gibbons et al 1994) the academic scientist and science in itself may become lost in a forest of interconnectivity and thereby give the impression that science, at least in academe, is on the retreat. In one sense this is a form of signal-to-noise problem. The actuality may well be that there is just as much »science being carried out«...but it is just in another place.

Any or all of the above could seem to give the impression that Science is under pressure - and that might be reinforced if society, or even the science community themselves, believe that the major tasks of science, especially say of chemistry and physics have been »solved«....and that not much remains to be done. (Most scientists would totally reject that notion. They would point to the newly emerging areas - the cognitive sciences, the neurobiological areas etc as wide open, yet to be explored fields. Any pressure that exists would then be seen as a selective attention problem).

But in one important way this is to entirely miss the point as to why science is not under pressure. For it is society at large which is »under pressure« and that societal or increasing global pressure generates the most extensive and challenging opportunities, indeed urgencies that science and its application has ever encountered. Indeed it is much greater, even than that which had to be faced during the course of World War II - when in many ways scientific knowledge and its application flourished. For we now face a new or at least attenuated »global war challenge« if you will, to which I turn shortly. Answering that challenge will require enormous funds, enormous scientific endeavour, legions of scientists, engineers and technicians. It will require multidisciplinary and interdisciplinary teams and new modes of thinking. The pressure, then, will not be on science per se rather than for science and its adequate delivery of its services.

2. The new cumulative global agenda

It is common place, now to refer to the enormous global environmental pressure which the world faces: six billion people inhabiting the planet (more than twice the number of only a few decades earlier) which may well rise to more than nine billion in a few more decades; a North-South divide whereby the South remains both relatively and absolutely poor (with a few notable exceptions) and the OECD countries ever richer and industrialised; the »global commons problem« - climate change, global warming, ozone depletion, oceanic pollution, species loss, decaying cities (uncontrollable megopolises), extensive pollution.... plus the fear that the part that medical advances made in previous decades may well not be able to cope with the ever faster virus mutation that we now have to fight against; food and agricultural problems made ever more complex by the uncertainties and associative or »manufactured risks« of GMOs, cloning and genetic modifications that may open up a Pandora's box of generic problems; all of this within the wider context of the problems of so-called »dualism« in the countries of the South whereby 80% of the 80% of the world's population who presently inhabit the planet viz. more than 64%,two-thirds of the world's population live in destitution and squalour. The latter not being able to respond to the environmental threats which now threaten to overwhelm the world... as I say all of this is now taken as legion, and will not to be detailed here for an enormous literature has by now developed (see Whiston et al 1992 and Whiston 2000).

Against all of these problems various analysts, especially those of a neo-Malthusian turn of mind (eg. Meadows and Meadows 1972, 1992 - Limits to Growth school) called for all growth to stop. That message was rejected by numerous other analysts (see for example Cole et al 1973, Freeman and Jahoda 1978, Whiston 1979) who called for many social, political, scientific and technological innovations as a means of addressing the world's problems. Similarly a wider world analysis and concern later emerged whereby analysts turned towards, and called for, so-called »sustainable development« despite the near impossibility of any really adequate definition of that term. Thus »sustainability, or more so, sustainable development« sounds good. Numerous agencies and Reports (Brundtland, Brandt Commission, the Rio Conference, Kyoto) have all called for changes in the scientific and technological agenda of the world within that new paradigm. Improved technology transfer between North and South, improved knowledge transfer have been the subject of much study and debate aimed at the improvement of the development process of so-called less developed nations. Similarly many have called for radical changes in the framework-thinking and ideology of such world institutions as the World Bank, The World Trade Organisation (WTO), the IMF and GATT towards an ethos whereby science and technology might function much more effectively against the challenges and global problems listed earlier. Similarly many have called for the greater accountability and control of Transnational Corporations and Multinational Corporations who both dominate the world economy and much of its scientific and technological knowledge and research agenda. But most of these calls for change have been much less successful to-date than some hoped for. And certainly much less than is required if the scientific agenda of the world, if we might be so pompous as to give it that title, were to be put into place.

Against that background such studies as the EU-FAST Programme entitled »Global Perspective 2010 - tasks for science and technology« examined in detail the challenges of science and technology with respect to the ensuing problems which the world faces. That research programme in calling for several fundamental changes in the contemporary world economic framework (for example the exploration and need of a new Environmental Bretton Woods structure... different from that which history has presently bequeathed us);a modified WTO; and new GATT or IMF arrangements, nevertheless emphasised firstly, what had not been achieved in global environmental terms - as a sort of yeardstick or benchmark of the challenges which still face the world.. Thus Table 1 below indicates what has not been achieved -or more specifically, what problems have become amplified since 1970 (a year when the world's first »Earth Day« was chosen out of recognition of the poor state the world was/is in).

Table 1. Global problems: environmental and resource issues over the period 1970- 1990 viz. the 20 year period following World Earth day (see text)

- Population continued to grow: over the 20 year period 1600 million people were added to the world's population (more than inhabited the planet in 1900). Three billion more people are projected over the next few decades.
- Global warming: CO2 build up and changing climatic conditions. CO2 now rising at 0.4% per year.
- Ozone layer depletion: continues and pollution reached health threatening levels in hundreds of cities and crop damaging levels in scores of countries.
- Loss of fauna and flora: thousands of plant and animal species with which we shared the planet in 1970 no longer exist.
- Oceanic despoilation: continues.
- Forest Loss: since 1970 the world has lost in excess of 200 million hectacres of tree cover (roughly equivalent to an area the size of the U.S. east of the Mississippi River).
- Increasing Deserts: deserts have expanded by 120 million hectacres claiming more land than is presently planted to crops in China.
- Soil erosion and related land loss: the world's farmers have lost an estimated 480 billion tons of top-soil (roughly equivalent to India's crop land).
- Automobiles: the total world car park is estimated to double by the first decade of the new millenium.
- Large Scale mining; single crop reliance; irrigation systems and large scale energy systems (dams, reservoirs) induce tremendous damage.

(Source: Whiston 1992, compiled from data in World Watch Institute Reports. Washington).

Table 2. Major areas for Science and Technology application for the 21st century

- Maintenance of ecospere and biosphere.
- Ecological structure: maintenance of biodiversity.
- Improved food production commensurate with minimisation of environmental and ecological damage.
- Sufficient clean water; efficient use of; recycling and treatment.
- Reduction of pollution and materials waste.

- Land and desert reclaim.
- Ecology of reforestation.
- Minimisation of oceanic despoilation, but utilisation of resource materials.
- Improved farming and agricultural techniques, related irrigation and soil use.

- Less material and energy-intensive systems.
- Greatly improved recycling and materials substitution.
- Low energy catalysis for production of synthetic materials and natural products.
- Biotechnology application to food, health, pollutive and waste control.

- Development of energy systems which are both sustainable and minimally pollutive or environmentally damaging.
- Alternative and renewable energy systems.
- Greatly improved transport systems; improved synergy of communication and transport systems.
- Further development of environmentally friendly technologies, products and processes.
- Greatly improved energy efficiency (transmission, production, use).
- Use of IT in broadest sense, not only in communication and data transmission but where ever it can assist in control and metering of analytic functions.

- Significant progress to reduce educational costs whilst maintaining effectiveness of systems delivery (wide range of technological applications).

- Increased scale of commitment to design of appropriate technologies in a global setting.

- Environmental re-claim of environmentally destitute military areas.

- Attempts to eradicate military applications which threaten global survival (nuclear, chemical, biological).

(Source: Whiston 1992)

Table 2 indicates the sort of scientific challenges which need to be addressed, as identified in the EU-FAST research programme. Nevertheless, extensive as the items are in Table 2 we should note that they are just the tip of an iceberg in relation to the future scientific and technological challenges which lie ahead. Almost every entry can be read, if read with a little imagination, as a challenge to science. It is not just technological *application* which is required. Thus new, more efficient, agricultural systems, *new* transport systems, *new* energy systems, *new* urban development schemes, *new* ways of delivering education, *new* ways of treating air, land and water.... demand *new scientific* agendas at a fundamental level. Irrespective of whether or not this new agenda reflects or results in a vast improvement in our present low level of understanding and applicative technology in relation, say to solar energy, or a hydrogen economy or other forms of hybrid alternative energy systems; or whether it relates to much fuller understanding of, say nitrogen-fixation (of fundamental importance to agricultural systems and subsequent environmental problems); or new low energy catalytic conversion processes which reduce energy requirements in manufacturing and synthetic-material-production by orders of magnitude thereby emulating nature itself (natural photosynthesis does not require high temperatures or high pressure unlike our presently crude plastic and synthetic molecule production systems... everywhere nature through millions of years of evolutionary testing and trial, beats and presently outwits us... and can show us much)then all of these things, and much more, are major challenges to both fundamental and applicative science. *Thus for science... it is a pressure to deliver... not a pressure to contract.*

Yet there is a more subtle requirement. It is of a human resource nature. For on the one hand, the majority of poorer nations do not have a well-developed local »indigenous« scientific and technological infrastructure. Nor do they possess, for economic reasons, a well developed physical infrastructure. Many nations are short of telephones, let alone a well developed IT/Satellite Communication Network. On the other hand it is debateable whether the »South« should seek to emulate the »North« in terms of the North's style of (say) agricultural systems, transport systems, urban development, high material and energy reliance per unit GDP. This is in no way to argue the South should hold back, not develop. On the contrary, for the very requirement of seeking global sustainable development demands that the South raises its standard of living, the quality of life for billions - and has the scientific and technological capacity to make its own arrangements for development along energy and material efficient trajectories. That is the agenda of now and of the future for both North *and* South. But the urgent requirement is to overcome the mistakes of the North in relation to that global, regional and local agenda. The North has already made its mistakes. That is why it emits so much CO2, so much NOx, HCs, dust particles or whatever; that is why it uses energy and materials so inefficiently; that is why its agriculture systems are so energy intensive; that is why many of its cities' transport systems are beginning to clog almost to a halt. The North's agenda came out of an earlier productive, technological and scientific age. It also came out of an earlier economic and social structure which is now seen as inadequate for the challenges ahead. Thus for the South to »copy« the North may be to squander the world's last chance. But for the South to fail to develop.... ensures a different form of global malaise - partly physical and partly social in nature though those two dimensions interact. The global commons ensures that through numerous feedback mechanisms the global delivery of CO2, say from China does just as much harm as the CO2 emitted from the USA or the EU. Inefficient industries anywhere ultimately throw their effluents into a global sea of pollutive despair. The consequence of all of this is that there must be a local indigenous capability in each geographical and social sector of the world. The alternative is a scientic and technological imperialism which extends one mode of thinking and structure in totally inappropriate ways. A critical mass of scientific and technological capability in every nation, which can help to *define, shape, influence* and *respond* to local needs in a more appropriate way than grafting on someone else's inappropriate technology is a requirement *sine qua non.* The need, then, is for indigenous development, not development which is North => South centric. This in turn demands a new scientific age in each part of the world ...who's scientists can then speak and interact as equal partners. Figure 1 below indicates this need in a most simplified form (for a much fuller explanation see Whiston),1996.

Figure 1:

North: S&T primarily defined by North's needs (at present)

o

o

(Two way communication and dialogue required on a vastly increased scale leading to mutual identification of common problems; leapfrogging; global definition of joint interactive problems)

o

o

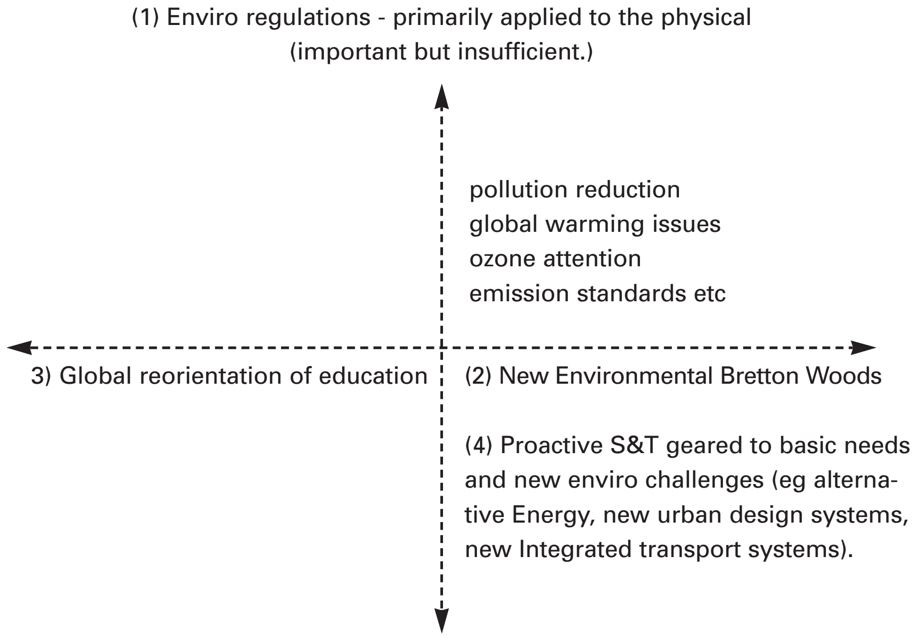
South : Greater indigenous development of S&T base

3. By way of conclusion

From the above it can be seen that there is an enormous need and opportunity for science and it's application over this new century and much beyond. The demand is both in the much richer OECD countries (to re-orient and reconstruct their own infra structural agenda which is so material and energy intensive/per unit GDP - although this indici has been falling somewhat) and also in the so-called less developed countries whether LDC or NIC. Even more so is the need for North-South interaction and collaboration of scientists - of exchanges, dialogue, communication and mutual identification - far greater than is presently the case. No nation should have to re-invent the wheel in any area of science and technological knowledge or application. That is not to say that they should not wish to improve on that original wheel, but they should be able to do so based upon a full, and complete knowledge of what humankind has presently so sorely and tortuously uncovered and engineered. At one time science was »open« through universal publication. In some ways it is much less so now due to the extension of the corporate process. Industry with its large laboratories and science network, the problems of IPR and patent rights in combination serves to limit the open diffusion of leading-edge knowledge. Intellectual property rights, gene »patents«, private software requirements or whatever compromise openness at just the historical moment when openness of scientific knowledge and its subsequent application is most universally needed. Also, if we include 'economists' into the scientific community or fraternity then the scientific task for economists is to develop an economic framework which has a global intergenerational »reach« which sanctifies, everywhere, scientific openness and the widest technological knowledge and the universality of access to that knowledge. Science under that aegis would be under no pressure at all... other than to deliver. At the same time it might well overcome via an increased idealism what we might call the »motivational inertia« of younger people to undertake a scientific career - the life blood of much needed potential talent would then be more assured than is presently so.

It should be recognised, however, that science alone cannot solve the global environmental problems so briefly outlined earlier. The main requirement is for a new socio-economic, political and cultural and ideological framework and agenda. Science - within a new socio-economic agenda has much to offer -even more than it does at present, great though that is. A new socio-economic agenda only better permits science to deliver its full potential. Nevertheless it is science and technology which actually delivers, at least along what we might call the »physicality dimension« of the global problematique to which we have been referring earlier (see figure 2 below)... if it is allowed to (See Whiston 2000)

Figure 2 : Various dimensions of the environmental agenda (see text).



In order to achieve that 'delivery' will almost certainly require different funding patterns of science, different monitoring and evaluation procedures and new institutional structures compared to those which presently prevail. Almost certainly it will demand that the communication and institutional structures around which science is organised and which in many ways controls the framework of science be seen *on a global basis* - not a national or regional one. Just as the EU has started, during the later part of the 20th century, to introduce a »framework for a framework programme« (at the pre-competitive level of research) across 15 or so nations, so as a global community we will have to invent, develop new scientific community structures which intertwine North and South, East and West, much more effectively and intimately than at present. The ancient regimes and institutional structures of (say) IDRC, UNESCO, ICSU, UNEP, IBE or whatever, though important and useful are grossly insufficient for that giant leap that has to be taken if we are to see the global environmental problem as a *global problem* - which has political, economic, cultural and regional interests as its containing boundary condition. Thus, it is not international collaboration »at the margin«

which is called for here rather than a *central core of wide international collaboration.* The majority of research and development is undertaken in the North (see Freeman and Hagerdoorn, 1992 who estimate it may be as high as 90%+ not including Russia) for many obvious economic and historical reasons; see also Hopkins and Wallerstein et al who place this historical concentration in a Triadic trade context involving the USA, EU and Japan and which may over the next three to five decades move towards a Dyadic structure). The North-centric concentration and definition of science and technological trajectories albeit in the Triadic context, has to be corrected if the overwhelming principle of science for the North is to be overcome (which presently shows up, say in the trajectories and pathways being followed by Biotechnology, GMO research, Pharmaceutical research etc) which by and large view the rich of the North as its market place and market niche opportunity - and pays scant attention to the local needs of the South.(see Thomas 1993). This is not offset by the argument that much of the GMO approach will »provide the food for the 21st century« -it is more likely to also control the market and production conditions of LDCs. Thus the development of the South's indigenous scientific and technological capability becomes of the utmost importance. The overwhelming power and influence of the North on the scientific and technological agenda has to be extended, universalised. This is not a »levelling down« requirement it is a »levelling-up imperative«. And within that context we again see, not pressure on science...but challenge and opportunity.

And there is another challenge: at the beginning of this article we referred to the earlier »attack« on the social sciences - an attack that was rebuffed. But much still lingers, which has many facets to it. Thus many natural scientists are sceptical, suspicious, of the social sciences. They view its domain as arbitrary and subjective, unproven and lacking legitimate theory. Equally many social scientists are either generally ignorant or contempuous of the physical and natural sciences -seeing its domain as deterministic and ignorant of the »real« ways of the world - especially the way in which humans interfere with that physical world. Additionally, many classical or neo-classical economists are themselves sceptical of any other ideological framework which counters their thinking. None of this is surprising since industrial and associative scientific imperialism also ensures, for a while 'ideological imperialism'.(see Hopkins and Wallerstein 1996). But all of this has numerous and fundamental or basic implications for the ways in which we view global development, and how we view interactions between North and South, and the 'approved ways of seeing development itself«... and consequently of the role of science and technology and industrial structures. Necessarily, and urgently, this calls for a rapproachment between the physical and social sciences and also the humanities. It calls for interdisciplinary ways of education, training and hence mutual dialogue between these »separate disciplines or ways of thought« and their separate worlds. In educational terms it requires an interdisciplinary spirit and structure much beyond that which presently prevails despite the numerous efforts made in the 1980s and 1990s to engineer such an overlapping of thought (Whiston1986). Ultimately it could mean that not only is neither discipline or perview under attack, but that the social sciences and the natural sciences in combination deliver to society at large much that is beyond their separate efforts. In combination they are much more effective, legitimate and robust.

Social analysts such as Beck (1992) have pointed to the complex risks which the global society now faces. Other analysts such as Giddens (1994) have suggested that we live in a different world, now, whereby neither »Left« nor »Right« can any longer satisfactorily deliver what is required to tackle the global problems which the world faces. Giddens in particular makes much of what he calls »social reflexivity«... pointing towards a more intelligent »dialogic« society which both demands and requires wider involvement, networking, social participation... in a way a new societal agenda matrix. It is debateable whether or not that »reflexivity« is at present common to all societies, but it does point to the need to engage scientists, sociologists, or whoever, in meeting society's changing needs. In deriving mechanisms for such a new dialogue at local, community, regional and international levels any pressure on science becomes softened, made more flexible by a wider social involvement. Thus, under fuller participatory conditions, science is not the subordinate, as she is at present, of the bureaucratic command economy but comes to play a much bigger part in influencing the societal agenda, as well as responsive to its needs. The scientist is then elevated from the role of technician to a more managerial position(albeit modified by other's views) and this greater managerial role thereby diffuses much of the pressure which may emerge in future decades. viz. Science can then operate from a more influential plain than is presently so. Giddens' also talks of »manufactured uncertainty«. Perhaps, just perhaps, with wider and more basic interdisciplinary education at all levels of the education process and also in society at large and also with the closer mutual dialogue of North and South, there just might be less »manufactured uncertainty« with regard to the enormous problems the world now faces - and the role of science, of technology in relation to those problems. The pressure then is a little less on society; the challenge and opportunity to science and to technology that much greater.

To speak of, or call for, a new global scientific agenda which involves all nations as equal partners; to argue for the extreme importance of local indigenous S&T infrastructures in poorer nations can sound like so much »idealism«.
It is important to recognise that S&T must, in the global environmental sense, focus upon not only trying to change the OECD nations' industrial, commercial and social infrastructure towards more sustainable trajectories but also reflect the conditions of the South - and the need to level up our globally polarised societies. Unless this is achieved in a comparatively short time frame in comparison to earlier world history then global sustainability, not just local sustainability is compromised if not impossible to achieve.
All of this requires a very different socio-political paradigm than that which presently dominates. But to many people this may not appear to be a viable perspective: and that, in part, is the nature of the problem.

In that context the reader is asked to return to Table 1 and to consider it very carefully (and much more could be added to the doleful list which comprises Table1). That list, which merely reflects the consequence of how the world economy presently works suggests that the world requires, urgently a different socio-economic framework and a much enhanced role for science and technology - a *proactive role* which involves many levels of the global society...who are presently either ignored or are not represented adequately.

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Globalisation and European Science

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Globalisation

In the following I will first discuss globalisation: what is meant by globalisation, what does it mean for science, and what does it mean for science in Europe? Then the question is asked whether globalisation puts a pressure on science. Finally I will bring forward some of the challenges that are facing science now and in the foreseeable future.

What is globalisation? The topic has become fashionable some years ago. It is one of those concepts, like 'new economy', that mean different things to different people. It is a 'loose concept' because there is no complete and necessary interdependence between the aspects that are mostly seen as linked to globalisation, such as information networks, transnational enterprises and international finance flows.

Some recent statements on globalisation can be found in the writings of, among many others, Sassen, Thurow, Huntingdon and Castells. Sassen writes about political, economic and cultural dimensions of globalisation in 'global cities'. Thurow points out that global market economies of scale and scope are becoming open to everyone, even if they live in relatively small countries and have small home markets. Huntingdon stresses that critical distinctions between people are primarily cultural. For Castells information society and globalisation are two sides of the same coin.

There seems to be agreement on the notion that with globalisation there is free international movement of goods, information and capital. Free international movement of people is not mentioned explicitly. As pointed out by Thurow, globalisation is not complete, but is mainly the case for the three major blocks: United States, Europe and Asia. The main driving force in globalisation is seen as economic: market globalisation, increasing international trade and investment. Economic globalisation, resulting from increasing international trade and investment is being driven by technological innovation. And again, knowledge is key to technological innovation.

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What does globalisation mean for science?

To my mind science has always been globalised, as it knows no borders by definition (Castells, 1996). Science, and accepted knowledge in general, is the systematic and critical accumulation of information, by research and education. Research results in 'codified knowledge', formally embedded in technology, goods, services and procedures; education is the process of transfer that results in 'tacit knowledge', embedded in people as creative, cognitive skills. Progress in knowledge leads to new insights and understanding, and in debunking of old truths. This facilitates decisions, actions, policies, and technological and social applications.

In recent times we have seen a breakthrough of modern technologies such as biotechnology, materials technology, nano-technology and information and communication technology. This has changed our vision of agriculture and food production, of health care, communication and logistics.

The combination of globalisation and technological innovation contributes to the emergence of information and knowledge industries linked through virtual networks. Whether we think this 'new economy' is a revolution or we consider it to be the result of trends that emerged over a long time, it is clear that during the last decades there has been a shift from mass production to technology and knowledge based production. Globalisation and technological change will continue to shape economies in this century.

The shift to a technology-driven, knowledge based system of production and distribution leads to a growing appreciation for science, since advanced technology is science driven. Technology used to be thought of as a given, while we now see technology more as an instrumental attribute that can be developed by investing knowledge and finance. The development of technologies of course has its own dynamics, mainly economic forces will determine the adoption of the technology, as well as by societal environmental and health needs.

Several analyses show that countries that invest more in science and technology do better economically (Nijkamp & Poot 2000, OECD 1996). That does not mean that the causality is clear, or that the volume of investment in science is the sole determining factor of economic success.

The role of knowledge in the modern network society is embedded in a setting of local, regional and global interactions, partly technical and partly social economic. This being part of a global network does not mean that the need for personal contacts and recognition among scientists will disappear, even if the information technology would enable them to this virtual cooperation. But doing research has, like doing other work, its social dimension and that will ask for an adequate environment.

The generation, acquisition and transfer of knowledge take place in a setting of, mainly international networks, for which adequate knowledge infrastructures are crucial. This means that Europe, and every country in Europe, has to get its act together. Europe needs a strategy aimed at ensuring that globalisation is advantageous not only to the nation as a whole, but to all the member states. That does not necessarily mean that the initiatives are concentrated in Brussels: there is a very essential role for organisations like the European Science Foundation (ESF). The framework programs in Brussels serve a legitimate purpose, but cannot replace the necessary involvement and responsibility of the scientific community.

Is science in Europe under pressure?

I do not think that in a financial sense science in Europe is under pressure. Its investment level may be less than the benchmark % of GDP in the United States and Asia, but that is not to say that the money situation is bad. The % of GDP is an arbitrary measure; as the GDP rises it is not self evident that relative to that the public expenditure for science has to grow. The public budgets may have become relatively less then in the past, but on the other hand enterprises have invested much more. Never before have we had so many scientists, publications and patents. Commercial exploitation of technology is abundant, and at the same time the awareness in politics, government and corporations is growing that basic, fundamental science is the backbone not only for innovation and the economy, but also for present and future problems regarding health care, the environment and security and for social problems in general.

The European Commission recently expressed its concern for fundamental research: 'Europe would be quite wrong to reduce its investment in this area.' (Commission of the European Communities 2000).

Most OECD countries spend more and more resources on the production of knowledge; the yearly growth is 2.8 %, just somewhat above the growth of GDP (OECD, Science Scoreboard, 1999).

So notwithstanding the many complaints and national pleas for bigger budgets for R&D, I do not consider lack of money the main problem for science. If science is under pressure, this is because of several other more structural reasons. Looking to the future I have selected four challenges that face science and technology in Europe: global trends, quality of human resources, scientific and technological infrastructure, and confidence in science.

Four challenges face Science and Technology in Europe

Global trends reshaping the economy and society
The S&T community has to explore the challenges confronting Europe as it experiences contemporary trends such as the explosive growth in electronic flow of capital and information, the increase of international trade, and the recognition of the need for environmentally sustainable development. The gradual transition to a knowledge-based economy, supported by a public knowledge infrastructure, is the consequence of the 'information revolution'. In a knowledge-based economy the contribution of the national income in production, transfer, application and sale of applicable knowledge will be growing substantially. The rise of services, education, entertainment and R&D is a clear sign of restructuring the pattern of activities in our society. Governments come to recognise the strategic significance of a good infrastructure for S&T, with extensive international co-operation. Science can only prosper in open networks; accessibility is crucial.

As governments come to recognize the strategic significance of a good knowledge infrastructure, with extensive international cooperation. Science can only prosper in open networks.

The global use of immaterial networks will probably introduce 'international virtual nomads'. Not only science and technology become footloose but knowledge workers as well. It would be interesting to try to develop a 'science balance sheet' in national statistics for international comparative use, that is dynamic and less technology oriented, and that could possibly lead to new insights in the benefits of a good infrastructure for science and technology.

The fast depreciation of knowledge makes it necessary to continue and intensify the effort to stay at the front of scientific progress. Human capital is the everlasting source for new ideas, not only for economic welfare but also for quality of life that nurtures cultural, social and scientific needs. To foster this there must be a fertile substrate in all countries and regions.

Europe has many advantages that can offer new possibilities f or growth: its languages, literature and cultural diversity, rich history, artistic creativity and design can be seen as immaterial values that can be realized in a European version of the new economy. What is needed is a willingness to explore in what areas Europe can best proceed. Academic communities should be actively involved in this process. It would be useful to develop a regular state of the art in S&T in European countries that analyses strengths and weaknesses and promising prospects. Foresight studies as known in several European countries could be conducted on a wider scale (Foresight Steering Committee 1996).

Quality of human resources

The body of scientifically competent people, though nowadays huge by historical standards, is nonetheless a tiny fraction of the population. A strong basis of scientific competence is needed in the first place in scientists as active researchers, but also in the wider range of people that uses science in professions, public policies and in industrial applications. The diminishing enrolments in the science and engineering at this moment are considered problematic, but in some countries there seems to be progress. Flows of graduates are influenced also by the population's age structure (OECD, 1999; Graverson, 1999). To get the rewards of the globalisation of science and international cooperation a country needs to have a strong position. That means investing in their human capital in education and in research.

Scientific and technological infrastructure

What kind of infrastructure is needed to cope with exponential growth of scientific knowledge and demand for technology? At the European level there is need for a European dimension for the S&T infrastructure, be it not at the expense of the national effort.

The quality of the infrastructure will largely determine the economic perspectives (Nijkamp, 2000). The rapid technological advances are among other factors the result of a broad array of R&D investments. To profit from the accumulated knowledge a welleducated population is needed, with a lot of human capital to absorb and deal with this knowledge. It is not sufficient just to buy knowledge, or to import knowledge intensive services and goods. Institutions that develop the knowledge and skills for people living in the region need to participate productively in the »new economy« as well as in society.

Economies of scale and division of labour will influence the European academic landscape. Universities will be competing for students and for scholars by offering attractive working conditions. Attention is needed for the roles to be played by the public and private sectors in creating and maintaining a cutting-edge knowledge infrastructure. There need to be alliances between universities, research organisations and enterprise, national and multinational. International cooperation is essential, not only among European countries

Economies of scale in research-intensive activities favour an international division of labour. In this respect the roles to be played by the public and private sectors in creating and maintaining a cutting-edge knowledge infrastructure will be essential.

Confidence in science

Research and technology have a major impact for society. This entitles the population to be informed properly about the results and their possible implications. One can think of issues of health, and developments in the life sciences, information technology and privacy, or security issues and risk. Applications of science and technology can be controversial and raise ethical questions. At the same time we need science and technology to solve the many problems societies encounter and to ascertain sustainable growth. Communication between the scientific community, government and citizens is essential. The media, old and new, are a crucial factor in this process, but the main responsibility lies with the scientists.

Knowledge is acquired on the basis of education and research. Knowledge is a great social and cultural asset with a value that cannot easily be overestimated. The value of knowledge may be difficult to measure, but certainly is there. Knowledge enriches in several ways, even if we do not know how. Like with many other cultural assets, the possession of knowledge produces a strong urge to know more. In this way science is self-reinforcing.

Let us hope with Giddens (2000) for global integration that enables us to solve the dilemma between the instrumental economic aspects of science and its potential for social and cultural enrichment by reconciling all aspects in good measure.

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Flipping the coin: The necessary modesty of science

- a rhapsodic reply

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In search of a reflective science

The Science under Pressure? Conference left me with a bunch of puzzling questions. Though familiar with Social Studies of Science (STS), Sociology of Scientific Knowledge (SSK) and in general Sociology of Science and Sociology of Knowledge, I'm not a scholar in regard to these issues, which in the wake of Mannheim, Kuhn, and Merton - and especially after the Sokal affair - have attracted some attention in, not least, Anglo-American, French and German academic institutions, but to my knowledge only very little in Denmark. After a very brief - and doubtless unfair resumé of papers given by some of the speakers - I will draw partly on mainstream sociology and partly on the modern heir of the French *epistemologié* (Bachelard, Canguilhem, Althusser), Michel Foucault. I will hereby seek to justify some sociological and philosophical insights that I found missing in the conference discussions in regard to the conditions, limits and consequences of science. My basic question is: do we dare to - not just - know *(sapere aude),* but do we dare to reflect on and analyse what it means to know, systematise and institutionalise truths in terms of supporting power, creating pollution, and fostering the progress of humanity and its doubles? In other words: let's flip the coin.

The aim is not to cancel or debunk some of the aspects and viewpoints listed below, nor to claim any kind of elevated truth, but rather to demonstrate an aspect of science studies that could enlighten science - and here I'm hinting mostly at the social sciences - with a dose of modesty. Despite the great variety regarding viewpoints, contents, issues, etc., the majority of the papers given at the conference seemed to share a basic trust in science which apparently did not allow for more fundamental and provocative moves. The conference appeared to interrogate a plethora of topics relating to science, truth, institutions, education, politics, economy and environmental issues. Fuller attempted to (re)establish the university as a republican institution educating citizens,[2]

1 Stefan Hermann is a lecturer in Sociology and has published articles about modern and classic sociology.

2 Here, Fuller seems to lean towards a democratised notion of Humboldt's idea of a 'Bildung durch Wissenschaft'.

criticising big science tendencies, trends of commercialisation and the traditional specialisation and *isolation* of academic disciplines, and in the same manoeuvre to rehabilitate science studies against the 'Kuhnian plot' (Fuller, this volume, Fuller 1999a, 1999b). Not surprisingly, Fuller's view of modern science is not appreciated by Helge Kragh (this volume), who angles science in a more traditional way and doesn't share Fuller's refusal to govern science as one governs business and, by discrediting Fuller's notion of a citizen science, throws the baby out with the bathwater in regard to valuable insights from SSK and STS. I will return to this issue. The biologist Emmeche (this volume) seems to apply some sociological insights in his brief problematisation of science as a job/vocation by considering social and institutionalised rules and norms which play significant roles with regard to publications and carrier possibilities, and points to the fact that funding and interest in certain scientific subdisciplines are led by political atmosphere and ephemeral conjectures. Emmeche is not motivated or driven by the same belief in science as the master solution to global environmental and economic problems as is Thomas Whiston in his account of global problems and political, practical and institutional obstacles to scientific solutions. Leydesdorff and Etzkowitz propose a model (triple helix) or theory of the complicated communicative interplay between science-industry-government in a non-hierarchically structured society. Hereby they try to locate science in society broadly speaking, and attempt to model current transitions in research systems. I'm not a triple helix expert, but it looks like they are getting into bed with Niklas Luhmann and Manuel Castells theoretically and in diagnostic matters. The paper delivered by Diana Wolff-Albers makes knowledge accumulation, education, and investment in human capital central axes in global competition. The more science, the better. Saving Roepstorff's anthropological analysis and investigation, I'd like to proceed and introduce what is to be my corrective issue.

Between truth and truth rituals

I regard science as a social and cultural practice, but nonetheless a very remarkable one (cf. Pickering 1992, Luhmann 1984). Science is shaped, constrained and enabled by institutional frames, culturally given values (Weber 1991), interactional norms, procedures, methods, tools of measurements, etc. which, compared to day-to-day life, have an albeit specific character due to the truth-seeking ambition. I tend to view these conditions as fundamental even though they are likely to change, in other words one could say they are historically founded. It seems beyond doubt that science can be studied from different and often incompatible perspectives. Since Enlightenment,[3] Science

3 One remembers Auguste Comte's famous dictum: 'Savoir pour prevoir, prevoir pour pouvoir'.

has had tremendous ambitions regarding its capability, prospects and legitimation to alter and reform nature (natural sciences), societies (social sciences) and individuals (psychology, pedagogy, medicine) regarding its advice or subsequent practical instrumental technologies. Not only have institutionalised truth and knowledge generation been coupled with a societal and political will to act, intervene, and change, informed by and in the name of science. Truth means a lot, we believe in truth, which does not mean that one neglects that something might be more true than other things. In many respects, Roepstorff's anthropological paper reminds us of the cultural and social patterns shaping academic institutions and mandating their agents with credibility, trustworthiness and authority. In Latour's memorable phrase, it works as a black box. This anthropological or sociological account does not debunk or in any way contradict or attack the soberness of scientific practices, it just highlights aspects of what happens tacitly or explicitly in laboratories or scientific communities; therefore it doesn't lead to relativism, still less nihilism.

But the sheer fact that some are enabled, licensed and equipped to attach truth to their propositions, descriptions, investigations, etc., has of course meant that one thing is to know and thereby become capable of acting in a directed manner, another to have the credibility to claim truth, thereby legitimising potential actions. This distinction between truth and truth revelation has, in recent years, in the context of social science, been re-actualised due to the booming industries of consulting, think tanks and commercially based research agencies. These agencies - in the social sciences - often work on market-based, competitive conditions, delivering amazingly simplistic descriptions of the social universe (e.g. Jensen 1999), which are consumed by businesses, administrations, politicians, and the public and generally applauded due to their seducing simplicity and the fact that they are produced by institutions claiming to be research institutes with an array of entitlements, norms and rules similar to 'independent' scientific research centres (smells like science). Viewed in this perspective, genuinely social science is, in my opinion, under pressure. This is not solved by suspending complexity, but rather by insisting on it - which means to define the discipline's *differentia specifica* vis-à-vis the oracles and 'siren songs' from market-based think tanks and research agencies. This is doubtless not a discovery. However, the point is only possible to grasp if one realises and manages the overwhelming need for truth *claims* (demanded and utilised in political and social life) and distinguishes it from and investigates its bonds to specific kinds of knowledge *production.*

Effects of science

This delicate and, in reality, often very confused line between truth rituals/-claims and the role of truth has to be put in perspective in regard to the effects of science. Allow me here to reproduce and apply a few insights from modern mainstream sociology and from Michel Foucault and his heirs (Donzelot 1979; Ewald 1986; Rose 1996, 1999a, 1999b; Schmidt/Kristensen 1986). In my view, the conference was dominated - apart from Roepstorff's and Fuller's paper - by a well-meant and sincere belief in science, which only took very little account of the fundamental involvement of science and technology in society. Science was seen as the solution and not as the problem. I do not think this is an either/or problematique, but a rather a complex question, a both/and.

Following Popper (who said that knowledge is built on flying sands), Anthony Giddens in his diagnoses of modernity (Giddens 1990, 1991, 1992, 1994, 2000; Giddens/Pierson 1998) suggests that modern societies have not realised the enlightenment ambition to create - qua science - a more predictable, safe and properly ordered world. Quite the opposite. Knowledge and applied knowledge have contributed to an immense institutional reflexivity that runs parallel to a more individual social-psychologically founded reflexivity which more than ever realises itself in high modernity. Knowledge creates uncertainty, flux, and undermines knowledge. Following Ulrich Beck (1992/1986), we could mention Tjernobyl, BSE, genetically modified food, the nuclear bomb, toxic pollution, greenhouse effect, etc., which are not per se caused by knowledge, but wouldn't exist without the systematic production and circulation of knowledge which eventually results in some kind of application. Supposedly value-neutral results have - when realised in the social and natural world - consequences which are unequally distributed and value some values in preference to others, to use a Nietzschean phrase. This should not lead to any kind of condemnation of science, but rather implant a touch of modesty before we celebrate science as the road to salvation.

Twisting this aspect, one can be led to the diagnostic philosophy of Michel Foucault,[4] which is more or less centred around the questions and interrogations in power, knowledge, and subjectivity and their mutual relations. Foucault never took an analytical philosophical standpoint, nor did he ever contest or ask to the truth of different hegemonic knowledge regimes *(regimes du savoir)* in the human sciences, but always problematised their effects and exclusion of other kinds of knowledge. In other words, he investigated know-

4 See Kristensen 1987; Lemke 1997; Dean 1999; Smart 1985; Hermann 2000 for an overview or essential articles and interviews like Foucault 1978, 1980, 1982.

ledge in practice - although not in the sociological way Latour and Woolgar did in *Laboratory Life* (1979/1986) - and found that a wide range of knowledge regimes had institutions and exercise of power as their foundation, and that the work of institutions (schools, asylums, hospitals, workplaces, prisons, etc.) followed and directed their work on subjects according to scientific principles (pedagogy, medicine, psychiatry, economy, demography, statistics). Scientific principles and a prioris with great historical variance (cf. Foucault 1966). He wanted to point to the fact that modern society, since Enlightenment, had discovered liberty (given to the subjects since the 19th century), but also fostered and institutionalised mechanisms, relations, and human technologies aimed at managing this freedom in the name of welfare, social order, progress, enlightenment, health, etc. Hence knowledge and power do not necessary contradict each other. Power is not an enemy of truth. But rather no exercise of power without the constitution of a field of knowledge (Foucault 1975). No governing of national economy without economic science, no efficient control of labour without modern organisational theory and models, no rearing and education without pedagogy, no management of health, eating, smoking, physical exercise without knowledge claiming objectivity in these concerns etc.

My motif for drawing on these perhaps exotic perspectives is not to downgrade science or opt for some kind of outdated theory of conspiracy - but rather to open, problematise, and highlight what I consider to be basic conditions in modern society. Conditions which do not necessarily follow some kind of historical necessity. We don't have to become Foucauldians to recognise this, one doesn't even need Nietzsche as helmsman for this appeal to modesty (Nietzsche 1974), but we can envisage Immanuel Kant - and his many followers in Critical Theory as well (e.g. Habermas). In his *Critiques,* Kant stated that philosophy's role was to prevent reason from going beyond the limits of what is given in experience. In others words to analyse limits and conditions of different kinds of reason (pure, practical, medical, political, economic, etc.). Respecting this legacy doesn't appear to be an inappropriate point of departure on the way to a reflective human and natural science.

Re-entering 'Science under Pressure?'

Addressing this very fundamental problem twists our attention away from nitty-gritty or pedantic - but in many ways not irrelevant - analyses of whether the triple helix is a betrayal of the freedom of true science or if universities should foster citizenship or specialisation, towards fundamental and very present questions on the worldly phenomenon called science, its conditions, effects and - at least in the social and human sciences - very conjectural truths. Truths which - through carriers and translations -intervene in the human world and create differences in the distribution of wealth, values, possibilities, problems, etc., and temporarily handle all kinds of issues.

The central question to me is therefore whether we have the courage to flip the coin, to present problems and results in a way that contributes to the transparency of science, its consequences, conditions and basic mechanisms. Albeit not in the usual sense of the term political, but a bit more polemic, I think we need a politics of truth, because truth does not present itself - it has to represented and produced. Of course, we'll open a Pandora's Box, sometimes turning out to be a can of worms and in other cases a treasure chest. This recognition - at the same time a reconciliation with the other side of the coin - is the pressure science needs.

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