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Probing the Dynamics of Scientific Knowledge

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To Peter Weingart
and, of course,
Henry Holorenshaw

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Science Studies
Probing the Dynamics of Scientific Knowledge

Sabine Maasen and Matthias Winterhager

"The sociology of science, once marginal, has become a growth industry practiced by an increasing number of scholars … It often comes as the nucleus of the so-called STS (science, technology, and society) programs and centers" (Bunge 1991a: 524). While this observation is shared in principle by a growing number of colleagues, both science studies and sociology of science, in particular, only rarely have been introduced in a systematic and easily accessible fashion.[1] Maybe due to its enormous success, the field of science studies rather commits itself to studying the various phenomena accompanying societies that differentiate specific systems, institutions, and practices to produce systematic knowledge rather than to self-reflection, or even less, to introducing itself. At the same time, however, due to its success, science studies has become a field that cannot be described but as heterogeneous. From the 1960s onward, and with a special thrust in the 1980s, science studies conquered novel territories (e.g., science policy, PUS, cultural studies), has employed various methodologies (e.g., discourse analysis, ethnomethodology, bibliometrics) and theories (e.g., network-theories), inquired into epistemological questions (e.g., reflexivity in sociology of knowledge) as well as into the instruments of research (e.g., the experiment) and – last but not least – has become institutionalized in wide range of departments and programs.[2] The expert in the field may take this lightly: "Although science studies cannot 'control' its subject matter, it can pick its methodologies and research questions very broadly and yet remain a recognizable field" (Bagioli 1999: xiv). The novice to the field, however, may shrink back from the double trend toward disunity' (Bagioli): As sience studies progresses, it further disunifies itself and the picture of science it studies. Hence the urgency and difficulty of finding a path through this jungle.[3]

Finding a path, however, cannot possibly mean 'unification' of science studies (cf. Galison 1996) but rather attempts to give an idea of what this interdisciplinary field, predominantly populated by sociologists, historians and philosophers of science[4], is all about. In this vein, one can basically choose between two options: Either try and write a comprehensive overview, necessarily sketchy if it comes to the details of each single approach. In this way one gets a map of science studies designed for preliminary orientiation in the field. Alternatively, one can select one overarching concern and probe more deeply into its various aspects, thereby learning about science studies *per exemplum*.

Basically, this book has been set up according to the latter option: We chose one overarching concern, namely the dynamics of scientific knowledge, and present nine self-contained studies that touch upon this concern in highly different ways: They encompass the whole range of scientific cultures: the natural and social sciences as well as the humanities; they cover issues as diverse as climate research (Aant Elzinga), historiography (Wolfgang Prinz), and methods such as bibliometrics (Anthony van Raan) as well as the role of metaphor in science (James J. Bono). Their heterogeneity notwithstanding, the studies presented all inquire into, or are themselves examples of, the dynamics of scientific knowledge. Moreover, the types of dynamics analyzed or exhibited not only result from intrascientific but from extrascientific processes as well: As to the former, Diane Paul, for instance, explores the use of biostatistical concepts in tracing the history of eugenics, hence, inquires into the heuristic value of analytic tools that have been developed in another discipline. In a similar manner, Peter J. Richerson and Rob Boyd advance the analysis of human culture by analogizing cultural to biological evolution. Other scholars investigate the dynamics resulting from extrascientific exchange: Nico Stehr ponders the need for a so-called 'knowledge politics' and Bruce Lewenstein critically discusses the emergence of what has become known as 'public understanding of science.' Last but not least, Wilhelm Krull testifies to the way in which science policy makes use of those (online-)observations of the dynamics of scientific knowledge production that science studies provide. Summarizing, although each article represents a self-contained study, the collection as a whole sheds light on one of the most intriguing phenomenon in the field of science studies today: the dynamics of scientific knowledge.

However, we will not leave the readers all alone in their attempt to make their way through the individual contributions. Instead, we will first introduce the overarching issue, that is, why 'dynamics of scientific knowledge'?, and embed this question in the broader context of science studies (cf. "Science Studies – Dynamics of a Field"). Second, each single contribution will be characterized as regards its connection to the overarching issue (cf. "Nine Studies in Science"). Third, throughout the book, each article will be preceded by a little vignette integrating each study into its broader context of research. Thus, in the end, we try and save a little of the former option of writing this book and do both: give a sketchy map of science studies *and* probe more deeply into some of its territories.

Why 'Dynamics of Scientific Knowledge'?

Knowledge in a Knowledge Society

Knowledge has become a major concern in many of today's societies. Consequently, this concern is no longer confined to those who produce it but it is also the daily business for those who organize, communicate, regulate, and use it. In fact, the increasing significance of knowledge has already led to a new label: 'knowledge society,' indicating that knowledge and society are mutually constitutive for each other. What are the defining characteristics?

- Knowlege is seen as a central, if not primary resource for societal reproduction, thus neighbouring, if not prioritizing money and power, the two other key resources driving the engine called society.
- In particular, this is indicated by the increase of knowledge-based professions that currently spread into ever-more parts of contemporary societies. On the collective level, expertises abound and compete. On the individual level, this translates into acquiring a portfolio of expertises throughout one's career.
- Developments such as the ones mentioned above are said to be caused by several processes: e.g., by the scientification of knowledge, the globalization of data and information networks, as well as by the growing perception of risk and contingency which significantly increases both the supply of and the demand for knowledge.
- This not only leads to new ways of dealing with the constant flux of knowledge (i.e., 'lifelong learning'), it also directs critical reflection to *the* source of data, information, and knowledge: science. Deeply entrenched with other subsystems of society (e.g., economics, politics, law) it co-produces both benefits and risks for individual and collective actors.

Hence, no wonder that science attracts new attention: Society wants to know more about science as a specialized subsystem, designed to produce 'true' knowledge, in particular, about its cognitve and organizational specificities, as well as about its relation to other subsystems within society and 'the public,' in general. In short: It wants to know more about the dynamics of science responding to the dynamics of society, presumably turning the latter into a 'knowledge society.'

Presently, 'knowledge society' is not a well-defined term but rather entails different messages for different audiences, political and scientific ones, in particular. One asymmetry is most telling: Namely, while the label knowledge society abounds in science policy programs and the media, it is far less often to be found in the academic discourse: The Science Citation Index notes only 14 entries for the last 8 years. Without carrying the interpretation too far, this observation may safely be said to imply two messages: First, apparently science has yet to acknowledge, and to participate in, the general societal discourse on the role of (scientific) knowledge. Second, if it comes to defining the role of (scientific) knowledge in society, science is neither the leading nor the primary voice. Thus, in a nutshell this observation tells us what is at stake: A new role for knowledge in society implies to look anew at science as the most prominent institution that produces it systematically.

Those who face up to this challenge respond – broadly speaking – in two ways, either directly or indirectly. The direct response is given by those authors who attempt to theorize the 'postmodern' condition of science on a general level; the indirect response is given by the plethora of scholars who study science in its various appearances and from various perspectives: its semantics, institutions, methods, instruments, histories, social practices, techniques … While this book is about a variety of seminal approaches in this field of science studies that need and should be read in their own right, this introduction would like to render those research strategies intelligible by way of addressing their background, the (alleged) 'postmodern condition of science,' first. To this end, we will touch upon some major theories regarding the role of science today. Their common concern: Do we live in a Knowledge and/or in a Science Society? Next, we will address the epistemological issue implied in this question: Do the dynamics in both science and society really endorse a postmodernist stance? Third, we will give a historical account of science studies dealing with knowledge dynamics. Only thereafter will we return to the studies convened in this book and their, if indirect, responses to the role of science in society today. Generally speaking, they all conceive of science as part and parcel of society and their dynamics, interactively produced. They all look for instruments that are appropriate for analysis of, or intervention into, various instances of this dynamics. This is what unites the studies in this collection and also indicates one of the concerns of science studies, in general.

Knowledge and/or Science Society?

In 1994, Michael Gibbons and colleagues challenged the received view of science and society by postulating that a new mode of producing knowledge is about to emerge. Whereas the traditional mode relies on producing science in academic places, according to disciplinary schemes, and only thereafter applying the knowlege thus produced to the extra-academic field, the new mode operates differently, sooner or later integrating the convential mode: Most of all, the new production of knowledge, called 'mode 2,' transcends academic circles by proceeding in a multi-, if not transdisdisciplinary fashion. Instigated more by general problems than by disciplinary questions, the context of application is the decisive frame of reference. The type of communication between the parties is characterized by consulting and negotiation and the organizational setting is flexible and transient. Accordingly, the level of institutionalization is low.

This proposal has met with enthusiasm and criticism alike. While politicians and research agencies readily accepted this new picture and put it on their agenda ('mode 2' since then figures most prominently in documents on science and funding policies), the reactions of (social) scientists were mixed.[5] Notably Gernot Böhme and Nico Stehr (1986) as well as Peter Weingart (2001) explicitly reject the view according to which science is about to lose its significance for the societal production of (true) knowledge. Above all, they counter with an increasing trend toward scientification of ever-more spheres of life. In this process, thus Böhme and Stehr, science is the decisive productive force, not knowledge, in general. Still, they refuse to talk about 'science society' and rather stick to the term 'knowledge society':

The focus is not merely science but the relationship between scientific knowledge and everyday knowledge, declarative and procedural knowledge,

knowledge and non-knowledge. It is only after one acquires a sense of the societal significance of such opposites and oppositions the full sociological significance of knowledge begins to emerge. Such a perspective ensures that one realizes the extent to which knowledge can form the basis of authority; that access to knowledge becomes a major societal resource and the occasion for political and social struggles (Böhme / Stehr 1986: 8).

In short: While the radical view as expressed by Gibbons et al. or Helmut Willke (1998, 1999) considers science as one among many different and equally important sources of knowledge, the moderate view as expressed by Stehr and Böhme, regards science as the dominant source of knowledge to which other forms of knowledge relate, if by way of adapting it to their local requirements. Being knowledge-based, does have equivocal effects on society, however: In a knowledge society, both the scope and contingencies of actions increase simultaneously. For instance, Stehr notes that the breadth of expertise and the society's penetration with reflexive knowledge leads to both, more data gathering and surveillence *and* to new possibilities for escape. Likewise, globalization prompts worldwide networks of knowledge *and* locally specific transformations (cf. Stehr 1994). Not surprisingly, paradoxes such as those dynamize the evolution of a knowledge society: Better knowledge and different technologies are produced to cope with unintended effects of science and technology.

'Post'- or Modern?

Unveiling the dynamics of scientific knowledge in a knowledge society is thus based on a constructivist epistemology: It focuses on the making and remaking of (scientific) knowledge(s) in societies. This stance may easily be regarded as 'postmodern,' implying that, ultimately, (scientific) knowledge is beyond rational analysis. In our reading, however, the observation that the production of knowledge (including scientific knowledge) is not only dynamic, but increasingly so, results from interrelated processes in science and society: These can be rationally reconstructed while, at the same time, acknowledging contingency and the growing significance of extrascientific knowledges.

Interestingly enough, the dynamics of scientific knowledge is inherent in modernist and postmodernist accounts. From a modernist stance, the dynamics of scientific knowledge was but a matter of temporary imperfectness. Ultimately, thus the hope, imperfect knowledge would, by specifying the unknown, prompt specified questions, thereby leading to perfect knowledge. Most importantly, this goal, though always imaginary, safeguarded the production of new knowledge against discredit: Eventually there would be no more unruly dynamics but stable knowledge, that is, truth.

This very promise of modern science of stabilizing knowledge, however, became subject to severe doubts. Accompanying scientific practice from its inception, these doubts have been raised in two variants (cf. Bauman 1992: 290ff.): Variant One, the modernist critique, holds that newly gained knowledge does not make sense within the realm of existing knowledge, thus is in need of novel explanations and/or theories. Variant Two, the postmodernist critique, states that newly gained knowledge is but one among others, maybe not even the best one, thus is in a steady state of competition and local adaptations. While Variant One legitimizes the ongoing production (and, hence, the dynamics) of knowledge in the name of truth, Variant Two undermines the trustworthiness of scientific knowledge itself: Scientific knowledge may not provide the certainty needed to stabilize knowledge once and for all. Ironically for some, both variants thus enforce the production of better, more competitive, more trustworthy knowledge.

In Bauman's analysis, today both types of doubts[6] amalgamize: Based on the conviction that there is no such thing as certainty anymore, scientists of any epistemological creed persist in producing more knowledges in an effort to counter contingency with pluralism. The search for truth as well as the search for equally plausible or locally more plausible stories keeps the engine going. Put in a nutshell, the dynamics of knowledge seems to be the most stable trait of the practice called science, modern or postmodern.[7]

If anything, the differing diagnoses of the role of science in society show that currently things are in a state of transition and the same holds for science studies. Science studies is a dynamic field in the midst of dynamic societies: Its epistemologies, its research agendas and methods, its style of communication and transfer functions are part of the overarching systematic reflection that modern societies entertain in order to cope with the (unintended) consequences of their modernization. By conceiving of the sciences as epistemic-institutional ensembles and historically changing cultural practices, the field provides a theoretical and empiral basis for comprehending the dynamics of scientific and technological developments as well as the mutual interpenetration of science, technology, and society (cf. Nowotny 1998: 9f.).

As to the question what STS (Science and Technology Studies) is good for, we follow David J. Hess in that it is always a wise thing to appreciate the history of science and technology, in general, but today, this is not enough: In addition, science studies serves an increasing demand for orienting and legitimizing science politics and science management. Moreover, science studies provides a forum at which the growing public concern with science, technology, and social values can be articulated. Be it the issue of institutional dynamics of science or the general place of science and technology in society,

> the future of STS lies in its ability to provide a site for public debates on issues of social importance, and for the evaluation of major research programs and technological decisions (Hess 1997: 155f.).

Science Studies: Dynamics of a Field

Thus, the task of observing knowledge today seems to have assumed a new quality. Various schools of thought, most prominently sociology of knowledge, history of ideas, and the interdisciplinary field called science studies has argued from its inception that knowledge and the social conditions, in which it occurs, are not independent of one another but deeply influence each other. Although scholars and schools differ as to the question in which way or on what level this mutual influence occurs, they nowadays all agree upon one basic, anti-positivist insight: The interrelation of knowledge and society is no sign of impurity or falsity in need of remedy. Rather, knowledge comes in socio-historical and situational packages that need to be analyzed in full. What is true for knowledge, in general, is true for scientific knowledge as well. Moreover, throughout about seven decades scholars in the realm of science studies attempted to cope with an increasingly dynamic interrelation of science and society: Not surprisingly, epistemologies, approaches, and objects of study vary enourmously. Put sketchily, one might recount the history of science studies as follows:

The Beginnings

Early on, scholars became interested in the relation between science and technology on the one hand, and the (capitalist) state on the other. St. Simon, Marx and Engels, Weber, although no scholars of science proper, were interested in the ways in which social structures and ideas, values and beliefs influenced each other. Weber's *Protestantische Ethik und der Geist des Kapitalismus*, for instance, had a considerable impact on the early sociology of knowledge, launched by Max Scheler and Karl Mannheim in the 1920s. Their guiding notion was that knowledge and society *are* related. Center stage was not science, however, but ideologies and other forms of political knowledge.

Indeed, for a long time, science was thought to be excluded from being infected by the social. If anything, scientific errors could be attributed to the social (cf. Lakatos 1971 and Laudan 1977): The early sociologists of knowledge in Germany had deliberately exempted scientific knowledge from their project. Elsewhere, however, scholars began to explore the relation of science and society. While pursuing different projects, they shared the assumption that the production and acceptance of knowledge cannot be understood as resulting from internal processes (alone). Other than positivists would have it, scientific knowledge was seen as to depend on external, i.e., social processes as well (or even entirely). In 1931, Boris M. Hessen, for instance, held that Newton's work was a child of his class and time and that his work was an attempt to solve technological problems posed by the rise of capitalism (Hessen 1931). Hessen's work shaped the Western Marxist sociology of science between the 1930s and 1960s, inspiring, among others, John Desmond Bernal.

Bernal's school was mainly interested in science policy and focused on the social conditions or scientific research as well as the uses and misuses of science.[8] By contrast, Michael Polanyi's epistemology emphasized the importance of practical skills and nonverbal communication, that is, the 'tacit knowledge' that conditions scientific work (Polanyi 1958). By implication, no one could understand how best to promote science who was not a scientist herself. On Polanyi's account, there was no alternative to freedom of scientific inquiry and adminstrative control of scientific resources by a scientific elite. The Bernal / Polanyi debate fundamentally was about how best to organize, support and direct science in a democratic political culture: Humanist versus elitist, political management versus self-regulation characterize the political implications of the opposing stances, affecting science studies until today, albeit ambivalently (cf. Rouse 1992: 5f.).

The Institutionalist View

If indirectly, Marxist notions and Durkheimian notions had inspired Robert K. Merton to study science systematically, thereby establishing what nowadays – after several modifications – is known as science studies (cf. notably Merton 1945). According to his institutionalist view, social factors indeed play a decisive role in shaping the products of science. In particular, he stressed the role of scientific ethos, which comprises four 'institutional imperatives': universalism, communism, disinterestedness, and organized skepticism (cf. Zuckerman 1988, Felt / Nowotny / Taschwer 1995: chapter 3). Following Mannheim in this respect, these norms safeguarded the autonomy of science and the objectivity of scientific knowledge, thereby securing science as an institution against corruption through social, political and economic interests. In this structural functional analysis, sociology of science is about investigating the institutional framework both allowing and conditioning the emergence of science as an autonomous cognitive system: "Specific discoveries and inventions belong to the internal history of science and are largely independent of factors other than purely scientific" (Merton 1970: 75). Methodically, the work was based on discourse analyses of scientific documents.[9]

Until today, the institutionalist perspective has two main objectives: It focuses on the internal structure of science and on its relation to other societal subsystems. Pertinent questions are, for instance, what are the norms guiding scientific activities? How did the disciplinary structure of science emerge? What are the interdependences between science and other societal subsystems, such as politics, economy and the media? Further issues are funding policies, knowledge transfer, evaluation, and so on. The institutionalist branch does not, however, investigate scientific knowledge as such (with the notable exception of bibliometric analyses, cf. below).

As regards the dynamics of science, Merton's approach offers two kinds of explanation (cf. Hornbostel 1997: 89f.): According to the first one, in scientific fields characterized by highly accepted goals, theories and methods, social factors, notably the social organization of research, modulate scientific production of knowledge by way of promoting or hindering scientific progress. As cognitive and social criteria correspond, the dynamics of science results but from empirically calibrating these norms that tend to be conflictive: On this view, science proceeds selectively, yet cumulatively. According to the second, if not strongly developed, kind of explanation, cognitive critera of producing and evaluating knowledge may vary. In less stable fields of research, that is, social factors may influence scientific contents and procedures, too. Thus, the dynamics of science proceeds on the cognitive level as well. Institutionalized norms no longer (fully) correspond cognitive criteria for reward, but the latter most likely become an autonomous resource of competition for reputation and power.[10]

In the words of Joseph Ben-David, summarizing the situation of institutionalism in the late 1960s, this brand of science studies is about the "... institutional study of scientific activity (as distinct from the study of concepts and theories of science)" (Ben-David 1970: 429). Hence, while the sociology-of-knowledge paradigm will soon focus on the contents of theories in science, the institutional paradigm inquires into the emergence and development of science as an institution: issues such as size, growth or stagnation, innovation, choice of topic become subject to comparative analysis; differences between premodern and modern science; between different stages of a national research system as well as between various national research systems. Basically, these studies either look for universal and constitutive traits of modern research (besides Merton, cf. also Luhmann 1990), such as *curiositas*, scientific ethos, reputation, or they look for specific institutional steering mechanisms, such as funding and organization of research. Both types of studies rest on a role theory of social action: Institutions shape the activities of goal oriented actors. To its critics, institutionalism testifies to what Whitley has termed a "black boxism" (Whitley 1972) that ignores controversies and discontinuities in science in favor of considering it a homogeneous cognitive system.[11]

The Kuhnian Challenge

This view was severely challenged by Thomas S. Kuhn whose work on "The Structure of Scientific Revolutions" (1962) regarded the development of science as a succession of competing paradigms.

Knowledge was seen as the outcome of paradigm-bound science which was itself identified by the existence of strongly bounded social structures with powerful mechanisms of cognitive and social control. While the Mertonian sociology of science has linked knowledge production to general and rather diffuse norms, the post Kuhnian sociology of knowledge sought coherent and strongly bounded communities as the major, if not only, locus of knowledge production (Whitley 1984: 684).

As regards the development of science, there are several interrelated claims that constitute Kuhn's approach:

– Science proceeds as 'puzzle-solving,' guided by a paradigm that is shared by the respective community of scholars. This state of a discipline Kuhn called 'normal science.'
– As research proceeds anomalies may appear, i.e., discoveries which contradict the ruling paradigm. The normal puzzle-solving activity breaks down and the paradigm runs into crisis.
– Eventually the community of scholars responds to crisis by looking for an alternative paradigm and then to move on to the new one. This occurs as a 'Gestalt-switch' and constitutes a revolution because it cannot be "settled by logic" (Kuhn 1962: 93). It entails a change of world views, and thus a process of conversion as the paradigms involved are incommensurable (cf. Kuhn 1962: 147).

If, with a considerable time lag and equally considerable range of interpretations (cf. Weingart 1986 and Maasen / Weingart 2000, chapter 4), the notion of paradigm, and notably the notion of paradigm shifts, ultimately initiated an anti-positivist turn in various (social) sciences, sociology of science included (cf. also Heintz 1993). Here, Kuhn's considerations became not only accepted by those who sought to capture the intellectual dynamics of science but especially by those who – in view of a rapidly growing scientific system, both governmental and industrial – plead for a paradigm shift in research planning as well: While the mainstream à la Merton had largely ignored the existence of 'science policy' and 'big science,' in particular, a renewed 'science of science' should and could provide the means to observe, organize, and steer science. The dynamics of science, according to this view, was in need of a rational way to plan research (van den Daele / Krohn / Weingart 1979). By stating that science was indeed open to social and politcal influences and subject to stages of maturity, Kuhn's theory was regarded as a promising step in this direction of research. In the 1970s, scholars began to inquire into the interdependence of social and cognitive factors in science, such as into the emergence of subfields (cf., e.g., Mullins 1972, Edge / Mulkay 1975). Other scholars analyzed the possibilities and limits of governmental intervention into research and the latter's orientation toward socio-political goals (Böhme / van den Daele / Krohn 1973, 1978).

Ironically, while Kuhn's theory had provided the starting point for this more externalist kind of investigations in sociology of science, the empirical studies that followed revealed severe limitations of this approach. Both lines of research mentioned above testify to this result. In the case of emerging subfields, Mulkay, for example, found scientific communities to be much more amorphous and tenuous than stated by Kuhn. Moreover, Kuhn had underestimated the impact of unexpected discoveries, lateral processes between neighbouring disciplines as well as the branching off of specialized fields of research. Analogously, Böhme, van den Daele, and Krohn could not confirm that the external influences increased throughout the maturation of a theory. On their finalization hypothesis, orientation toward external goals and responsiveness toward external control should have been minimal in pre-paradigmatic stages and maximal in post-paradigmatic stages. Rather, thus the authors, scientific communities turned out to be highly penetrable at all stages of theoretical development. Science policital intervention, in particular, regularly produces cognitive pluralization and institutional specialization rather than unification and stabilization. Hence, Kuhn's unitarian model of science and his monist principle of explaining the development of science qua paradigms (cf. Whitley 1974) could neither guide systematic comparison nor research planning (cf. Hohn 1998).

Quantification and Measurement of Science

From a methodological viewpoint, the majority of science studies in the past and still today can undoubtedly be characterized as qualitatively oriented research. However, there is also a long tradition of using quantitative data and methods in the field. Until the 1950s, there were only few papers on the measurement and quantification of science. At that time, Eugene Garfield first published in *Science* (Garfield 1955) his idea of a Science Citation Index, and, in fact, a few years later he started the production. Although originally developed as a new tool for searching the scientific journal literature, the SCI right from its beginning turned out to be valuable for quantitative studies of science as well. Derek John de Solla Price was the most prominent scholar who discovered the potential of the SCI as a unique source for empirical studies in history and sociology of science. In his book *Science since Babylon* he studied the growth curves in science with mathematical models, based on publication counts for very long time series (Price 1961). Together with the follow-up study *Little Science, Big Science* (Price 1963) his work became most influential and stimulated a growing number of papers on all aspects of quantitative measurement of science.

As Garfield consequentially improved his product through implementation of new technologies, the SCI became available as a database on tapes and during the 1970s even online. What had formerly been impossible with the print version of the SCI could now be realized by means of advanced computer retrieval systems: counting publications and citations on all levels of aggregation for journals, subfields, fields, disciplines, research groups, institutions, countries etc. Researchers from various disciplines began to use the SCI and other literature databases for publication and citation analyses; *bibliometrics* established as a new specialty. Francis Narin pioneered the application of bibliometric methods for evaluative purposes and the production of science indicators (Narin 1976). With his successful delivery of bibliometric measures for integration into the *Science Indicators* volumes of the US National Science Board he demonstrated that there is a real demand for the 'products' of quantitative studies of science. Bibliometrics showed up as an *applied science* – and, in fact, until today science policy and science administration agencies in many countries purchase bibliometric studies.

The boom in science indicators in the 1970s led to a conference "Toward a Metric of Science: The Advent of Science Indicators" in Stanford, where Yehuda Elkana, Robert Merton, Joshua Lederberg, Henry Small, Arnold Thackray, Harriet Zuckerman and others reviewed the state of the art (Elkana et al. 1978). Most of the critical arguments on the theoretical and methodological problems of indicator construction, which have been reported at this meeting are still up to date (cf. Glänzel 1996). Also in the 1970s, Henry Small developed a method for 'mapping' science by co-citation cluster analyses (Griffith et al. 1974, Small / Griffith 1974). With these cocitation maps it is possible to draw two-dimensional representations of the cognitive and social structures of specialities in science.[12]

It was during the 1980s and 1990s that the scientometrics community established itself in a more formal way with regular international conferences and an international society. The development was enforced by the fact that the 'customers' of bibliometric studies (science policy and administration) expressed a steadily growing demand on reliable indicators for all sorts of evaluation tasks.[13] More and more researchers have got direct access to the data sources via CD-ROM and the Internet *(Web of Science)*, and the number of bibliometric studies is continuously growing worldwide.

However, with so much demand for bibliometric indicators today there is also some danger for the field to get too much commercialized. A balance between basic and applied research is essential for the health of the field (van Raan 1997), but some of the bibliometric research teams may become too dependent on the 'business' of indicator production. Important theoretical and methodological problems are unsolved and we are still waiting for a clear answer to the question "*Which reality do we measure*?" (Weingart et al. 1990).

The Constructivist View

Throughout the 1980s – completely unaffected by quantitative approaches in science studies – the constructivist branch of the field started competing with and thereby striving to replace institutional accounts of science.

> There seems to be little point in focusing our analysis of cognitive norms on general rules dealing with logical consistency, verifiability or replication as if these notions can be taken as analytically unproblematic; for the meaning of such rules will be as varied as the specific contexts in which they can be seen to operate. This has become increasingly clear as a result of historical analysis and the growing number of sociological case studies (Mulkay 1980: 56).

Students of constructivism, summarily labeling their branch of science studies 'Sociology of Scientific Knowledge' (SSK), predominantly began to look at the epistemic dimension of science.[14] Other than Merton and more radical than Kuhn would have it, constructivist notions do not restrict the influence of social factors to issues such as theory choice. Instead, they state that the production of scientific knowledge is socially conditioned 'through and through.'[15] This relativistic position has become known as radical externalism: Context determines content, or even context *is* content. Reality does not deliver objective criteria that would allow us to judge a theory as true or wrong. Rather, scientific theories undergo a multi-stage process in which, by way of successive "deletion of modalities" (cf. Latour/Woolgar 1979; Fleck 1980: 101), situative observations are transformed into context-free statements, thereby stabilizating the theories underlying the latter. Based on Emile Durkheim, the classical sociology of knowledge (Karl Mannheim, Max Scheler), the work of Ludwik Fleck[16], informed by science theoretical positions expressed by the late Wittgenstein as well as by Imre Lakatos, Mary Hesse and Paul Feyerabend, but also oriented toward the Interpretative Sociology, notably Ethnomethodology, David Bloor was among the first to formulate a radical externalist programmatic called "strong program."[17] The central tenets are:

> 1 It would be causal, that is concerned with the conditions which bring about belief or states of knowlege … 2 It would be impartial with respect to truth and falsity, rationality, success or failure … 3 It would be symmetrical in its style of explanation. The same types of causes would explain, say, true and false beliefs … 4 It would be reflexive. In principle its patterns of explanation would have to be applicable to sociology itself (Bloor 1976: 7).

Leaving open the answer as to how exactly social and cognitve factors interfer, more precisely, what exactly is 'social' in science, two main strands of research emerged to fill this gap: One centers upon *science as knowledge*, the other one upon *science as practice* (cf. Heintz 1993).

At the beginning of contructivist reasoning in sociology of knowledge 'the social' equalled external factors conditoning science.[18] Scholars following the so-called *interest model* hold that

> opposed paradigms and hence opposed evaluations may be sustained, by divergent sets of instrumental interests usually related in turn to divergent social interests (Barnes/McKenzie 1979: 54).[19]

Authors such as H.M. Collins and Michael Mulkay, as well as scholars who belong to the tradition of Symbolic Interactionism and the Chicago School advance a *discourse model* of scientific knowledge, stressing that knowledge emerges in communication by way of negotiation. The final result can neither be reduced to the individual researcher nor to the problem at hand, but is a reality that has been interactively produced in controversial fashion.

Through contestation and modification, the meaning of scientific observations as well as of theoretical interpretations tends to get selectively constructed and reconstructed in scientific practice (Knorr-Cetina / Mulkay 1983: 11).[20]

The decisive turn for many approaches consisted in a shift of attention from science as knowledge to science as practice (– thus a title of a sampler by Andrew Pickering 1992). In the following years scholars became interested in 'the making of,' hence, a *constructive model* of science.

In the old framework, disorder, turbulence, agitation, circumstances were to be eliminated for a world of order, logics and rationality to appear and be maintained. In the new framework, order is nothing but local circumstances obtained from, and maintained by, dissolved from time to time in disorder; if you eliminate the opportunism, the context, the fiction building, the agitation, the reconstruction, the rationalisation you get nothing at all (Latour 1981: 70).

On this view, science and everyday communication are not different in kind; truth and reality are the consequence, not the cause of scientific research. In this vein, constuctivists, among other things, looked into the making of the objects under study (e.g., the lab mouse; cf. Amann 1994), into the significance of instruments and experimental practices (e.g., cf. Lenoir 1988) and other "inscription devices" (Latour / Woolgar 1979), into the making of facts (e.g., real life experiments, cf. Krohn / Weyer 1989), or into the making of connectable results of research (e.g., by writing a scientific article, cf. Knorr-Cetina 1984), into the management of uncertainty (Star 1985), as well as into the social and material mechanisms deciding over scientific controversies (Collins 1981). Other scholars go so far as to declare the epistemic boundaries between human and non-human actors as purely scientific attributions. Latour and Callon, for instance, accordingly to their 'actor-network theory' regard both fishermen and scallops as agents in a complex game and consequently as both having agency, and thus explanatory power (Callon / Latour 1992). In the actor-network theory, context and content are products of networks: As the latter expand, the former, be it facts or technologies, become more robust.

Likewise, social structure changes through conflicting relations in the network, i. e., an agonistic field.[21]

Recombinations in Science Studies

Throughout the 1990s, the constructivist program, in general, has increasingly met with criticism. Especially two distinguished publications stimulated heated debates about contructivism and even on science studies in general: "Higher Superstition: The Academic Left and its Quarrels with Science," written by biologist Paul R. Gross and mathematician Norman Levitt (Gross / Levitt 1994) and "Transgressing the Boundaries: Towards a Transformative Hermeneutics of Quantum Gravity" by physicist Alan Sokal: The latter submitted a parody of postmodern science criticism to the cultural studies journal *Social Text*, without telling the editors that it was a parody. Three weeks after publication Sokal revealed the hoax in an article in *Lingua Franca*. The book of Gross / Levitt and even more Sokal's experiment caused a long-standing debate, generating numerous articles on different platforms. Sometimes labelled as "science wars," the issue already has its own web sites with extensive documentation of the discourse (*http://members.tripod.com/ScienceWars* and *http://physics.nyu.edu/faculty/sokal)*.

But criticism also came from the inside of science studies:

> Constructivist studies have not provided a better understanding of what researchers see as negotiable and what they consider beyond dispute, what they implicitly or deliberately accept as knowledge to be taken for granted as given institutional arrangement and what they contest … Elements of a sociocognitive order beyond the level of locally contingent episodes of interaction and beyond individual choices and preferences will have to be invoken in order to say anything specific about why and how some researchers succeed in getting some of their knowledge claims widely accepted while others fail (Hagendijk 1990: 5).

There is a growing number of scholars who refuse to participate in what may be termed "hyper-contingency-theory" (Hohn 1998), consuming itself in fruitless debates about different relativistic positions. They rather want to return to types of science studies that allow to account for issues such as 'spontaneous discoveries,' 'scientific consensus,' 'reliable knowledge' and 'robust theories': While not denying constructivist insights altogether, these scholars want to bring back institutionalist insights and/or the material world into the explanatory horizon of science studies.

First, the so-called material stance presents one way of granting the constraints or resistances of the empirical world a causal role. It is best expressed by Ian Hacking according to whom science is practice if understood as "a play between many things: data, theory, experiment, phenomenology, equipment, data processing" (Hacking 1992: 55). Together with Andrew Pickering and David Gooding, Ian Hacking thus reinstates all elements, including empirical research, as equally crucial forces in the development of scientific knowledge. In this view, for example, non-conclusive or contradictory experiments clearly restrict possible interpretations.[22] This re-introduction of the empirical-materialist level seems to counter-balance an over-stretched externalism.

Second, a new branch of science studies, 'Cultural Studies of Scientific Knowledge,' regards science as a result of conflicts over knowledge and power in a society. While in a way one may consider this just a new brand name for old approaches[23], it is special in that it lends toward the explicitly 'critical' end of theorizing (to raise but a few flags: radical science movement; post-Marxist, feminist, antiracist schools of thought). Cultural studies of science

– considers science not a distinguishable kind of knowledge but rather a fundamentally heterogeneous endeavor[24];
– insists upon the local, material and discursive character of scientific practice;
– acknowledges that the traffic across the boundaries between science and society is always two-way;[25]

in short:

> Cultural studies 'of science' are located within ongoing conflicts over knowledge, power, identity, and possibilities for action … Yet, in doing so, they aim to participate in constructing authoritative knowledge of the world by critically engaging with the scientific practices of making meanings (Rouse 1992: 21, 22).[26]

This happens in the midst of contested and contestable, from time to time changing values which are themselves subject to contemporary STS. Exceeding Merton's institutional and technical norms, scholars inquire into "temporal, national, gender, democratic and other values as they ground institutions, theories, design, methods, policy, and other dimensions of science and technology" (cf. Hess 1997: 147).

Third, sociology of science voiced yet another opposition toward equalling science studies with sociology of knowledge: Scholars re-think the institutionalist approach. According to the radical constructivists, scientific insights are but social constructions. As has been mentioned above, pertinent studies inquire into the research practices (Latour / Woolgar 1979; Knorr-Cetina 1984; Lynch / Livingston / Garfinkel 1983) as well as into the communicative procedures in scientific discourse (Collins 1981; Mulkay / Gilbert 1984; Engelhardt / Caplan 1987).[27] However, as Zaheer Baber remarked in a review on various works in science studies, "… the issues raised by Robert Merton are still around with us" (Baber 1992: 18).

In the 1980s, Richard Whitley, for example, suggested to conceive of science as profession:

> Science today is a highly general umbrella term which covers a vast range of activities conducted by a large number of qualified personel in a variety of work organizations for a variety of purposes (Whitley 1984: 299).

Accordingly, the academic model of scientific work is no longer sufficient for understanding the professionalized sciences. He thus investigates the institutionalized conditions of science on a macrosociological level, thereby combining structural functional analyses of science as a reputational system, analyses of professionalization in science with insights of recent sociology of science. As Baber noted later, researchers indeed care a lot about, for instance, reputation and funding, hence are concerned with (if not, absorbed by) institutional structures framing research. Consequently, authors such as Arie Rip (1993), Suzan Cozzens (1986) Hasse, Krücken, and Weingart (1993) as well as Uwe Schimank (1995a) plead for a stronger consideration of institutional factors.

Even more radically, Mario Bunge concludes his review on the new sociology of science. Not only does he reject the latter's preference for looking at "science from afar" (Bunge 1992: 71) but also does he note the failure to address nonlocal and topical questions such as, for instance, the "decline of epistemic communism" (sharing data and materials due to increased competition); the increase "in exaggerated claims and unabashed publicity" or "the mounting number of fraud and plagiarism;" "the prosperity of anti-and pseudoscientific doctrines" (cf. Bunge 1992: 71). These and related questions call for a renewal of institutionalist accounts in science studies.

In brief, the time seems right to re-acknowledge institutional factors in science studies, both for political and scientific reasons: As to the former, research policy has become an important part of politics today. Pertinent topics are the analysis of innovation in science, the difficulties of implementing scientific results, or the conditions for inducing societally useful topics of research. Thus, on the one hand, extrascientific dynamics enhances the need for investigating the possibilities of politically regulating or intervening into science as an institution. On the other hand, by way of intrascientific dynamics, neo-institutionalism has been revived in various disciplines, such as the political sciences (March/Olson 1989), organization theory (Powell/DiMaggio 1991), and economics (Granovetter 1985). From this perspective – internal differences notwithstanding – institutional rules, juridical norms and formal organizational expectations restrict the range of activities of an individual actor and promote conformity, yet allow for creativity within limits. The logic of action follows a "logic of appropriateness" (March/Olson 1989).

In recent times, inspired by both rational choice theories (e.g., Esser 1990) and interactionist approaches, neo-institutionalist accounts increasingly focus on creative agency in dealing with roles and norms that, for the most part, are diffuse, fragmented and contradictory. On this view, actors pursue their goals strategically on the basis of (yet are not fully determined by) the roles and norms typical of the organization or subsystem in which they act. While acting strategically is not confined to certain subsystems, the modes and goals of strategic action differ considerably from subsystem to subsystem: Scientific actors are headed for reputation, political actors strive for power, and economic actors go for money. Accordingly, scientists act for system specific goals under conditions of system specific norms. Academic structures (e.g., scientific innovation, acquisition of funds) shape their reputation-seeking activities: Those norms and the subsystemic code of truth are distinct markers of scientific activity. Neo-institutionalists thus insist on an epistemic differences of science with respect to other spheres of societal action. At the same time, however, this claim is not to reject and replace sociology of knowledge-type of sciences studies. Rather, it is about complementing the latter as both paradigms differ in *explanans* and *explanandum*: 'Social construction of contents' and 'institutional traits of modern science as societal subsystem' can mutually enrich each other (cf. below).

If it comes to the question of how institutions are produced and reproduced by action, neo-institutionalism still needs to be elaborated. Besides allusions to habitualization theories (Berger / Luckmann 1966) and advanced theorizing in rational choice theory (cf., e.g., North 1990), scholars inquire into what they call actor-oriented institutionalism (cf., e.g., Mayntz / Scharpf 1994): In this line of thinking, research regulation results from a complex constellation of actors, differing in interests and power to influence the subsystem. Although restricted by juridical and organizational norms the outcome of regulating activities in research policy is by no means determined.[28] The intricate relations between these actors (governmental, corporate, research institution, scientists) can be studied with the help of a variety of approaches in game theory (cf., e.g., Coleman 1982), by way of modelling "critical masses" (Marvell / Oliver 1993) or dynamic social processes (Mayntz / Nedelmann 1987). On a different note, network theories of various brands inquire into less formal social linkages that play an important part in the making of science (e.g., invisible colleges[29], specialty groups, agnostic alliances[30], transscientific fields).

Epistemology Reconsidered

Summarizing, for these and other questions, science studies may be well-advised to broaden or pluralize its paradigmatic outlook on science: science as knowledge, science as practice, science as material culture, science as profession, science as institution, science as subsystem all yield interesting insights into the dynamics of making and remaking reliable knowledge. As regards the relationship of constructivist and institutionalist accounts, we follow Hohn in stating that science studies is not about 'either / or' but about combining both, if in different ways, depending on the problem at hand. Scientific activity is neither fully determined by externalist nor by internalist factors. True to social constructivism, science does not dispose of a priviledged kind of rationality but is – like other forms of political or economical activities – characterized by 'bounded rationality' and confined to strategies of 'satisficing.' However, unlike other social subsystems, science is indeed based upon "institutionalized factual critique" (Luhmann 1970: 241) and upon cognitive innovations (cf. Whitley 1984). Whether or not one agrees with Hohn that, ultimately, institutionalism is a kind of constructivism, therefore calling for epistemic integration (cf. Hohn 1998: 306), one may proceed from an empirical stance first. One might hold that scientific knowledge

> is constrained to a greater or lesser extent by input from the material world … and that the relative importance of this influence as compared with social processes is a variable that must be empirically studied (Cole 1992: x).[31]

Research is regarded as a kind of problem solving determined by various factors in unforeseeable, yet – at least post-hoc – detectable ways.

Epistemologically speaking, we thus hold that stances that as yet lack a generic name may prove most promising, namely those which neither adher to positivism nor to radical versions of constructivism. As far as Cole is concerned, he suggests to speak of "realist constructivism" (Cole 1992: x): why not? Ultimately, science should not be about epistemology but about doing research. Likewise, science studies should not be exhausted with epistemological questions but study science. Recent research in the realm of history and philosophy of science, pursued in this spirit, gives evidence to the ways in which one can most fruitfully combine constructivist and 'Mertonian' approaches (cf. Galison 1987, Giere 1988, Hull 1988 and Cole 1992[32]). For instance, research on the 'dynamics of cumulative disadvantage' helps to better understand the relatively stable disparities between women and men in salary and rank, which, in turn, provide a basis for policy issues (cf. Hess 1997: 59–64). In particular, as affirmative action programs have become disputed, the research suggests alternatives in personnel management, e.g., by modifying institutional mechanisms that magnify cumulative disadvantage.

On a general level, as knowledge – scientific knowledge, in particular – has become a much-embraced and, at the same time, much-contested part of society, science studies should and can assume an active role in observing the intricacies and pitfalls of the interactions of science and society. In the end, science studies forms part of an disillusioned kind of enlightenment: The (if loose) couplings between science and politics, economy, the media as well as its close interaction with the general public are in need of observations and interventions. As the observations can only approximate truth and as the interventions can only approximate the intended effects and prompt unintended ones as well, observations have to be incessant and interventions re-assessed. Most fundamentally, that is, both observations and interventions are themselves part of the game named dynamics of science and society and hence, themselves, part of the reflexive exercise called sciences studies. The scene of action: amidst knowledge societies (cf. Weingart 2001).

Nine Studies in Science

This brief and by no means exhaustive *tour d'horizon* of science studies was meant to show that – yes – it is an ensemble of heterogeneous endeavors only loosely connected by their subject: science. Yet, there are some trends and common concerns. Science studies today

- reconsiders the issue of institutionalism and constructivism: Increasingly, scholars call for epistemological rapprochement;
- disposes of a broad array of methodological tools – both qualitative and quantitative ones – that currently become combined and recombined, depending on the issue at hand;
- is a decidedly interdisciplinary endeavor, albeit biased by the scholar's disciplinary background;
- goes transdisciplinary: Not only does science studies disclose the complex dynamics of science and other societal subsystems evermore intricately, but also do politics and the general public ask more intensively for scientific expertise on specific issues and scrutinizes science, in general. Several applied programs like *Public Understanding of Science (PUS)* or *Science Indicators* rely on and are strongly connected to basic research in science studies.
- Shows a considerable degree of institutionalization in both teaching and research.

In response to internal specialization and increasing external demands, science studies reflects upon the dynamic interaction of science and society in more sophisticated ways. While this book cannot represent all types and levels of analyses, it probes more deeply into some of the most recent and promising aspects:

Eugenics – Looking at the Role of Science Anew. Diane Paul's article on "A Statistical Viewpoint of Historical Hypotheses: The Case of Eugenics" presents a fascinating attempt to reorient historiographic insights by way of using analytical tools that were originally developed in another discipline: in biostatistics. By deliberately employing statistical methods and concerns, Paul ventures into a new set of metaphors, a new vocabulary even, and, hence, a new way of framing the issue of eugenics. The long-standing notion according to which scientific advances played a central role in eugenics' decline thus is confronted with issues such as 'independence of evidence' or the 'dangers of pseudo-replication.' Although not alien to historiographic thinking, the impact of these concepts is increased by being couched in these terms. What is more, ultimately, we are in a position to tell different stories. Paul's study shows that and how the transfer of concepts dynamizes accounts in history of science.

Humanities – Inquiry Into the Growing Demand for Histories. In a similar manner, Wolfgang Prinz in his article on "Making sense" takes recourse to metaphors in order to elucidate the function of 'telling stories' in historiography. Therapeutic metaphors and analogies to storytelling, in particular, help to illustrate his claim that historiography, more than reconstructing the past, is about constructing the present. Histories thus, by necessity, come in the plural, they are selective and (politically) biased – on the grounds of which they, too, are not stable but highly dynamic entities. By employing therapeutic intervention as a heuristic tool, Prinz suggests to regard the historiographical endeavor "to uncover the truth about the past as integral part of a complex psychodynamical process that takes place in the present" (Prinz, this volume, 82).

Bibliometrics – Monitoring Emerging Fields. Anthony van Raan and his collaborators present an example of the quantification and measurement of science. In the past, scientometrics has often been associated with the boring business of simple number-counting of publications and citations. However, modern bibliometric methods show an advanced potential of application for analytical studies in sociology and history of science, as well as for science policy. Van Raan's case study on environmental medicine demonstrates that scientometrics is more than just the production of tables with citation statistics: Sophisticated bibliometric methods can be used as a tool for exploring the cognitive and social structures of new, unorthodox (i. e., interdisciplinary) fields in science. The study shows that it is possible to delineate an upcoming interdisciplinary field bibliometrically and that emerging themes as well as the most important groups in the field can be identified and analyzed with bibliometric means. Thus, quantitative approaches in science studies can provide a valuable information for peer review and evaluation processes.

Science Policy – Making Universities Cope with Science Today. Wilhelm Krull, in his article, explains the dramatic challenges for universities on the threshold of the twenty-first century. With the example of Germany, he analyzes the most important dimensions of change that universities are currently undergoing. His examination is arranged around the following critical issues, which are relevant for universities in most countries today. As regards funding, the role of the state will decrease and that of the private sector will substantially increase. World wide web and multimedia technology is leading to more and more virtual colleges, and university attendance will loose importance. Traditional disciplinary specialization will fall back against more inter- and transdisciplinarity. Evaluation and performance assessments, i. e., indicators for research and teaching 'outputs,' will play a significant role in budget allocations. Internationalization will be enforced not only in students but also for the teaching staff. All these trends produce a climate of dynamic change for universities.

Evolutionary Theory and the Social Sciences – Increasingly a Mutual Exchange. Peter J. Richerson and Robert Boyd, in their contribution on "Culture is Part of Human Biology …," argue for the use of evolutionary concepts in the domain of the social sciences. Specifically, they talk about blurring the boundaries between the biological and the social to explain human culture. This statement, to be sure, does not entail a plea for reductionism, one way or the other. Rather, human culture being a highly complex phenomenon that needs an evolutionary account on both the biological and the social level. On the so-called coevolutionary view, culture, like genes, create patterns of heritable variation. As natural selection will operate on any pattern of heritable variation, it will affect both culture and genes. Moreover, genes act as selective environments to culture as culture acts as selective environment to genes, if on different time scales. Only a complex interaction of both can explain the enourmous adaptivity of human culture and the dynamics of its constitutive social institutions.

Climatology – Innovative Research Strategies in a Dynamic Field. On a more general, historiographical level, Aant Elzinga's article on "Climate Research in the Field" is concerned with science policy doctrines and their history, with particular reference to their theoretical underpinnings as viewed from a social epistemological perspective. With the example of polar expeditions and research in Antarctica, he investigates the discourse on "Global Climate Change" in a novel way: He looks at the complex dynamics of the research process in this field. The case of climate change is particularly intriguing in that it is not only one of the major scientific issues that made its way up to the headlines of news magazines during the 1990s but it is also one of the most prominent domains for studying *risk communication* among science, politics and the public (cf. Weingart et al. 2000). Moreover, the field is a typical example of science in *mode 2*: highly interdisciplinary, sharing knowledge from many different disciplines; highly dynamic, rapidly developing; basic science and at the same time with a high potential of application to the needs of mankind.

Metaphors – Moving Targets in the (Social) Sciences. James J. Bono explicitly addresses the role of metaphors in science. He pleads for metaphors as instruments of thought and action in every kind of activity, scientific ones included. In line with Elizabeth Grosz, he insists on metaphors as most evidently blurring the boundaries between discourse and practice: They are both discursive and practical entities in that they are not just pieces of text but performative. Drawing on a study by Lily Kay (2000), Bono gives the following example: "Without the metaphoric construction of heredity – especially DNA – as informatic code, the mobilization of molecular biology and affiliated disciplines in the late twentieth century to produce an entire array of instruments, recording devices, and protocols to 'read' the molecular alphabet in which the book of life is written could not be imagined" (Bono, this volume, 227). The theoretical and experimental dynamics of those disciplines and their impact on societal discourse, according to this view, is subtly revealed by disclosing the performative power of metaphorical concepts upon which we reason and act.

Science and the Public – Pushing PUS with Science Studies. Bruce Lewenstein analyzes the complex interaction of science, politics and the media, which partly is also addressed by Elzinga's case study on climate research. Although surveys about public attitudes toward science and technology have been done already some 30 years ago, only since the 1990s the new field *Public Understanding of Science (PUS)* began to establish itself in the context of science studies. Lewenstein gives a critical view of PUS programs which have been introduced in most industrialized countries in recent years. He discovers a fundamental contradiction between democratic ideas of equal participation and the meritocratic ideal that produces scientific elites. Elite scientists do not understand the public's perception of science and therefore will be unable to produce PUS programs which serve the public well. PUS programs should not primarily be about scientific results but about scientific procedures. Put differently, PUS-programs should be less about 'public understanding of *science*,' and more be about 'public *understanding* of science.'

Knowledge Politics – The Paradox of Regulating Knowledge Dynamics. Nico Stehr focuses on modern societies' ways to cope with the dynamics of knowledge they increasingly rely on. As has been noted earlier, knowledge societies are confronted with a dilemma: More knowledge, even if systematically produced, may not only add up in a cumulative but also in a competitive fashion. Who decides? Adapting knowledge to local conditions changes knowledge and may have unintended consequences. Who knows? The issue of observing (novel) knowledge-in-practice, too, is not only a matter of intrascientific quality control but also (and increasingly so) becomes a matter of trans- and extrascientific assessment procedures. Both, the subject of regulation and the regulatory practices are under constant surveillance. In the end, however, regulating knowledge, hence, is about dynamizing (i.e., regulating) discourses on regulating knowledge.

In brief: The studies tackle the issue of eugenics, the humanities as well as climatology and environmental medicine. They inquire into science policy and knowledge politics and address topics such as regulating knowledge, and reorganizing universities. Moreover, they reflect upon the public understanding of science. Finally, they explore methods to grasp the intricate dynamics of (scientific) knowledge by way of bibliometrics, metaphor analysis, and address the dynamics of human culture with the help of coevolutionary theorizing and modeling. They do so with full-fledged articles, or essays, include tables and graphs or diary notes, lend toward historical or systematic analysis, respectively, make use of qualitative or quantitative methods.

With respect to subject matters addressed, methods used or style of writing – traditional or experimental – the authors convened in this book thereby touch upon issues and interests that characterize the intellectual career of the very scholar who once brought science studies to Germany and is still engaged in various projects, often in cooperation with colleagues in Europe and the US – Peter Weingart. As he is thoroughly critical of labels, we thus do not even begin to ascribe labels to him. Suffice it to say, that, while always having been a critical observer of science-in-society, he never gave up a rationalistic view: Science as an institution and as a mode of systematically producing trustworthy knowledge, to him is a success story, if in need of constant surveillance. Science studies, notably sociology of science, can help to understand the emerging paradoxes resulting from tightening couplings between science and other societal subsystems, such as politics, economy, and the media (cf. Weingart 2001), producing phenomena such as fraud, problems of legitimation, the urgency to go public, etc. In general, however, for those interested, we refer to his website (http://www.uni-bielefeld.de/iwt/pw) and, with this volume, give a specific presentation of science studies that, by implication and some explicit references, scattered throughout, is designed to characterize his work as well. Another title for this book thus could be: *Science Studies According to Peter Weingart*. May it be an inspiring source of dynamic thinking in science studies as his ideas have been inspiring to us!

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Notes

1 Leaving aside introductory sections in books and articles, the most notable exceptions are: the two-part article by Mario Bunge

(1992), introductions by David J. Hess (1997) and by Ulrike Felt, Helga Nowotny, and Klaus Taschwer (1995), the handbook edited by Sheila Jasanoff, Gerald E. Markle, James C. Petersen, and Trevor E. Pinch (1994), contributions by Bettina Heintz (1993, 1998), the reader edited by Mario Bagioli (1999), two samplers and a booklet by Peter Weingart (1972, 1974, 2001).

2 Next to obvious locations such as departments for history, philosophy, sociology, history of science, and, of course, science studies, one finds the field also in medical, law, or art schools and – least obvious perhaps – in mining schools (cf. Bagioli 1999: xvii)!

3 "The World Wide Guide to Science Studies" in the Internet, although not exhaustive, is most telling if it comes to demonstrate the heterogeneity of the field today: HS: History of Science, HM: History of Medicine, HPS: History & Philosophy of Science, HT: History of Technology, PM: Philosophy of Medicine, PS: Philosophy of Science, SciEd: Science Education, SS: Sociology of Science, SST: Sociology of Science & Technology, STPol: Science, Technology, & Public Policy, HSTM: History of Science, Technology, and Medicine, HST: History of Science and Technology, STS: Science & Technology Studies (*http://scistud.umkc.edu/wwg/info/subject.html*, December 1997). The internal heterogneity notwithstanding, the field is characterized by lively interactions and a considerable degree of institutionalization. Associations, newsletters, conferences, programs, and centers all testify to the fact that science studies has become of particular interest. While for a long time this interest has been confined to the intrascientific realm, throughout the last 25 years transdisciplinary arenas have emerged in which science studies play an increasingly important role, science policy and 'public understanding of science' being most pertinent examples.

4 One could, of course, easily extend this list by adding anthropology, feminism, cultural studies, literary criticism, etc. Our location in, and account of, science studies, however, although committed to an interdisciplinary outlook, primarily is a social scientific one. Alternatively, David J. Hess, in his introduction into science studies, makes a point of ordering the chapters along the broader disciplinary divisions: the 'philosophy of science, 'the sociology of science,' the 'sociology of knowledge' and the 'critical and cultural studies of science.' It is thus a helpful source of quick and com-

prehensive introductions into the disciplinary constituents of science studies (cf. Hess 1997).

5 Beyond the pros and cons, Peter Weingart addressed the underlying paradox guiding the recurring calls for inter- or transdisiplinarity in the face of ever-more fine-grained specialization of knowledge production (cf. Weingart 2000: 40): In his view, on the level of both organization and contents of scientific pursuit, interdisciplinarity and specialization do not contradict each other but are mutually reinforcing strategies of knowledge production. Eventually, and despite all claims to the contrary, inter- or transdisciplinarities, rather than eventually leading to a, if distant, 'unity of science,' regularly lead to new demarcations and specializations in (scientific) knowledge – thereby inevitably dynamizing the latter. "The discourse on interdisciplinarity is, in effect, a discourse on innovation in knowledge production" (Weingart 2000: 30).

6 One might as well say: two types of erosions (the erosion of truth, the erosion of certainty), but this does not imply the erosion of authority of science at large. Even though scientists produce different, if not contradicting results, science as an institution is still regarded as most trustworthy (Hartz / Chappell 1997).

7 This, to be sure, holds for rationalists and relativists alike. The most striking difference between the two factions is this: While rationalists still believe in unequivocality, relativists adhere to strict ambivalence. Stories, reasons, or meanings are *meanings for* certain persons, tribes, communities, *produced in* certain social, cultural, historical situations. From a perspective, however, that looks at the dynamics of any kind of knowledge, including scientific ones, these positions are but further incentives for producing 'true' knowledge or 'yet another story,' in other words: for producing *more* and *different* knowledge.

8 Today, as Bunge points out, they would rather be classified as internalists for they never claimed science to have a social content (Bunge 1991: 529).

9 Opinions vary as to whether and how strongly Merton was an internalist or an externalist. Bunge, voting for a middle position, states that his school "practiced a kind of externalism and internalism, never embraced constructivism and relativism, and did not underrate the importance of ideas" (Bunge 1991: 533). From a constructivist perspective, however, things look differently: Based

on epistemic realism, Merton's scientific products were exempted from 'social contamination' (Knorr-Cetina) – an assumption heavily attacked by constructivists.

10 Hornbostel rightly argues the latter kind of explanation to be as promising for connecting institutionalist to non-institutionalist accounts of science (Hornbostel 1997: 90; cf. also Merton 1977: 68).

11 To explain the functioning of science on the basis of norms deduced from highly selective documents published by a few scientists, has been heavily criticized: Law regards it a "selfvalidating methodological and theoretical system. We look for norms, we choose certain types of data – those where we expect to locate the norms, and we go on to interpret that data normatively. If we fail to find shared norms we take it that our methods are not good enough, or that the area has not been institutionalized properly" (Law 1974: 168). Generally, empirical data raised doubts as to whether following those norms would have a functional effect for the development of science at all (cf. Weingart 1972, 1974).

12 Until the late 1970s, spread over a variety of scientific journals, so many papers in quantitative studies of science had been published, that a specific journal for this growing scientific community was due. Thus in 1978 the first issue of *Scientometrics* was published. Established as an international forum "For all Quantitative Aspects of the Science of Science, Communication in Science and Science Policy" the journal covers important research contributions not only from Europe and the US, but also from other regions with a strong tradition in quantitative studies of science – like India, Russia, Hungary and other east European countries.

13 Another application relying on science indicators is Pierre Bourdieu's account of science as a bipolar field, one pole – scientific – being autonomous and self-referentially organized, the other pole – societal – being heteronomous and politically / strategically organized (cf., eg., Bourdieu 1975, 1988). Based on a conflict model, science thus is a field of competition for reputation and power, i. e., symbolic, cultural and economic forms of capital. Other than Merton's 'sporting' and rule-oriented account of scientific endeavor, Bourdieu conceives of science as war for authority, in which scientific-technical skills and social power are

inextricably intertwined. The rules guiding the scientific controversies are neither explicated in methodologies nor social norms, but regulated by a discipline-specific habitus – it materializes in practices of accumulating symbolic capital that are transmitted by way of disciplinary socialization. Bourdieu studies these practices of maximizing scientific prestige with the help of science indicators, notably publication and citation data – in his view, these data mirror objectively the activities of scientific knowledge producers (for a critique, cf. Hornbostel 1997).

14 Since the late 1970s, the constuctivist sociology of science branched off into a variety of directions. To mention but a few, cf. Michael Mulkay's discourse analysis (Mulkay et al. 1983; Gilbert/Mulkay 1984), Steve Woolgar's work on reflexivity in sociology of knowledge (Woolgar 1988), the laboratory studies (Latour/Woolgar 1979, Knorr-Cetina 1984); studies on the experiment and on the technical culture in science (Gooding 1990, Lenoir/Elkana 1988), studies on scientific controversies (Collins 1981), ethnomethodoligical studies (Lynch 1985, Lynch et al. 1985), the actor-network approach (Latour 1987, Callon/Latour 1992). Today, the delineations between the individual schools are not as clear-cut anymore. As an example, lab-studies are nowadays interested in transcending the confines of the laboratory and ask for more general phenomena in the realm of scientific discourse and transscientific negotiation.

15 This wording is by Sal Restivo, thereby referring to mathematics (cf. Restivo 1992).

16 Fleck's work on "The Emergence and Development of a Scientific Fact," published in 1935 (here: Fleck 1980), has been appreciated only lately, namely after its discussion by Baldamus (1977) and its translation into English by Merton (1977). "… the initial repression of his work was due to the fact that it anticipated a sociology of science which nobody could have possibly understood or predicted at the time" (Baldamus 1977: 151). In a way, Fleck assumed the role of a hybrid: Being periphal to all reference groups, he was in no way restricted by forms (and forces) of disciplinary consensus. As a great many of his ideas have been taken up by the "new socologists of science," however, Fleck will not be discussed separately in this introduction either but only occasionally referred to.

17 In reaction to critiques as regards the notion of 'interest' and, notably, the transition from interests to knowledge cf., among others, Hasse/Krücken, Weingart 1995), the authors de-radicalized their approach: "It is claimed that interests inspire the construction of knowledge out of available cultural resources in ways which are specific to particular times and situations and their overall social and cultural contexts …. It is true that no laws or necessary connections are proposed to link knowledge and the social order" (Barnes 1977: 58). What is more, authors cautioned against dispensing of scientific rationality *per se*: "The strong thesis does not imply, however, that there is no distinction between the various kinds of rational rules adopted in a society on the one hand, and their conventions on the other. There may be a hierarchy of rules and conventions, in which some conventions may be justified by argument in terms of some rational rules, and some subsets of those rules in terms of others. None of these possibilities imply that rational rules go beyond social and biological norms of transcendent rationality" (Hesse 1980: 56).

18 The following is based upon Heintz 1993.

19 A well-known study in this vein has been pursued by Paul Foreman, relating antirationalist tendencies in Weimar Germany to the early acceptance of the anticausal program entailed in quantum physics. (For a thorough critique, cf. the compilation by Karl von Meyenn 1994.)

20 Interestingly, Mulkay and Gilbert in a study on accounting for error among scientists found the following: "Whereas correct belief is portrayed as exclusively a cognitive phenomenon, as arising unproblematically out of rational assessments of experimental evidence, incorrect belief is viewed as involving the intrusion of distorting social and psychological factors into the cognitive domain" (Mulkay/Gilbert 1984). Discourse analysis can thus provide a detailed account of the argumentative resources scientists rely on if accounting for scientific insights as true or false, yet cannot answer theoretical questions as to how these attributions relate to scientists' interests in evaluating contradicting claims (cf. Hornbostel 1997: 116).

21 This approach has been heavily criticized for confusing identities and relations and, in effect, maintaining the differences between natural and social entities (cf., e.g., Gingras 1995).

22 This, for instance, has been called "Widerstandsaviso" by Fleck (Fleck 1980: 124).
23 See the collection of authors convened under this rubric in Rouse 1992, 2.
24 On this, Michel Foucault provides some of the key concepts. In particular, scholars rely on his notion of a 'dispositif': It refers to science as a "hererogeneous ensemble of discourses, institutions, laws, administrative measures, scientific statements, philosophical, moral, and philantropic propositions ..." (Foucault 1980: 194). (Scientific) knowledge, on this view, is intimately connected to forms of power: "The exercise of power perpetually creates knowledge and, conversely, knowledge constantly induces effects of power" (Foucault 1980: 52)
25 A large faction of the work in the realm of 'Public Understanding of Science' (PUS) is done by scholars who adopt a critical cultural stance, pertinent approaches and issues being as diverse as 'scientific literacy,' the analysis of scientific and technical controversies in a democratic culture and 'ethnoscience' (cf., e.g., Nelkin 1994).
26 On a polemical note Rouse concludes his programmatic by stating: "... social constructivism is antagonistic to the cultural authority claimed by the natural sciences, but uncritical of scientific practices. Cultural studies reverse this stance, aiming to participate in constructing authoritative knowledge of the world by critically engaging with the sciences' practices of making meanings" (Rouse 1992: 22).
27 Scholars focus on micropractices in research as well as on their relation to political and economic interests. In a prototypical manner, Latour's study on Louis Pasteur (Latour 1984; 1987) or Lenoir's study on research in the German Kaiserreich (Lenoir 1992) show so-called 'seamless webs' of social factors conditioning science. These and other case studies revealed useful insights into the making of scientific facts and also – implicitly or explicitly – rejected and replaced the institutional paradigm.
28 An overlap can be seen here between science studies and Technology Assessment (TA) as a separate field with a similar development. From the beginning of the 1970s, TA has been introduced as an instrument to monitor critical issues in science and technology in governments and parliaments in many countries. Although the USA played a leading role in TA during the 1970s

and 1980s, in 1995 the US Congressional Office of Technology Assessment (OTA) was finally closed down. The voluminous output of OTA is still available (http://www.wws.princeton.edu/˜ota/), but today there seems to be much more TA activity in Europe (Vig / Paschen 1999). All the official OTA assessments from 1990–1995 are archived at this site, along with many background papers and other documents.

29 Based on Price (1963)

30 Collins / Restivo (1983)

31 Accordingly, we agree with his evaluation of constructivism: "Constructivists do show that the doing of science is not the rational rule-governed activity it has been depicted as and that serendipity and chance play a significant role in the construction of local knowledge outcomes. Studies done by social constructivists do suggest (but have not yet demonstrated) that local knowledge outcomes *may* be influenced by social variables. These studies have not proved that the extent to which theories match data from the empirical world has no influence on local knowledge outcomes. They show that science is underdetermined but do not show that it is totally undetermined" (by empirical data) (Cole 1992: 229).

32 In his book on "Making Science," Cole (1992) argues that it is social variables interacting with cognitive variables that influence the foci of attention and the rate of advance in science. Social variables alone, however, cannot explain the communal acceptance of a scientific solution.

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Eugenics

Looking at the Role of Science Anew

Among the most troubling phenomena of the last century, one finds the political, moral and scientific issue of eugenics. As is well-known, eugenics became a movement mostly within the framework of public health throughout the Western industrial countries, especially Britain, the United States, the Scandinavian Countries and Germany. The underlying notion was that the endangered or already degenerated heriditary stock could only be improved by the control of individual reproductive behavior and/or the reform of social institutions held to be counterselective (cf. Kevles 1985). Thus eugenists focused on human reproduction and its institutions, notably on the choice of mates and marriage (cf. Schallmeyer 1918). The selection-oriented social analysis was translated into a comprehensive scheme of social reform. In Germany, this was carried to extreme measures, and ultimately became associated with the atrocities of the Third Reich (cf. Weingart et al. 1988).

For obvious reasons, eugenics has become the center of a long-standing debate engaging scientists and the public alike. In particular, historians and sociologists of science inquire, among other things, into the rise and fall of an overwhelming ideology, the change of values connected to it, as well as into the role of scientists, professions, and politicians involved. Thus part of the history of eugenics is the history of scholarly attempts at understanding it. The question is: What is the role of science if it comes produce, obscure, and/or enlighten eugenics as a powerful tool of reasoning and intervention that regulates the behavior on the level of individuals and populations? Thus far, historians of science tended to tell stories that 'make things straight': In these stories, science played a significant role in both the waxing and waning of eugenics.

Only recently, scholars began to doubt that story, among them Diane Paul and Peter Weingart. Both authors stress the idea that by importing ideas or instruments from other disciplines dealing with related phenomena one makes use of a rich heuristic base from which on may look at certain stories (in science studies) anew. Diane Paul, for instance, suggests to employ specific statistical tools that reckon with methodological concerns similar to the historians', such as the issue of sampling data or the independence of evidence. In fact, she calls those statistical terms a "set of productive metaphors." Treating concepts as metaphors is precisely the approach Weingart uses when he, for instance, looks into the fate of Darwinian terms, or metaphors, such as "struggle for existence." From this perspective, one can observe the specific ways in which various disourses make use of a term thereby expanding and changing both its meanings and pragmatics over time (Weingart 1995). Statistics or metaphors are but two tools that help to regain the distance necessary to avoid (probably all too pleasing) short-cuts in science studies, notably in history of science. Diane Paul's historical account of the role of science in eugenics, guided by statistical concepts, is a powerful case in point.

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A Statistical Viewpoint on the Testing of Historical Hypotheses: The Case of Eugenics

Diane B. Paul

Introduction

This essay represents an exercise in what might be called a non-speculative, non-abstract philosophy of history. Most historians seek to establish causal connections. Since statisticians' stock in trade is distinguishing cause from correlation, I suggest that we might glean fresh historiographic insights from their work. I do not mean by this that historians should become more quantitatively-oriented, much less that they ought to make use of specific statistical tools. Indeed, no parametric (and for most historians, few if any non-parametric) statistics are applicable to their domain. But statisticians do reckon with methodological issues – involving sampling, replication, independence of evidence, and so forth – that are deeply germane to the historians' task. I argue that a sensitivity to statisticians' concerns and methods might suggest a new vocabulary, new ways of framing issues, and a new set of productive metaphors. The point is not to (further) scientize history, but to propose a potentially useful heuristic for thinking about problems of ascribing causation to events in the human past.

To illustrate its value, I reanalyze a long-standing debate in the history of eugenics. Both Peter Weingart's work and my own involves a critique of the view that scientific advances played a central role in eugenics' decline. I begin with a synopsis of the received view, followed by an account of the arguments that have convinced most historians that this view is unsatisfactory. Taking a 'reflexive turn,' I then reevaluate aspects of the critique in light of methodological considerations prompted by a recent encounter with the field of biostatistics.

The Received View and its Critics

On the once-conventional view, support for programs of selective breeding flourished at a time when the science of genetics was in its infancy and eroded as the science became more sophisticated, exposing eugenics' technical flaws.[1] Thus in the first flush of enthusiasm for Mendelism, geneticists attributed many mental, moral, and temperamental traits to heredity, and often to single Mendelian factors. If individuals with bad heredity could be prevented from breeding, it seemed that the problems of 'feeblemindedness,' pauperism, sexual promiscuity, and crime would quickly be controlled. According to Charles Davenport, then America's leading geneticist, after only a single generation of segregating mental defectives and anti-social individuals from society during their reproductive years, "the crop of defectives will be reduced to practically nothing," as would the host of social problems they create (1912: 286).

However, by the 1920s it had become evident that most traits, and certainly behavioral ones, were influenced not just by one but several factors, now called 'genes.' Moreover, these genes acted in complex ways with each other and the environment, so there was no unilinear relationship between a gene and a character. Most important, discovery of the Hardy-Weinberg principle (which makes it possible to calculate the frequency of carriers when the frequency of the gene is known) destroyed the hope that selection against undesirable traits could eliminate or even greatly reduce them. It is an implication of that principle that, when a trait is recessive and rare, most of the deleterious genes will be hidden in apparently normal carriers. Since the affected individuals represent only the tip of an iceberg, their sterilization or institutional segregation would do little good. To appreciably reduce the incidence of a trait, it would be necessary to prevent the numerous heterozygotes from breeding. But even if this were politically possible, there was no way to identify them. Thus the scientific facts made increasingly clear that such a policy was unrealistic, and support for eugenics – at least in its more interventionist forms – therefore faded. Although many geneticists continued to voice eugenic ideals, they no longer endorsed practical measures to control human breeding. Geneticists had been leaders of the early eugenics movement, which was crippled by their withdrawal of support. Or so goes the customary interpretation of eugenics' rise and fall.[2]

It continues to inform much popular writing on genetics and eugenics. Textbook authors seem to find it especially appealing, perhaps because it short-circuits inconvenient ethical and political debate. If eugenics were based on technical errors, which have long been exposed, it is a historical curiosity, not a matter of contemporary concern. It is also attractive to geneticists, who like scientists generally, are naturally disposed to view "science as a progressive social factor which enlightens and dispels prejudices" (Roll-Hansen 1988: 295). As geneticists, they have a particular incentive to attribute eugenics' decline to increasing scientific sophistication. The troubling history of their field is constantly invoked by critics of contemporary human and medical genetics. It is surely comforting and convenient to assume that geneticists, who once enthusiastically supported eugenics, also exposed its shortcomings. It shows that they were ultimately reliable and, by implication, that their heirs are worthy of trust.

In more sophisticated versions, this interpretation has also appealed to historians of science with a strong rationalist/realist orientation. Thus, in a series of important papers, Nils Roll-Hansen has defended the view that science has been a socially-progressive force, in the history of eugenics as elsewhere. While not ignoring the significance of ideological and political factors, particularly the post-World War II emphasis on individual rights, Roll-Hansen has consistently stressed that increasing knowledge of human heredity made clear the inefficiency of eugenical selection, and hence was largely responsible for the decline of eugenic thinking in the 1940s and 1950s (1989: 343–345). In his rightly influential history of eugenics, Daniel Kevles similarly acknowledges the role of non-scientific factors, while according scientific developments an important role in eugenics' decline. Kevles distinguishes 'mainline' from 'reform' eugenicists; the former tended to be socially and politically conservative, infected by racial and class bias, and scientifically naive, while the latter tended to be liberal or left in their thinking, reject racial and class bias, and understand that too little was known about human heredity to justify coercive measures. In Kevles's view, an important factor in turning the reform geneticists, such as J.B.S. Haldane, Lancelot Hogben, Julian Huxley, and H.S. Jennings, against mainline eugenics was "the rapidly advancing field of genetics" (Kevles 1985: 124–125).

Today, the view that eugenics was unable to withstand its encounter with scientific fact, or was even seriously undermined by it, is no longer fashionable in history or social studies of science. It cuts against the grain of constructivist accounts of knowledge, which as Barbara Herrnstein-Smith notes, stress "the *participation* of prior belief in the perception of present evidence," as opposed to realist and rationalist accounts, which "insist on the possibility of the *correction* of prior beliefs by present evidence" (Herrnstein-Smith 1991: 140). But even historians not inclined toward constructivism have come to doubt whether the undoubted advance of science played a significant role in the waning of eugenics. The doubts arise from the following considerations:

1. The scientific developments conventionally said to have eroded support for eugenics occurred in the 1910s, long before eugenics became disreputable. The multiple-factor explanation of continuous variation was first suggested by the English biometrician Karl Pearson in 1904, followed by the Swede Herman Nilsson-Ehle in 1908–09 and by the American Edward M. East in 1910, while R.A. Fisher's famous paper, "The correlation between relatives on the supposition of Mendelian inheritance," appeared in 1918. Discovery of the Hardy-Weinberg principle and recognition of its implications also occurred early. The work of G. H. Hardy and (independently) Wilhelm Weinberg dates to 1908; its consequences for eugenics were first noted – by East – and refined by R.C. Punnett in 1917.
2. A related consideration is that a thorough critique of eugenics long predated its decline. Thus, the methodological problems that vitiated much of the work that issued from the Eugenics Record Office at Cold Spring Harbor were identified by the biometricians Karl Pearson and especially David Heron in *Mendelism and the Problem of Mental Defect,* published in 1913–14. Heron in particular identified numerous technical flaws in the work of Charles Davenport and other American Mendelian eugenicists in a series of very well-publicized critiques (Spencer / Paul 1998).
3. The researchers responsible for the growth of relevant knowledge and correction of error were themselves eugenics enthusiasts. They included Nilsson-Ehle, East, Pearson, Heron, and even Punnett (who is often misconstrued as a critic [cf. Paul / Spencer 1998: 122]).
4. Although the Hardy-Weinberg theorem undermined claims for the rapidity of selection, it did not demonstrate that eugenics was futile. As noted earlier, it had often been said in the 1910s that one or two generations of eugenical selection would be enough to eliminate the problem of mental defect. But as the implications of the theorem were absorbed, it became evident that this claim was untrue, and by the early 1920s, the nature of the mistake was well-understood.

However, the new understanding seems not to have led anyone committed to eugenics to abandon their stance. Nor is there any reason it should have. For one thing, mental defect was not considered rare. Indeed, the *raison d'être* of the eugenics movement was fear of the normal population being swamped by the 'feebleminded.' As R.A. Fisher showed, on standard assumptions about the incidence of the trait, substantial progress would result from a few generations of eugenical selection. And, as Fisher also noted, the point of eugenics was never to eliminate the last defective individual. Moreover, even if selection did work very slowly, most eugenicists would continue to support policies to prevent mental defectives from breeding. In their view, even a small reduction in the trait was better than none. H.S. Jennings, who did much to make the implications of the Hardy-Weinberg theorem clear to the public, reflected a common view when he wrote: "Even though it may get rid of but a small proportion of the defective genes, every case saved is a gain" (Jennings 1931: 207).

5. In at least some respects, eugenics gathered steam in the 1930s. If science exposed the futility of eugenical selection, how can we explain the fact that between 1933 and 1940 compulsory sterilization laws were adopted by all of the Scandinavian countries, Germany, and Japan (among other states), while existing laws were more rigorously enforced? In the 1910s and 1920s, there was considerable opposition to compulsory sterilization on the grounds that the statutes were ineffective, rested on unfounded assumptions about the heredity of the targeted defects, promoted sexual promiscuity, and were biased in their application. In the U.S., most eugenicists favored segregation. But opinion shifted in the 1930s. Prominent foes of sterilization now came to think it sensible. The turn from segregation to sterilization was presumably a response to the world-wide economic depression. Sterilizing institutionalized individuals, who could be returned to the community, reduced the burden on the public fisc.

That is not to claim that science was superfluous. Indeed, it clearly mattered in at least two ways. First, it prompted eugenicists to modify their arguments. The claim that feeblemindedness could be eliminated in a generation or two was effectively abandoned. Moreover, the Hardy-Weinberg theorem provided resources for critics. Both in- and outside the genetics community, the claim that selection was too slow to justify the effort was frequently employed by critics of segregation or sterilization. What it did *not* do was convince proponents of these practices to change their mind. Here as elsewhere, prior beliefs were remarkably stable in the face of apparently contradictory evidence. Individuals who thought that mental defectives should not be permitted to breed had very little trouble accommodating the Hardy-Weinberg principle.

6. Revelations of Nazi atrocities did produce widespread revulsion against genetic explanations of individual and group differences in general, and eugenics in particular. In the 1950s, "cultural determinism" reigned (cf. Nelkin/Lindee 1995: 34–37). But this development obviously had nothing to do with science. As Weingart notes,

> the crucial factor in the [post-war] loss of legitimacy of eugenics and race-biology, in conjunction with the overwhelming moral indictment, was a shift in political values, i.e. the restoration of the rights of the individual and not, as is often claimed by the scientific community, the prevalence of 'good science' – the new genetics – over 'bad science' – German race-hygiene – and/or the end of the 'abuse' of science by corrupt political regimes – the Nazi's suppression of genetics (Weingart 1999: 173).

Moreover, the rejection of eugenics was very uneven. Many scientists did not share the new enthusiasm for cultural explanations of human differences; their misgivings were reflected in resistance to the first version of a 1949 UNESCO statement asserting that all races were genetically equal (Provine 1986). As early as the mid-1950s, a backlash against the dismissal of eugenics was evident. Prominent molecular scientists, perhaps emboldened by the discovery of the double-helical structure of DNA and unraveling of the genetic code, argued that recent medical and military developments necessitated what they explicitly called 'eugenics.' In the view of these scientists, advances in medical treatment were allowing many individuals who would in the past not have reproduced to enjoy near-normal fertility. At the same time, expanded medical and military uses of atomic energy, especially atmospheric nuclear testing, were increasing the load of deleterious mutations. Moreover, many commentators assumed that a perceived population explosion would anyway require restraints on human breeding. If it were necessary to control population quantity, they reasoned, why not also control population quality?

In the 1950s and 1960s, scientists such as H.J. Muller, Bentley Glass, and Linus Pauling in the U.S., Francis Crick, Julian Huxley, and N.W. Pirie in Britain, and Hans Nachtsheim in Germany (as well as the American theologians Paul Ramsay and Joseph Fletcher), argued that there was an urgent need to replace the current laissez-faire system of reproduction. In 1952, Nachtsheim even attempted to resurrect the Nazi sterilization law of 1934 (Weingart, Kroll, and Bayertz 1988; Weingart 1999: 175; Paul and Falk 1999). A number of commentators thought they saw a trend, with eugenics again becoming fashionable (cf. Paul, in press). They could hardly have been more wrong. By the mid-1970s, eugenics was definitely in disrepute, at least among those who came to speak for the public in the realm of reproductive genetics.

This turn of events seems most plausibly explained by the social turmoil that began in the 1960s. The anti-war and civil rights campaigns challenged established authority, a trend reinforced by the patients' rights and womens' movements that followed in their wake. A series of scandals involving experiments on human subjects undermined the assumption that physicians could be trusted to act in their patients' best interests. At the same time, women demanded control of their economic resources, their life decisions, and especially their own bodies. 'Autonomy,' 'choice,' and 'self-determination' became feminist dictums, and the concept of 'reproductive responsibility' was replaced by 'reproductive rights.' In the new perspective, reproduction was an entirely private matter, in which the state had no business meddling. These are the main elements in the case against the conventional view that attributes the decline of eugenics to the progress of genetics. To summarize: The scientists responsible for the developments said to have undermined eugenics were themselves eugenicists, whose discoveries occurred too early to provide plausible explanations for shifts in attitudes that began only in the post-World War II period. The most oft-cited scientific discovery – the Hardy-Weinberg principle – was not after all inconsistent with advocacy of eugenical selection. Nor can scientific discoveries explain the sudden resurgence of eugenic discourse in the 1950s and 1960s and its equally rapid fading.

Rethinking the Critical Case

For all these reasons, the conventional account seems to fail. But its critics are also vulnerable at several points. Thinking about statistical methodology directs our attention to the following problems in both the received and more recent views:

1. Sampling bias: *Whose* views are to be sampled? I have looked primarily at geneticists. But I could have selected a different focal group, such as doctors, social workers, home economists, or politicians. Moreover, it is possible – even probable – that none of these are reflective of the views of the public(s). As Martha Nussbaum notes, all cultures involve conflict over norms, and 'what most people think is likely to be different from what the most famous artists and intellectuals think' (Nussbaum 1997: 127–128). In general, norms are articulated by elites. What looks like a general cultural shift may instead reflect shifts in thinking among elites or even the replacement of one set of elites by another. In this case, it is relevant that beginning in the 1970s, bioethicists began to replace scientists as the primary spokespersons on social and ethical issues in genetics. During the 1950s and 1960s, most books on genetics-related issues were authored by distinguished scientists, and it was to scientists that journalists and conference-organizers typically turned for guidance on such issues. But by the 1970s, that discourse was dominated by bioethicists. Having emerged as a distinct intellectual discipline in the 1970s, bioethics was inevitably affected by the patients' rights and feminist movements, and it embraced as its core value the principle of respect for autonomy. Thus underlying norms about reproduction may in fact have been much more stable than we would be led to believe if our evidence were limited to statements by professionals.
2. Ascertainment bias: This related kind of bias, in which skewed results arise from the way in which cases come to our attention, vitiated much work in eugenics itself. For example, Heron pointed out that some data collected by the Mendelian eugenicists was tabulated and analyzed only when at least one child in each family was mentally defective, thus creating an excess of defectives. But ascertainment bias is equally a problem for historians. My work, like that of many others, considers (a handful) of individuals, who

came to attention because they were famous and published in leading journals. If they constituted an unrepresentative sample, adding more instances would not help. The result would be what statisticians call "pseudo-replication."

3. Multiple endpoints: What counts as evidence for the truth of our hypothesis? The problem of deciding what to measure is notoriously severe when the subject has fuzzy boundaries and is hard to define (Gilovich 1991: 59). The term "eugenics" has protean meanings. Some definitions are extremely broad, incorporating virtually any activity in the realm of human breeding. For example, today prenatal diagnosis is sometimes considered eugenics on the grounds that it involves selection of fetuses. But it is more often excluded on the grounds that the technology serves individual rather than social purposes and/or that the means employed are voluntary rather than coercive.

   How eugenics is defined has political implications. Critics of contemporary genetic testing generally prefer an expansive definition, thus associating testing with disfavored practices of the past. Supporters, on the other hand, tend to favor a narrow definition, thus divorcing testing from those same practices. However, the choice of definition also matters greatly for any thesis about eugenics' decline. When an earlier generation of historians claimed that eugenics fell into disrepute in the 1930s, they referred not to the general idea of improving human heredity through selective breeding but to the specific beliefs about class and race superiority and specific practices associated with the 'mainline' movements. That is why these histories characterize geneticists such as J.B.S. Haldane and H.S. Jennings as critics of eugenics, notwithstanding views about who should and should not breed that would, on the broader definition now (implicitly) adopted by most historians, mark them as proponents. Underlying disputes about continuity or discontinuity are often disguised conflicts over definition.

   Even if we could agree on the meaning of eugenics, it is not obvious what the best indicators of its waxing and waning would be. Passage of laws or other concrete policies? If so, which? Attitudes? If so, whose? I have used passage and enforcement of the sterilization laws as a measure of support for eugenics. But there is considerable evidence that advocates of such laws were generally indifferent to the cause of mental defect; in their view, it was irrelevant whether feeblemindedness was attributable to heredity or environment. What mattered was that institutionalization was expensive and that mental defectives made bad parents. On this line of reasoning, there is no inconsistency between increasing support for sterilization and declining belief in the power of genes to shape mentality and behavior. Thus changes in respect to sterilization may correlate poorly with hereditarian beliefs or other conventional markers of eugenics.

4. 'Optional Stopping' (or 'Variable Windows'): This is an analogue to the problem of multiple endpoints, involving shifting timeframes rather than kinds of evidence. When does one stop counting? Roll-Hansen has noted that it may take considerable time for the implications of some scientific developments to be recognized and incorporated into practice. He is right. Perhaps the discovery of multifactorial inheritance and the complexity of gene action had long-delayed effects. But without specifying a timeframe in advance, the temptation will be to stop at the point that the hypothesis is confirmed.
5. Particularity: History is always the single realization of a process. This problem of having one sample path of course unites studies of nature and society: paleontology, historical geology, evolutionary biology, systematics, the study of the origin of life, perhaps even cosmology – as well as the histories of science or, for that matter, of printing, or peasant revolts, or changing styles of dress. There was one Cambrian explosion – or only one we can study – just as there was only one Copernican and one industrial revolution, and one world-wide Depression. Only Emile Durkheim founded French sociology – and only once.

    Of course to test historical hypotheses, we can sometimes make use of 'natural experiments.' One form of natural experiment is the comparative method (cf. especially Adams 1990). The industrial revolution occurred in multiple places, as did the eugenics movement. But use of the comparative method to look for fundamental commonalities is fraught with difficulty. In the history of eugenics, there are such a such a small number of cases that we quickly use up the 'degrees of freedom.' Moreover, these cases are not independent of each other: The eugenics movement in Germany was influenced by eugenics in the United States, which was influenced by eugenics in Britain, and so forth.

6. Interaction: R.C. Collingwood enjoined historians to get inside of an event, into people's heads, to rethink the thoughts and relive the experience of historical actors. But as even scholars sympathetic to Collingwood have noted, this approach can take us only so far, since historical events and states of affairs occur 'over the heads' of the participating individuals (cf. Dray 1989: 9). Charles Rosenberg has made a similar point in arguing the need for etic as well as emic approaches in the history of science on the grounds that the larger conceptual, social, and material structures in which their work is embedded "are often opaque to the objects of one's research" (Rosenberg 1988: 566).

The need to take account of woods as well as trees (in Rosenberg's phrase) means that we face the problem of sorting out interacting factors, a task that is particularly daunting when there are a large number of weak interactions, and so small contributing causes. In biology, effects may be masked – or exacerbated – depending on the company they keep. While there can be an effect of A regardless of B, and of B regardless of A, whether A matters may also depend on the presence and level of B. Even the direction of A can reverse depending on B.

In the realm of parametric statistics, the analysis of variance, for all its limitations, provides a tool for untangling the effects of multiple factors, each having not only "main" effects of their own, but also effects only in conjunction with other factors. Of course we can not use anova to sort out complexly interacting factors in history. But we can be attentive to the fact that particular scientific developments may be causally efficacious only when linked to specific social events. That point is illustrated by the history of the Hardy-Weinberg theorem. In the 1920s, it seemed to most people to provide a better reason to expand eugenic efforts (by identifying the hidden carriers) than to abandon them. By the 1970s, the same theorem seemed instead to provide self-evident proof of eugenics' futility. What changed were our values. When individual rights came to be held in high esteem, the theorem came to carry quite different implications than it had previously.

In short, the study of statistics directs our attention to the importance of replication and independence of evidence, to the dangers of pseudo-replication and of sampling and ascertainment bias, to the temptations of optional stopping, and in general to the difficulty in concluding that a factor is causally efficacious in the world. Historians are of course well aware of these issues, which have all been described under other labels. But employing the language of another discipline, particularly one focused on causes, can bring some of their features into sharper relief. It can also help us to be more genuinely reflexive about our own work.

Acknowledgements

Two years ago, I was persuaded to develop a freshman seminar in quantitative thinking. A less logical choice to teach such a course would have been hard to find. I had no background – having slept through two cookbook-style statistics courses in graduate school – and even less interest in matters quantitative. However, faced with the daunting prospect of having to teach the subject myself, if only at a baby level, I bought a small library of books and also sat in on yet another course. That this time it clicked is attributable mostly to the extraordinary skill of the instructor, Richard Lewontin, and the section leader, Andrew Berry (who helped me think through the implications for historiography). I am grateful to Hamish Spencer and to Mark Solovey as well for useful comments on a draft of the essay.

Notes

1 For a more detailed account of the history of this argument cf. Paul and Spencer (1998).
2 According to Buchanan et al., earlier historians believed that: "Eugenics was abandoned as the science of genetics progressed, leaving genetic scientists increasingly dubious of the factual claims of the movement" (Buchanan et al. 2000: 39).

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Humanities

Inquiry Into the Growing Demand for Histories

The disenchantment of the world through the intervention of science and technology did not leave the humanities unaffected: From the mid-1960s onward they have repeartedly considered themselves as being in a state of crisis. Notably, they criticize the dominance of the natural sciences, not least when it comes to public attention and funding, but also with respect to the more and more scientific standards of communication and organization. While macro-analytical studies have shown that the humanities participated in the general growth of the academic system in the 1970s, and actively responded to it by internal specialization and differentiation (cf. Weingart et al. 1991: 14ff.), representatives of the humanties painted a different picture. They not only insisted on being a special 'culture' (Snow 1959) or 'tribe' (Becher 1989) but, more precisely, on a special function, namely that of 'compensating' for what got lost in contempary society: Most prominently, Odo Marquard suggested that the humanities were important in that they tell stories that help to sensitize and orient people in a throroughly scientized world (cf. Marquard 1985; accordingly, this function would need a specific science policy, cf. Pöggeler 1980). In this view, the scholarly research of, say, cultures, languages and histories contributes to enlighten and empower people so as to rationally act with and among modern technologies.

Historians, while engaging in non-academic enterprises as well (e.g., expositions), predominantly pursue this task within the confines of academia. Interestingly, internal specialization shows, among other trends, a shift toward modern history, social history, history of non-European countries as well as of technology including science and medicine (cf. Weingart 1991, chapter 2.3) – obviously, these histories are designed to equip the members of contemporary globalized, high-tech societies with orienting knowledge. Accordingly, the discipline engages in epistemic self-reflection: In particular, it reflects upon its self-proclaimed specificity of telling stories. Its narrativity (cf., e.g., Rüsen 1987), its rhetorics (cf., e.g., White 1990), its centrisms (eurocentrism, androcentrism, …) center stage in various debates. Writing histories (or his-tories, for that matter) has become a target for science studies as well (cf. also Paul, this volume). Thus far, however, scholars have predominantly concerned themselves with external factors, such as growth, specialization and differentiation as well as with science policy (Frühwald et al. 1991); with views held from within the humanities (Prinz / Weingart 1991), as well as with transdisciplinary productions of humanist knowledge (Gibbons et al. 1994). In the following essay, Wolfgang Prinz suggests to conceive of histories (whatever their area of research or methodology) as serving a specific demand – the demand for an overwhelming cultural concern in making sense. Histories, more than re-constructing the past, construct the present. Seemingly complying with the historians' own account, Prinz gives the theme a special twist, though: Making sense does not so much result from science political ambition (cf. above) but from folk psychological necessity.

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Making Sense

Wolfgang Prinz

We,
amnesiacs all,
condemned to live in an eternally fleeting present,
have created the most elaborate of human constructions,
memory,
to buffer ourselves
against the intolerable knowledge of the irreversible passage of time
and the irretrieveability of its moments and events.[1]

I am a fan of historical studies and I have always admired them from an amateur's perspective. Still, at the same time I have always had mixed feelings when it comes to understand what scholars in historical studies are actually doing and what the point of their business is. Today I believe I know the answer. The point of their business is making sense of facts. Yet, I am not sure whether I really understand what this means. For instance, since I find it difficult to think of sense as a thing that is somehow inherent, or residing in facts I cannot see an obvious difference between the making and the faking of sense. Further, since I think of sense as a thing that always needs to be shared with other contemporaries I cannot see an obvious difference between the making of history and the making of politics. Of course, I realize that many scholars in the field hate this proximity and alleged affinity, but I also know that a number of others enjoy it quite well.

Mixed Feelings

My mixed feelings about history are threefold, with admiration, envy, and trust as chief ingredients.

First, I admire historians for the coherence of the stories they tell and for the boldness with which they create them from scarce sources and documents – that is, from highly selective left-overs that provide evidence about a very small number of events, as compared to the vast pool of events that may actually have happened. To be sure, my admiration is not only for the beauty of the stories but also for the boldness of the claim that they tell the truth.

Second, my envy has ever pertained to the high appreciation and esteem historical studies earn in our culture. In a way, it is a hallmark of the ideology of modern Western civilization, that the best way to understand what is going at a given time is to explain it in terms of what has happened before. Interestingly, this principle is not only applied to macro-social entities like cultures, states, or peoples, but to micro-social entities such as firms, families, or individuals as well: Everybody believes that the study of the past is a prerequisite for the understanding of the present, and everybody is convinced that historical studies, as we know them, can do this job quite well. This is why I envy them.

Still, and third, I also distrust them. The reasons for my distrust are the reverse of the coin that explains my admiration. If it is true that the stories about the past are bold constructions on the basis of highly selective evidence, it is, in my view, indispensable that reflections about selectivity and bias (both deliberate and imposed) become an integral part of the scientific endeavor to construct such stories. I do not see much of these reflections, though, and this is what fuels my distrust.

I was trained as an experimental psychologist, and this may explain part of the story. Psychologists, too, are trained in selecting and interpreting data that help them to understand other people's actions. However, they are systematically trained to distrust what people tell them and even mistrust their own understanding of what they see these people doing. Moreover, when it comes to relating data to theories, psychologists have developed a methodological culture of taking the selectivity of their data base into account – as well as the inevitable bias inherent in such selectivity. Given this background, my mixture of admiration and distrust may not be too surprising. Likewise, my feeling of envy can be traced back to my professional background, too. Psychologists, unlike historians, do not often enjoy public appreciation for doing their job well. Therefore, my envy comes as no surprise.

What does it actually mean to tell a story about events that happened in the past? For the rest of this chapter I will discuss two aspects of selectivity and bias inherent in historical studies. First I will discuss the issue from the viewpoint of the facts to be conveyed. How are stories produced from facts and how do facts get picked and glued together for the sake of telling stories? This may be called story semantics. Second I turn from story semantics to story pragmatics and discuss selectivity and bias from the viewpoint of the discourses addressed by stories. How do stories get adapted to the audiences they want to speak to and how do new stories get streamlined to catch attention and compete with old ones?

Ways of Making Sense

What does it mean to tell a story *about events that happened in the past*? How can the story we tell today be related to the events that happened yesterday? In which sense can such stories be true? First I will examine how stories get individuated and how the facts for a given story are selected. Then I will turn to the inner workings of stories about human actions, that is, the basic semantics of integrating and making sense of their bits and pieces.

Picking facts. Stories need to have a beginning and an end, and this is true of both fact and fiction. Stories about historical events are both written and read in the understanding that they pertain to facts, that is, events that have actually taken place. Facts, however, have no inherent beginnings and ends, and therefore the beginning and the end is always in the story about the facts and never in the facts themselves. This may be trivial to state, but I do not see much reflection of this triviality in historical studies. Quite on the contrary, they often convey the impression that their stories begin and end where the happenings that are being told have their natural and inherent opening and closing.[2]

Let us suppose that we have fixed where our story begins and where it ends. Further, let us assume that we know a number of facts about events that have happened in the domain and the time of our story. For the sake of the argument, suppose that we know of 1,000 such facts. Obviously, we are then faced with the issue which of these events belong to our story and which not. How do we decide which ones we should pick? Again, the facts themselves do not tell us. It is the framework of the story we have in mind that helps us make our decisions.

The story we have in mind determines where it begins and where it ends and what belongs to it. Let me call this top-down selectivity. At the same time, we are faced with heavy bottom-up selectivity. Bottom-up selectivity arises from the simple fact that what we can know about events that happened in the past can always be merely a tiny sample of all the events that actually happened. To put it in an extremely naive form, 1,000 events of which we know, in the time window and in the domain of our story, may be 1,000 out of 1,000,000 that have actually happened.

Even worse, the sample is by no means representative. It is, on the contrary, biased in various ways. Such biases of what we can know as compared to what actually happened have often been discussed. I will not go through them systematically, but only mention some of them. For instance, one source of bias comes from the fact that most of what we know about events in the past is derived from intentional artifacts, that is, objects and documents fabricated by certain individuals for certain purposes. We may have access to the events contained in, or documented by, these artifacts, but to the huge number of events that are not thus documented, we have no access at all. Second, only a tiny fraction of all these artifacts has survived until today. The vast majority got lost, and it is certainly not by chance which ones got lost and which survived. This creates another source of bias in what we can know about past events. Third, the information that we actually access and, hence, the pieces of knowledge we actually know, will once more form a subset of those pieces that we could know if we had access to all sources still available.

Interestingly, this picture is completely homologous to what textbooks on Psychology have to say about the functional locus of forgetting in human memory. According to text-book wisdom, forgetting (i.e., selective loss of information) may occur at three different levels: encoding, storage, and retrieval. If an event that has actually occurred in the past is no longer available for report in the presence, this may be for three reasons. One is that it was never entered into the memory system at all; second, that it was in fact entered into the system but got lost during storage; and third, that it was entered, is still there, but cannot be retrieved.

The problem here is not that we know so much less than actually happened. The problem is rather that the sample of events of which we know can never be taken at random from the population of events that actually happened. Both top-down and bottom-up selectivity are ubiquitous and inevitable in historical studies and there is no way to escape from the biases inherent in them.

What can one do in a situation like this? Again, I cannot resist drawing on a psychological analogy: When a person suffers from a deep-seated unsolvable conflict, psychotherapy has two basic options. One is to suppress, or even repress, the unpleasant thoughts related to this conflict and to find a way to lead a decent life all the same. The other is to make the unpleasant thoughts explicit and give them a role in the client's life (which will then be somewhat less decent, at least for some time). When we translate these two options from psychotherapy to history, one is to forget about selectivity and tell stories as straight as possible. The other is to recognize selectivity and tell stories with this proviso. Any historical study has to choose its position somewhere between these alternatives.

Gluing facts together. Stories are, of course, much more than mere collections of certain facts, as my cartoon-like sketch has suggested so far. Rather, the point of a telling story is to make sense of certain facts by gluing them together in a particular way. Once more, there is a tricky relationship between making sense of the facts contained in a story and making sense of (and, hence, legitimizing) the story itself. The story makes sense (as a story) to the extent it shows that the facts make sense (as facts). How, then, can (stories about) events that happened in the past make sense?

With respect to this, the business of history is once more closely related to that of psychology. This is because both of these endeavors are (at least in large parts) concerned with explaining and evaluating human action and because both share (at least to some degree) a common conceptual framework for doing so. Much of this framework is provided by the beliefs and convictions shared by folk psychology (or, more specifically, by the wisdom of its Western-culture brand). Folk psychology provides a framework of basic semantic principles for understanding human action. The logic of folk psychology serves to glue facts together and makes stories coherent. Whether or not and how a fact gets integrated into a given story depends on whether or not and how it fits into the folk-psycho-logic of the story. Therefore, this logic acts as another constraint on possible stories and, hence, as another source of selectivity.

The semantics of folk-psychology is used in two major discourses: action explanation and action evaluation. As concerns action explanation, folk psychology offers two views: a subjective view that looks at the action as originating in, and caused by, the acting subject him/herself, and an objective view that looks at the action as caused by factors acting upon the actor, irrespective of whether or not they are mentally represented.

The subjective view makes use of the *logic of reasons*. This view explains the occurrence of certain actions in terms of certain mental states preceding them and, presumably, causing them. People perform certain actions for certain reasons. For instance, a person who is hungry may have the wish to get rid of this state, and he/she may believe that this wish can be satisfied by having a meal. This belief-and-desire-type of account is ubiquitous in action explanation in every-day discourse and much of both historical and psychological discourse still relies on it. Of course, when it comes to explaining more complex actions than having a meal like, for example, taking far-reaching political decisions etc., belief-and-desire explanations may assume much more complex forms. Still, the basic scheme is unaltered: The occurrence of a certain action is explained by (a complex chain of) mental antecedents. In such cases, the chain of antecedents will often take the form of a dialogue – be it internal within the actor him/herself or external between the actor and some of his/her contemporaries.

Conversely, the objective view makes use of the *logic of causes*. This view explains the occurrence of certain actions in terms of certain causes that either lie in the actors themselves or their environments. Causal action explanations can bypass, as it where, the actor's mental awareness. For instance, we may account for the fact that a person acts in a particular way in a particular situation by tracing this action back to a state or a trait we attribute to him/her (e.g., we think: my colleague did not say hello to me this morning, because he was in a bad mood – or because he is a reserved person anyway). Or we may attribute the occurrence of an action to external conditions (e.g., my colleague did not say hello to me, because his parents did not teach him adequate social behavior, or the like). In our everyday folk-psychology discourse these two forms of action explanation are mainly applied to the behavior of individuals. However, they can be, and in fact are, likewise applied to the behavior of collective agents such as governments, administrations, or corporations.

Evaluating facts. At first glance, the logic of causes seems to be entirely different from the logic of reasons. Unlike rational explanations that refer to mental states as causes, causal explanations refer to nonmental causes like states and traits in actors or their environments. However, when one turns from action explanation to action evaluation, it becomes apparent that the two views are not that much different. Their common ground becomes obvious when it comes to the discourse of evaluating actions in moral terms. For the discourse of evaluation it plays no major role whether we explain the action in terms of reasons or causes. In both cases we take it that the agent could have acted otherwise and is therefore responsible for the action. Hence, at least in the discourse of evaluation, folk psychology tends to believe that human agents are capable of exerting control not only over the network of reasons (of which they are aware anyway), but also over the network of causes of their actions (of which they are usually unaware). In a way, the discourse of action evaluation requires that causes be converted into reasons – in which format they are then entered into consciously controlled action decisions.

In sum, I submit that the logic inherent in folk psychology puts strong constraints on ways in which stories about past events can make sense. By saying that I do not mean to say that historical explanations are just psychological explanations. I'm far from claiming that folk psychology provides a sufficient framework for historical explanation. What I do claim, however, is that folk-psychology categories form a necessary constraint for historical explanations: There is no way of coming up with stories about past events that do not conform to the logic of reasons and causes for action explanation. Still, historical stories differ from psychological stories in several respects. For instance, psychological stories tend to be stories about the actions of individuals and their explanation in terms of reasons. Conversely, historical stories tend to be stories about the actions of collective agents and their explanation in terms of causes.

Ways of Sharing Sense

What does it mean *to tell a story* about events that happened in the past? How are the stories that one can tell constrained by the fact that they are communicated and addressed to certain audiences? How are our ways of making sense affected by the ways of sharing sense? Let me mention three of such constraints.

Syntax. Every storyteller knows that telling stories is a particular form of literature that requires a particular format and follows a particular syntax. Stories are more than just linear concatenations of facts that follow each other according to a temporal, rational, or causal scheme. Rather, their implicit syntax requires that their plot follows a basic scheme, requiring (more or less) stable states in the beginning and the end and culminating in (more or less) exciting happenings in between – thereby converting, as it were, the initial state into the end state.

There is yet another sense in which stories are more than just concatenation of facts in accordance with certain schemes. Good stories have a point and good storytellers have a way of communicating their story such that its point becomes apparent. The point of the story is what people still remember after they have forgotten most of the facts. Again, a story's point has two faces: It makes the causal and rational structure underlying major events in the story apparent in a new and interesting way and, at the same time, it thereby makes it obvious that this particular story about these particular events makes sense and is justified as a story. In a way, then, a story's point is the meeting point for the making and the sharing of sense.

Audiences. S/he who tells a story usually has a particular audience in mind, to which the story is addressed. Quite obviously, the storyteller's notion of his/her audience puts important constraints on the way the story is being told. The audience and the story form part of a particular discourse whose participants share some basic knowledge in the domain the story belongs to, some basic beliefs and expectations about major issues in that domain and, perhaps, some basic rules about the proper way of exchanging and discussing views about these issues. Therefore, when it comes to telling stories audiences are not accidental circumstances. Instead, they are constitutive facts in the sense that s/he who tells a story has no way of escaping and freeing him/herself from the story's audience – however implicit it may be.

This is true of both facts and fiction, but for stories about facts it has crucial implications. One is that it creates a dual commitment on the storyteller's part. On the one hand, s/he is committed to the known facts about past events. At the same time, however, s/he is committed to present and future audiences of the story in the making, and there is often no obvious and no easy way to convey the logic underlying human action in the past to a present-day audience, let alone unknown future audiences.

Another implication is that storytellers will always tend to pick one out of several possible audiences – even if they are not aware of it. Picking an audience is, in a way, equivalent to selecting the discourse of which the story in the making is supposed to form part. Hence, picking an audience determines to which beliefs and expectations the story will have to speak, which issues it will have to touch upon and which expectations it will have to fulfill. Stories about past events can obviously speak to a number of different audiences, such as the scientific community (in a narrow or a broad sense), the political community, communities discussing ethical, moral, or legal issues, or even the broad community of laymen with historical interests. The stories historians have to tell can be addressed to each of these communities, and each of them puts different constraints on the way these stories should be told.

Markets. Whenever a story about certain events in the past is born, it enters into a world of already-existing stories about the same, similar, or at least related events. In other words: it enters into a story market where, in the long run, only the fittest stories will survive. Yet, unlike living beings, stories do not compete with each other directly. Instead, what they compete for is attention and prominence in the discourse they are meant to form part of. In a way, these discourses and the mentality of their participants is, at any time, formed and shaped by the reception of a certain body of already existing texts and stories. This is the mental scenario the new story encounters and this scenario forms the market place in which any new story has to struggle for survival. Stories speak to certain other stories (and compete with them) by virtue of the fact that they speak to certain audiences that constitute themselves on the basis of certain texts and stories. This is what the logic of discourse amounts to: story audiences and story markets are two sides of the same coin.

Therefore, each discourse has ist own story market, and a given story's fitness on this market is determined by the rules and criteria that apply to that discourse. On each of these markets a number of factors will contribute to a story's survival, for example, how true it is, how realistic, rational, how straight, informative, instructive, how authentic, coherent, convincing, how enlightening, exciting, entertaining it is, etc. Though strictly scientific discourses should only be committed to truth, that is, the extent to which the story reflects happenings in the past, even these discourses are also committed to some of the other criteria, and this applies even more to most of the remaining discourses.

The Past and the Present

My conclusion, then, is that the business of telling stories about the past has two faces: reconstructing the past and constructing the present. My point here is not that these two faces exist – this is a commonplace notion. My point is rather that, contrary to commonplace wisdom, the commitment to the present is much stronger than that to the past, and that this applies to both the making and the sharing of sense. I realize, of course, that not many scholars of history will be prepared to accept this message. This may not be surprising in view of the fact that reflections about the past play an explicit role in their daily business, whereas constraints arising from the present are only implicit. Further, since deep-rooted ideology tells us that the present is constrained by the past, we cannot easily accept the notion that our understanding of the past should, in turn, be so much constrained by the present.

Like in the theory of therapeutic intervention, there are two options here. One is to stick to that ideological belief and keep on uncovering the truth about the past. This is what happens in analytic therapy. The other option is to regard the endeavor to uncover the truth about the past as an integral part of a complex psychodynamic process that takes place in the present. This is what happens in cognitive therapy. Scholars of history are in the uncomfortable position to find their way between the Scylla of the past and Charybdis of the presence.

> *Man may well ask the animal:*
> *Why do you not speak to me of your happiness*
> *But only look at me?*
> *The animal does want to answer and say:*
> *Because I always immediately forget what I wanted to say*
> –
> *But then it already forgot this answer and remained silent:*
> *So that man could only wonder.*[3]

Notes

1 Sonnabend, Geoffrey (1946) Obliscence: Theories of Forgetting and the Problem of Matter, Chicago / IL: Northwestern University Press (p. 16).
2 A beautiful recent example is provided by the opening statements in three major authoritative accounts of modern German history. "Am Anfang war Napoleon", "Im Anfang war das Reich" and "Im Anfang steht keine Revolution" – these are the very first sentences by which Thomas Nipperdey, Heinrich August Winkler and Hans-Ulrich Wehler speak to, and compete with each other in the way they open their respective accounts of German history in the past two hundred years (cf. Volker Ulrich's review of Winkler, H. A. [2000] Der lange Weg nach Westen. Bd. 1: Deutsche Geschichte vom Ende des Alten Reiches bis zum Untergang der Weimarer Republik, München: C.H. Beck, in "Die Zeit" 13 / 2000, which itself opens with the assertion: "Auf den ersten Satz kommt es an.")
3 Nietzsche, Friedrich ([1874] 1980). On the Advantage and Disadvantage of History for Life, Indianapolis / IN, Cambridge / MA: Hacket Publishing Company, Inc., (p. 8).

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Bibliometrics
Monitoring Emerging Fields

In 1958, a young man with a B.S. in chemistry from Columbia University borrowed US-$ 500 from Household Finance to produce an index to the current scientific literature in chemistry and the life sciences. It was Eugene Garfield, who at that time developed what we know today as Science Citation Index (SCI) or Web of Science. 40 years later Garfields company, the Philadelphia-based Institute for Scientific Information (ISI), employes 850 people with offices in 7 countries and sells a variety of library and information science products, indexing more than 8000 leading scientific journals in 35 languages. In 1992, ISI was acquired by Thomson Scientific, a subsidiary of The Thomson Corporation, a leading international business (annual revenues of US-$ 6 billion, common shares listed on stock exchanges). But the history of Garfields idea to set up an index of cited literature is not just a story of economic success (Cronin et al. 2000). Immediately after the SCI appeared on stage, scientists recognized it as a unique source for science studies, namely sociology and history of science. Derek John DeSolla Price was among the first, who discovered the potential of the SCI to give empirical insights into structures and developments of science (Price 1963). Although primarily produced as a tool for searching scientific articles, the SCI provides access also to aggregated data on disciplines, specialities, journals, institutions, countries and other entities. In fact during the past four decades the SCI together with its little 'sisters' SSCI (Social Sciences Citation Index) and A&HCI (Arts & Humanities Citation Index) became the major source for a new scientific field: bibliometrics.

A major product of bibliometric research are indicators, in most cases built from selected and aggregated counts of publications and citations. These indicators turned out to be important not only for studies in history and philosophy of science, but also for purposes of science policy and administration. Since 1972 the US National Science Foundation publishes biannual volumes of 'science indicators' (National Science Board 2000), including publication and citation statistics for international comparisons. Combined with other measures and peer review, bibliometric indicators can be used in the context of research evaluation. Bibliometricians have been heavily offended because of the political consequences which their indicators can have (MacRoberts 1989).

The question 'Which reality do we measure?' (Weingart et al. 1990) still needs to be answered as well.

Although the origin of the SCI is in the United States, there is much more bibliometric research activity in Europe than in the US. The largest group, headed by Anthony van Raan, is affiliated with the University of Leiden in the Netherlands. On the following pages van Raan and his collaborators present a lesson of what can be achieved with modern bibliometric methods – far beyond the pure number-counting of publications and citations. It is a valuable example for the application of sophisticated bibliometric methodology in exploring the interdisciplinary structures of new, unorthodox scientific fields. In fact it shows how such a field can be delineated and how emerging themes as well as the most important groups can be identified and analysed with bibliometric means.

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A Bibliometric Methodology for Exploring Interdisciplinary, 'Unorthodox' Fields of Science. A Case Study of Environmental Medicine

Anthony F.J. van Raan, Martijn S. Visser,
and Thed N. van Leeuwen

This article tackles the problem of how to explore a 'not well-defined' or 'unorthodox' field of science. Often, such fields are problem-oriented and interdisciplinary. 'Environmental medicine' is taken as an example, and used to explore two central questions: First, what are the most important groups, for example, in Europe and particularly in Germany, and how do they perform? Second, what themes are possibly emerging in this field of research? Before answering these questions, we have to ask what the field of environmental medicine look likes, how it can be defined, and how it can be 'delineated.' We present a first approach based on several bibliometric techniques, which can be regarded as part of our well-developed practice, in combination with some novel strategies.

First Approach: Definition of the Field on the Basis of Scientific Journals

How to Define 'Environmental Medicine'

The objective of this study is to answer two central questions concerning the interdisciplinary research field 'environmental medicine.' First, what are, worldwide, the most important and/or possibly emerging themes in this field? Second, how well are German research groups and institutes performing in this field, also in relation to possibly emerging themes?

Before we can answer these questions, we have to start by asking what the field of environmental medicine looks like, how it can be defined, and how it can be 'delineated.' This study presents a first approach based on a combination of several techniques that can be regarded as part of our well-developed CWTS practice, along with some novel strategies. Our approach can be seen as a general method for exploring 'unorthodox,' mostly interdisciplinary fields of science. Therefore, it contributes to a much-needed extension of analytical tools in the study of interdisciplinarity (cf. Weingart/Stehr 2000).

'*Umwelt-Medizin*' or 'Environmental Medicine' is not an established, well-categorized research field within the important international databases, neither in the multidisciplinary Science Citation Index nor in the widely used medical database MEDLINE. Therefore, we have to develop a method to define, or to delineate, this 'unorthodox' field as well as possible.

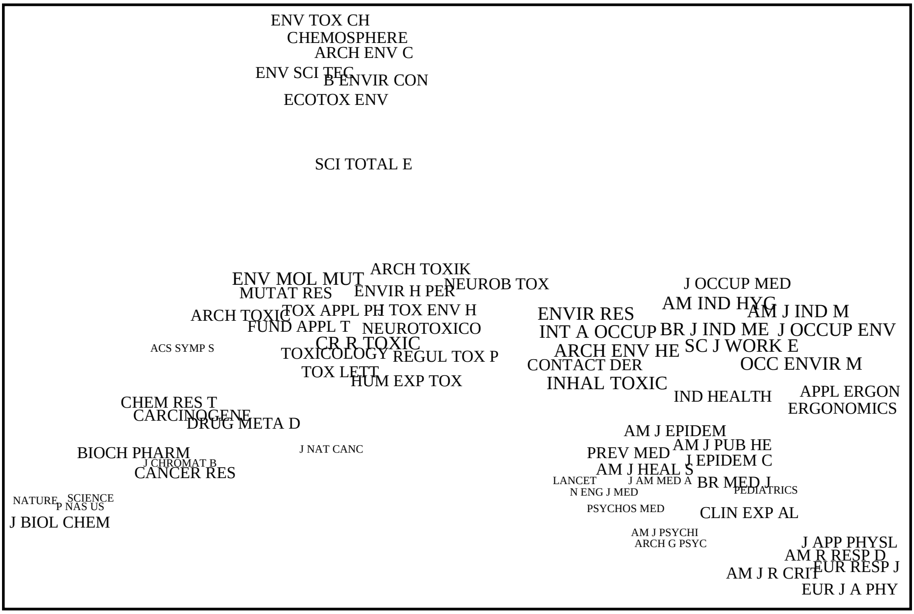
We took the following approach: On the basis of a first survey via *Internet* on environmental medicine (*Umwelt-Medizin*), we identified nine German research centers, mainly university institutes, in Aachen, Bochum, Düsseldorf, Gießen, Göttingen, Mannheim, Marburg, Munich, and Tübingen. We emphasize that this survey was certainly not intended to be exhaustive, and also should not have been, because the idea was to find 'starting points' via the Internet.

The next step was to collect publication data from these institutes. Most institutes make their publication lists over a longer time period (e.g., from 1995) available through their websites. This enabled us to identify the central *international journals* in the field of environmental medicine. Our first round was sufficient for this purpose. It was not necessary to collect a large number of relevant journals, because we developed a specific, iterative procedure to create a large set of environmental-medicine-relevant journals. In fact, five major journals formed the 'seeds' for an advanced journal-to-journal, citation-based analysis that ultimately generated a 'landscape' of about 70 journals grouped into several clusters. These 'seed-journals' are: *Environmental Health Perspectives*, *Journal of Toxicology and Environmental Health*, *International Archives of Occupational Health*, *Archives of Environmental Health*, and *Industrial Health*.

It should be noted that our Internet survey also found more nationally oriented, German-language journals and other periodicals. Although these national communication outlets are certainly important, particularly for daily practice, we did not consider them in this study, because our objective was to position European and particularly German groups on the international map of environmental medicine. We also noticed that several journals with an international status are published in the German language as well. The 'problem' with these journals, however, is that their articles are, on average, cited considerably less frequently in the international literature than those in English-language journals.

As mentioned above, we created a 'landscape' on the basis of *citation relations between journals*, starting with the five 'seed journals.' An extensive description of the journal-to-journal citation cluster analysis is given in Tijssen and van Raan (1994), together with other 'bibliometric mapping' methods. The more closely together journals were positioned on the map, the stronger their citation links. The results of our analysis are presented in Figure 1.

Figure 1: Bibliometric map of environmental medicine and related fields. This map is based on citation relations between journals (iteration procedure starting with 'seed journals' indicated by boxes; cf. main text).



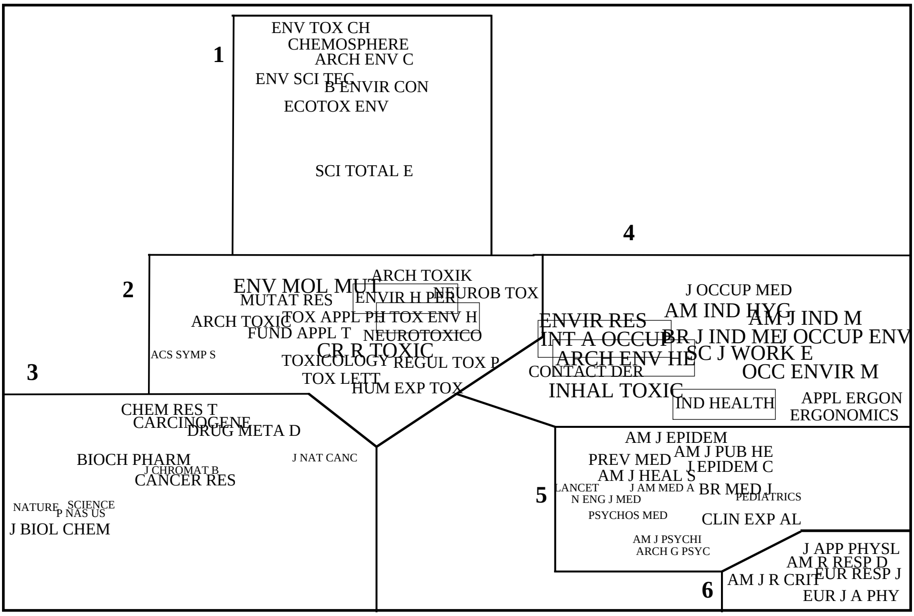
We view this landscape as a preliminary but good approximation of the research field 'environmental medicine.' Because this landscape was based on journal-to-journal citation relations, it contained only journals covered by the Science Citation Index. Therefore, it was not a 'perfect' representation of the field. Nonetheless, it certainly yielded a very useful *map* to guide the further steps in the analysis. SCI-covered journals represent the better and best international journals in most scientific fields, and thus SCI-covered journals form the 'hard core' of most natural science and medical fields.

As discussed above, the more nationally oriented (German-language) journals were not entered into our analysis because they were not covered adequately by the SCI. This made some subfields or specialties, particularly those with a typical national focus (and [parts of] the research groups concerned), 'invisible' on our map. Nonetheless, once scientific work had been published in the international, mostly SCI-covered literature, it would appear in our analysis.

The following *clusters* can be seen in Figure 1: Environmental toxicology and chemistry ('north' side of the map, 1), applied toxicology (center-left, 2), carcinogenesis research ('south-west', 3), environmental and in particular occupational health ('east', 4), epidemiology ('south-east', 5), and allergies and respiratory diseases ('far south east', 6). General journals such as *Nature*, the *Journal of Biological Chemistry* are included in the 'nearest' cluster. These clusters can be regarded as subfields of environmental medicine. Figure 2 is the same as Figure 1, but now these six clusters are indicated. Thus we have found a first thematic division of environmental medicine. We consider Clusters 1, 2, and 4 as the most central subfields, and the journals in these clusters as the *core journals of environmental medicine*. These journals, such as *Environmental Toxicology & Chemistry*, *Chemosphere*, *Archives of Environmental Contamination and Toxicology*, and *Environmental Science & Technology*, are given in Appendix 1.

These journals, however, also belong to 'already established' fields within the Science Citation Index. These fields (first 10) are environmental science, toxicology, public health, pharmacology and pharmaceutics, environmental engineering, allergy, dermatology, chemistry, genetics and heredity, and neurosciences. This clearly shows the 'interdisciplinary composition' or 'interdisciplinary profile' of environmental medicine. Later, we will compare the profile of the field *as a whole* ('the main stream profile') with that of outstanding groups or institutes within the field. Significant 'deviations' of these outstanding groups from the main stream may indicate important developments.

Figure 2: Bibliometric maps of environmental medicine and related fields. Same as Figure 1, now the six clusters as discussed in the main text are indicated.



The total number (worldwide) of core-journal publications in the period 1995–1998 was 24,714 of which 1,569 came from Germany. Table 1 shows the division of publications over the most active countries.

The German 'share' in environmental medicine research was 6.4 percent compared with 7.4 percent for science as a whole (1995). We conclude from these figures that Germany is internationally somewhat underrepresented in environmental medicine as far as our set of core journals defined above is concerned. Sweden, the Netherlands, and particularly Finland are quite 'over-active' in environmental medicine. However, we stress again that environmental medicine is mainly an applied research field. Therefore part of the German contribution will be in German-language journals that are *not* covered by the SCI, or in SCI-covered journals that do not belong to the clusters identified in Figure 1. A similar situation probably applies to France.

Table 1: Numbers of Environmental Medicine Publications 1995–1998

Country	Number	Percent
US	9,765	39.5
UK	1,673	6.8
Germany	1,569	6.4
Canada	1,469	5.9
Japan	1,147	4.6
Italy	861	3.5
Sweden	818	3.3
France	797	3.2
Netherlands	782	3.2
Spain	583	2.4
Finland	556	2.3
All other	4,694	19.0
Total	24,714	100.0

It is interesting to analyze which journals other than the core journals themselves frequently *cited* the core journals. These citing journals represent the 'direct periphery' of environmental medicine. Their names in conjunction with those of the above core journals reveal the *mainstream themes* of environmental medicine. Most of them addressed occupational / industrial / working environment and health, microbiology in relation to environmental contamination, and xenobiotica. Furthermore, we found several major environment-related themes within toxicology: eco-toxicology, genetic toxicology, neuro-toxicology, inhalation-related toxicology, and food toxicology; water-related themes such as aquatic toxicology and marine pollution; drug-related themes such as applied pharmacology, drug metabolism, and regulatory toxicology; plus a major allergy theme of contact dermatitis. Analytical chemistry proved to be very important as the 'instrumental' part of environmental medicine. The first 10 of these citing journals, such as *Water Environment Research*, *Analytical Chemistry*, *Drug Metabolism and Disposition*, and *Environmental Pollution* are given in Appendix 2. The list of these citing journals shows the strong links between the set of environmental medicine core journals and, above all, water-related problems, analytical methods, and cancer research.

Most Prominent European Research Groups

The approach described in the foregoing section provides us with a *journal-based* delineation of the field. This could be used as the basis for a further bibliometric analysis. Within the set of core journals as defined in the foregoing section, we identified the *most prominent research groups/institutes* on the basis of the articles published in 1995–1998 in all journals from the three clusters. We distinguished between two types of ‘prominence’ in this analysis: the most *active* groups in terms of number of publications and the most *influential* groups in terms of number of citations received (high ‘impact’). It has to be pointed out that the ‘most active’ groups are often the large institutes (and have a large publication output for this reason). Nonetheless, it is clear that very good research work can be done in smaller groups as well. Therefore, we decided that the best way to identify prominent groups was to look for the most influential groups; that is, groups with a high impact in the first place, *plus* a publication output above a specific threshold.

First, Table 2 presents the European (31) groups belonging to the 100 most active groups worldwide, ranked according to number of publications in 1995–1998. All these groups/institutes had 50 or more publications (P^3 50), hence, a minimum of circa 12 publications per year. In order to assess the scientific influence of these groups, we measured the ‘impact’ of each group or institute. This was done by counting all citations received by these 1995–1998 publications *from 1995 up till mid-1999*, and calculating the average number of citations per publication (*CPP*). This is our first impact indicator. The next section presents a detailed discussion of the methodology and a more extensive set of indicators. Within these 31 most publishing European groups (P^3 50), we selected the best 10 percent (in terms of high impact) by taking CPP^3 3.0. These groups/institutes are marked in bold.

We wish to emphasize that this exploratory study pinpointed groups/institutes by their main organization (e.g., university) only. It is possible that several groups within one university or large institute published in our set of core journals. Because this study regarded them as one ‘group,’ in such cases, we were actually dealing with all the environmental research activities of that university or large institute as a whole. A more detailed study would be necessary to focus on specific departments.

Table 2: European groups with the most publications.
Ranked according to numbers of publications 1995–1998

	P	CPP
Karolinska Institute Stockholm	*226*	*3.64*
University London	186	2.75
ETH Zurich	*154*	*7.14*
Finnish Institute of Occupational Health	146	2.83
University of Lund	*122*	*4.15*
RIVM, Bilthoven (Utrecht)	*116*	*3.23*
Agricultural University of Wageningen	*115*	*5.79*
University of Utrecht	*107*	*4.20*
University of Amsterdam	*94*	*3.33*
University of Milan	91	2.52
University of Birmingham	83	2.49
University of Umea	*81*	*3.41*
University of Uppsala	*78*	*3.27*
Zeneca, Macclesfield (UK)	*76*	*4.28*
Free University of Amsterdam	*74*	*3.86*
National Institute of Working Life, Stockholm	73	1.99
University of Leuven	*65*	*3.12*
University of Kuopio	63	1.87
University of Stockholm	*62*	*5.53*
University of Lancester	*59*	*5.85*
University of Helsinki	*58*	*3.48*
Natiotanl Public Health Institute, Kuopio	56	2.66
University of Munich	56	2.14
University of Lyons	*56*	*3.00*
University of Jyvaskyla	*55*	*3.49*
University of Göteborg	55	2.95
University of Odense	*53*	*4.64*
TNO Zeist (Utrecht)	53	1.91
University of Düsseldorf	52	2.44
University of Bayreuth	*50*	*6.82*
CSIC Barcelona	*50*	*6.18*

Three German universities were present in this list: Munich, Düsseldorf, and Bayreuth. Only Bayreuth was above the $CPP^{3}3.0$ threshold. The impact of Bayreuth was by far the largest, and in fact one of the highest on the list. Therefore, we may conclude already that this is a *prominent German research group* in environmental medicine, at least according to our definition of this field given above. We emphasize however that a more detailed assessment of research performance will be presented in later on in this article. It should also be noted that we did not find the Bayreuth group in our Internet survey. The reason for this will be discussed below.

Table 3 reports the 15 German groups with the most publications (groups marked in italics are among the European groups given in Table 1; i. e., groups with $P > 50$), again ranked according to number of publications in 1995–1998. *CPP* (1995-mid-1999) is also indicated. We have already noted the high impact of the University of Bayreuth.

Table 3: German groups with the most publications. Ranked according to numbers of publications 1995–1998

	P	CPP
University of Munich	56	2.14
University of Düsseldorf	52	2.44
University of Bayreuth	50	6.82
University of Würzburg	47	3.70
University of Mainz	46	2.67
University of Hamburg	43	2.58
Free University of Berlin	42	3.02
Free University of Berlin	42	3.02
University of Erlangen-Nuremberg	42	3.17
University of Tübingen	42	2.95
University of Göttingen	41	2.78
GSF München	41	2.58
University of Dortmund	38	3.26
BASF Ludwigshafen	32	1.61
University of Ulm	33	3.88
Fraunhofer Institute Schmallenberg	32	1.63

Very high-impact ($CPP \geq 10.0$) groups in Europe that are not reported in Table 2 because their number of publications was lower (i. e., $10 < P < 50$) are:

University of Granada	32	10.78
Brunel University	*21*	*35.43*
University of Helsinki	*12*	*10.08*

The *extremely high impact* of the group at Brunel University, Uxbridge, UK is immediately apparent. This can be explained only by some very frequently cited publications. The next section will discuss highly cited publications as indicators of 'hot topics,' and come back to the performance of the Brunel group.

We emphasize that the above figures are a *first indication* of research output and impact. A more detailed analysis of selected groups / institutes is presented in Section 3. We also emphasize that German research groups, in general, may score lower than, for example, UK groups because of the relatively low impact of German-language papers *in journals covered by the SCI*. This may have quite a dramatic influence on a SCI-based performance assessment of Germany compared with other countries (particularly the commotion around the article by the UK Chief Scientist Robert May in Science, May 1997). This will be discussed extensively in a forthcoming paper (cf. van Leeuwen / van Raan 2000).

We conclude from the above that our bibliometric analysis permits a preliminary identification of European groups or institutes that can be characterized as highly active and / or highly influential. Prominent European groups can act as 'benchmark' institutes for comparisons with German institutes. As indicated above, this is particularly the case for highly productive, high-impact groups such as at the Karolinska Institute in Stockholm, ETH Zurich, University of Lund, and the Agricultural University of Wageningen.

We stress that the numbers of publications given in Tables 2 and 3 may differ considerably from the numbers derived from publications lists in, for example, the annual research reports of the groups or institutes concerned. Our analysis considers only those publications that meet the following two selection criteria: (1) *general*, for example, only publications covered by the Science Citation Index and related indexes, as well as only publications of a special 'article type' (cf.

methodology discussion in the appendix), and (2) *specific*: a further selection by the set of journals described above.

Important Themes Identified on the Basis of Frequently Cited Publications

We applied a third bibliometric analysis to our journal-based definition of environmental medicine: most cited papers in the period 1995–1998. The importance of such an analysis is twofold. First, it reveals the groups / institutes with publications of the highest impact, which is an indication of the quality of the research groups concerned. Second, we consider the topics of these high-impact publications as important themes, *hot topics*. Not necessarily all of them will be breakthroughs or new developments (review publications with an extensive state of the art of a research field can also be cited very frequently!). In most cases, however, high-impact papers will be *nonmainstream* contributions.

On the basis of the titles of the top-100 most cited publications, List 1 presents a number of 'hot topics.' In the case of frequently cited *review* papers, however, it is mostly not a 'hot topic' but an important though 'classic' theme. Therefore, it is crucial to distinguish between types of articles in this analysis. Frequently cited review topics are given in italics.

List 1: Important research themes

Cytochrome-P450 Inhibitors
Estrogenic environmental pollutants
Male reproductive health and xeno-estrogens
Biodegradability and aging of chemicals
Endocrine disrupters
Phyto-estrogens and cancer
Estrogens and dentistry
Aquatic colloids
Sorption by soil models
Antrazine in surface water
Oxidative damage to DNA
Particulate / ultra-fine particle air pollution
Phytoremediation of contaminants

Metal-ion binding to humic substances
Organochlorine compounds and cancer
Harbor contaminants
Ion-trap mass-spectrometry
PCB's
Plants to remove heavy-metals from soils and aquatic streams
Apoptosis
Neuro-toxicity
Pesticides and breast cancer
EDTA in natural water
Mercury in coastal waters and rivers
Land-ocean interaction
Photo-catalytic degradation
Carcinogenicity of diesel exhaust
Fly-ash and acute lung injury

Many of the high-impact publications originated from US groups or institutes. The first 10 US groups within the set of *top-25-cited* publications were: Merck & Co.; Tufts University, Boston (in cooperation with the University of Granada); Connecticut Agricultural Experimental Station, New Haven; Texas A&M University; University of Florida (in cooperation with groups from Denmark, Finland, France, UK, and Tulane University in New Orleans); Cornell University; US Environmental Protection Agency (EPA, in cooperation with Procter & Gamble and the Agency of Toxic Substances and Diseases Registry in Atlanta); College of William & Mary, Williamsburg (in cooperation with several other US groups); University of Missouri; University of Rochester (in cooperation with Tulane University and the University of Florida).

When identifying the European groups contributing to the top-25 impact publications, then the position of Brunel University immediately strikes the eye. This university was involved in 6 of the top-25 publications. We already mentioned the very high impact of the Brunel group in the foregoing section. It is clear that this was based mainly on this remarkably high share of the top-25 cited publications.

Other European groups contributing to the top-25 were: University of Granada (we also mentioned its very high impact) in cooperation with Tufts University, Boston; Imperial Cancer Research Foundation, London, in cooperation with Brunel University; MAFF (UK) also in cooperation with Brunel University; National University Hospital in Copenhagen in cooperation with Brunel University, University of Florida, Tulane University, University of Turku (Finland), National Food Agency in Soborg (DK), University of Odense (DK), INSERM in Rennes (F), University of Paris V, MRC in Edinburgh; University of Helsinki (also mentioned earlier for its very high impact); RIZA in Lelystad (NL) together with two Dutch firms; University of Geneva; Rowett Research Institute (UK) in cooperation with the Institute of Preventive and Clinical Medicine, Bratislava, and the Czech Academy of Sciences.

Second Approach: Definition of the Field on the Basis of Institute Names

Why a Second Approach to Define the Field?

The definition used so far to identify groups and institutes in environmental medicine was based on a *set of core journals*. It is highly possible that these groups and institutes do not 'present' themselves with their institutional names as being 'environmental medicine research groups' (e.g., 'Institute for Environmental and Occupational Medicine'). They may be, for example, departments of epidemiology, departments of allergy research, or institutes of general environmental research.

On the other hand, many groups and institutes indicate specifically that they are working in environmental medicine through their *institutional names*. They 'advertise' themselves, as it were, as environmental research institutes. Given the interdisciplinary nature of the field, it is possible that these research groups publish (substantial parts of their work) in *other* journals than those used in Section 1 to define the field.

Therefore, we have to conclude that, alongside the journal-based definition of the field, we need a *second* definition based on institute names. The second analysis for identifying relevant research groups searched in the entire Science Citation Index (SCI) and Social Science Citation Index (SSCI) in the period 1995–1998 for all institutes or groups worldwide, with the following keywords (abbreviations) in their institute's name (in the address field of the publication record): 'environm…' (or 'Umw…') *or* 'occupat…' (or 'Arb…') *together* with 'med…' or 'hyg…'. For the SCI / SSCI, this analysis is possible only in our CWTS bibliometric data-system. To our knowledge, no other SCI / SSCI based system allows address keyword analysis.

The search yielded a large set of groups and institutes, 15,962 publications worldwide, of which 1,410 came from Germany. Thus the German share in the world total of environmental research defined on the basis of institutional names was 8.8 percent. This differed from the finding in Section 1 that revealed a German share of 6.4 percent with the journal-based definition of environmental medicine. Most of this difference was probably explained by the use of different journals.

Most Prominent European Research Groups

We used a frequency analysis to rank all European groups / institutes (German groups / institutes in italics) with an average of at least five publications per year, that is $P > 20$ (1995–1998), cf. Table 4. In contrast to the journal-based method, the probability of having several groups in one university or larger institute was small, because it would be unlikely to find groups within a university or large institution with similar names. Because we performed a detailed impact analysis on the results of a combination of the journal-based and the name-based methods, we shall present only publication numbers (output).

It would be interesting to see how the groups and institutes identified with the two methods differed; the one based on a selection of environmental medicine journals; the other, on the use of environmental medicine (or related terms) in the name of the group or institute. The most obvious way to do this would be to compare the lists of groups resulting from both analyses. Recall that the journal-based method may reveal more than one research group in a university or larger institute. These comparisons are discussed in the next section.

Again, we stress that the publication numbers given in the above tables may differ considerably from the numbers derived from publications lists in, for example, the annual research reports of the groups or institutes concerned due to general (cf. appendix) and specific (institute name) selection criteria.

Table 4: European groups with the most publications
Ranked according to numbers of publications 1995–1998

	P		**P**
Karolinska Inst. Stockholm	653	University of Padua	42
GSF München	*411*	*University of Freiburg*	*39*
University of Lund	181	*University of Tübingen*	*39*
University of Düsseldorf	*173*	*University of Erlangen-*	*36*
University of Linköping	142	*Nuremberg*	
University of Birmingham	133	University of Brescia	33
University of London,	86	*Technical University of*	*30*
Imperial College		*Munich*	
University of Aarhus	81	University of Pavia	30
University of Vienna	76	University of Newcastle	29
University of Göteborg	75	University of Wageningen	28
University of Glasgow	70	University of Montpellier	26
University of Umeå	67	University of Milan	25
University of Leuven	66	Finn.Inst.Occup.Health	24
University of Helsinki	63	University of Odense	24
University of Uppsala	60	Swedish University of	23
University of Aberdeen	57	Agricultural Science	22
University of Ulm	*51*	University of Bergen	
University of Bochum	*49*	*University of Aachen*	*21*
University of Göttingen	*47*	University of Florence	21
University of Amsterdam	43	*University of Hohenheim*	*20*
University of Essen	*42*	University of Verona	20

Comparison of First and Second Field Definition

Many of the universities and institutes identified with the name-based definition of environmental medicine had been found already with the journal-based definition. As discussed at the end of the last section, the journal-based definition is *broader* because it also includes groups and institutes that do not name themselves explicitly with environmental medicine. A comparison of both methods reveals that this is particularly the case in Germany for the University of Bayreuth. Another European example is groups / institutes at the ETH Zurich.

On the other hand, groups and institutes that use environmental medicine in their name (or related terms) may use other journals than those in our core set. To study this possible difference, App 3 presents the top-20 journals in 1995–1998 of all groups worldwide with environmental medicine or related terms in their name. It can be seen that more than one-half of these journals belonged to the core journal set used in the first definition of environmental medicine (App 1). This explains the considerable overlap of groups and institutes in environmental medicine found by both methods, as is clear from a comparison of Tables 1 and 2. Similarly, App 4 gives the top-10 journals for German groups with environmental medicine (or related terms) in their name. Here, the picture differed somewhat from the worldwide findings. Only 3 of these 10 journals belonged to the core journal set (see above). There were two German-language journals, one clearly within the field (*Zentralblatt für Hygiene und Umweltmedizin*) and another with a general medical scope (*Deutsche Medizinische Wochenschrift*). Two other journals were in English but devoted mainly to a German audience, and belonged to related fields (*Naunyn-Schmiedesberg Archives of Pharmacology* and *Fresenius Journal of Analytical Chemistry*).

Clearly, the German groups and institutes with environmental medicine and related terms in their name often use journals 'outside' the core journal set as defined above. Undoubtedly, the choice of German-language or primarily Germany-oriented (though English-language) journals plays a role here. It explains why both methods will reveal considerable differences in groups and institutes, as was the case with the University of Bayreuth.

Research Performance of Selected German and European Institutes

Research Impact

After identifying European and, in particular, German research groups in environmental medicine on the basis of two different methods, we performed a *standardized bibliometric performance analysis*. We applied our analysis to three selected German institutes/groups: one large organization, GSF München, and groups at two universities, Düsseldorf and Bayreuth. The same analysis was also applied to three selected European institutes: Karolinska Institute in Stockholm as a large (university-related) institution, and groups at two universities: in the Netherlands, Wageningen (agricultural university) and, again in Sweden, Göteborg. Publications were collected on the basis of both methods of field definition *combined*.

The core of our bibliometric approach can be described as follows:

Communication, that is, exchange of research results, is the driving force in science. Publications are not the only, but certainly very important elements in this knowledge-exchange process. High-quality work triggers reactions in fellow scientists. They provide the international forum, the 'invisible college' in which research results are discussed. In most cases, these fellow scientists perform their role as members of the invisible college by referring in their own work to the earlier work of other scientists. We all know that the process of citation is a complex one, and that it certainly does not provide an 'ideal' monitor of scientific performance. However, the same criticism holds for peer reviews as well (cf. Moxham/Anderson 1992). The application of citation analysis at a statistically low aggregation level (e.g., just one publication) is hardly meaningful in terms of performance assessment. However, application to the work of *a group as a whole over a longer period of time* does yield, in many situations, a strong indicator of scientific performance, and, in particular, of scientific quality given the correlation with peer review judgements (cf. Rinia et al. 1998). An important, absolutely necessary condition for the citation analysis is, nonetheless, that it be part of an advanced, technically highly developed bibliometric method.

Research output was defined as the number of articles from the institute found in the Science Citation Index (SCI), the Social Science Citation Index (SSCI), or the Arts & Humanities Citation Index (AHCI). We included the following publication types as 'articles': normal articles (including proceedings papers published in journals), letters, notes, and reviews (but not meeting abstracts, obituaries, corrections, editorials, etc.). We developed special software to calculate a set of standardized, basic indicators.

Table 5: Bibliometric Research Performance Indicators 1995–1998

Country; Institution of Group/Institute	P	C	CPP	CPP ex	Pnc	JCSm	FCSm	CPP/ JCSm	CPP/ FCSm	JCSm/ FCSm	% Self Cit.
GSF München	442	809	1,83	1,11	0,48	1,98	1,89	0,92	0,97	1,05	0,39
University of Bayreuth	51	279	5,47	3,57	0,24	2,57	2,06	2,13+	2,66+	1,25	0,35
University of Düsseldorf	131	372	2,84	1,81	0,35	3,15	3,09	0,90	0,92	1,02	0,36
Agricultural University of Wageningen	139	543	3,91	2,40	0,42	2,40	2,01	1,63+	1,94+	1,19	0,38
University of Göteborg	99	225	2,27	1,72	0,46	1,89	2,13	1,20	1,07	0,89	0,24
Karolinska Institute	609	3.737	6,14	4,86	0,34	3,44	3,46	1,78+	1,77+	0,99	0,21

The first column of Table 5 reports the number of papers published (*P*); the second column, the number of citations (*C*) for the time period 1995–1998. The analytic scheme is as follows: For papers published in 1995, citations were counted during the period 1995–1998; for 1996 papers, citations in 1996–1998; and so forth. There is ample empirical evidence that in the natural and life sciences – basic as well as applied – the average 'peak' in the number of citations is to be found in the third or fourth year after publication (Moed et al. 1995). Therefore, a 4-year analysis period is appropriate for impact assessment. The third indicator column reports the average number of citations per publication *(CPP*, calculated by dividing the total *P* of the entire time period by the total *C* in that period counted as reported above). The fourth column presents the same indicator, but now corrected for self-citations, *CPPex*. The fifth column contains the percentage of noncited papers, *Pnc*. It should be emphasized that this percentage of noncited papers covered, like all other indicators, the given time period (4 years). It is highly possible that publications not cited within such a relatively short time period will be cited after a longer period of time.

It is clear that these indicators are not very informative without reference values. How do we know whether a certain volume of citations or a certain citation per publication is low or high? Therefore, it is absolutely crucial to make a comparison with (or normalization to) a well-chosen international reference value, and to establish a reliable measure of *relative, internationally field-normalized impact*. Hence, the problem is to measure impact relative to an international average. We tackled this as follows: We calculated the average citation rate of all papers (worldwide) in the journals in which the institute had published (*JCSm*, the *m*ean *J*ournal *C*itation *S*core of the institute's 'journal set'). Thus, this indicator *JCSm* (sixth column) defined a worldwide reference level for the citation rate of the institute. It was calculated in the same way as *CPP*, but now for all publications in a set of journals instead of all publications of an institute. Details on these calculations are reported in van Raan (1996). By comparing these two indicators, we were able to assess whether the measured impact was *above* or *below* international average. A novel and unique aspect of our comparison with a worldwide reference value was that it took into account not only the type of paper (e. g., normal article, review) *but also* the specific years in which the papers were published. This is absolutely necessary, because the average impact of journals may reveal considerable annual fluctuations and large differences per article type (cf. Moed / van Leeuwen 1995, 1996).

The comparison of the institute's citation rate *(CPP)* with the average citation rate of its journal set (*JCSm*) introduced a specific problem related to journal status. For instance, if one institute publishes in prestigious (high impact) journals and another institute in rather mediocre journals, the citation rate of articles published by both groups may be equal *relative to* the average citation rate of their respective journal sets, even though the first group evidently performs better than the second. Therefore, we developed a second international reference level, a *field*-based world average *FCSm* (seventh column of Table 5). This indicator is based on the citation rate of *all* papers (worldwide) published in *all* journals of the field(s) in which the institute is active and not just the journals in which the institute's researchers publish their papers. Here, we used the definition of fields based on a classification of scientific journals into *categories* developed by ISI. Although this classification is far from perfect, it is currently the only classification available to us in terms of an automated procedure within our data system. We used the same procedure as that applied in the calculation of *JCSm* (cf. van Raan 1996).

Often, an institute is active in more than one field (i. e., journal category). In such cases, we calculated a weighted average value, the weights being determined by the total number of papers published by the institute in each field. For instance, when an institute published in journals belonging to the ISI category 'Environmental research' *and* in journals belonging to the category 'Toxicology,' then the *FCSm* of this institute would be based on both field averages. Thus, the *FCSm* indicator represents a *world average* in a specific (combination of) field(s). About 80 percent of all SCI-covered papers were authored by scientists from the United States, Western Europe, Japan, Canada, and Australia. Therefore, our 'world average' was dominated by the Western world. Again, we observed a general increase of *FCSm* values.

Because worldwide citation rates are increasing, it is essential to normalize the measured impact of an institute (*CPP*) to international reference values. Therefore, we calculated the ratio of *CPP* to the world averages discussed above, *JCSm* and *FCSm*. These ratios are presented in the 8th and 9th columns of Table 5. When the ratio *CPP / JCSm* was above 1.0, the impact of the institute's papers exceeded the journal-based (i.e., the journals used by the group / institute) world average.

A particularly powerful indicator is *CPP / FCSm*. This 'crown' indicator relates the measured impact of a research group or institute to a worldwide, field-specific (i.e., all journals in a field) reference value. It is the *internationally standardized impact indicator*. This indicator enables us to observe immediately whether the performance of a research group or institute is significantly far below (indicator value < 0.5), below (indicator value 0.5–0.8), around (0.8–1.2), above (1.2–2.0), or far above (>2.0) the international (western-world-dominated) impact standard of the field. As shown in Table 1, Wageningen and, in particular, Bayreuth had a very high performance. The other groups / institutes, GSF München, Düsseldorf, and Göteborg performed around world average. We have to emphasize that the extended research performance analysis presented in this section addressed a restricted number of selected groups / institutes, and not all the groups / institutes identified in this study.

An important issue is the level of aggregation or size of the institutions. It is clear that the larger the group or institute, the more difficult it is to maintain a high average performance, because there will often be subunits with lower performance. Therefore, the larger an institute, the more performance will tend to lower average values. In these cases, it is better – and even preferable – to conduct the bibliometric research performance analysis on the level of the smaller subunit as well. Table 5 should also be examined in this light. There were differences in size of about one order of magnitude! For instance, Bayreuth had about 50 publications in the given time period, but GSF München around 400 and Karolinska around 600. Particularly in the latter case, we can speak of an exceptional performance, given the score on the *CPP / FCSm* indicator and the size of the institute. Examples of middle-sized groups / institutes are Düsseldorf, Wageningen, and Göteborg.

The ratio *JCSm / FCSm* (10th column) is the institute's 'journal status' indicator. When it was above 1.0, the mean citation score of the institute's journal set exceeded the mean citation score of all papers published in the field(s) to which the journals belonged. In other words, the institute published in the higher impact journals of the field. This preference for publication in the higher impact journals was particularly strong in Bayreuth and Wageningen.

Research Profiles and Interdisciplinarity

A further important part of our bibliometric analysis was to *break down* the institute's or group's output (publications) into research fields.[1] This 'spectral analysis' of the output is based on the simple fact that researchers generally publish their work in journals belonging to more than just one research field. E.g., researchers at an immunology research institute will publish mainly in the typical immunology journals, but also in journals classified to oncology, haematology, and so forth. In this example, publications in immunology journals will form the largest group, and, consequently, this field will be the largest one in the research profile. Because we ranked fields in the profile according to their size (in terms of numbers of publications), the field immunology would be positioned as number one at the top of the profile. A specific immunology group may have 'genetics' and 'neurosciences' as second and third field in its profile. For another immunology group, 'oncology' and 'dermatology' may take these positions. So this breakdown of the institute's or group's output into research fields provides a clear impression of all the fields involved in the research activities of the institute or group. In other words, it provides us with information about its interdisciplinarity (cf. van Raan 2000), and therefore we can also call such a research profile its 'cognitive orientation.'

Not only size (number of publications) was given in the profiles. We also determined the indicator *CPP/FCSm* of the articles in these different fields (with international field normalization always to the specific field!), so that the fields within which the interdisciplinary research profile of the institute or group reveals a high (or lower) performance became visible. In our example above, this could mean that we would find that the first immunology group was very strong not only in its 'core' field of immunology but also in neurosciences.

As discussed above, a research profile analysis can also be applied to the field of environmental medicine *as a whole* and can be considered as a characterization of the 'mainstream.' This profile is given in Figure 3. It is based on about 25,000 publications from 1995–1998. It becomes apparent immediately that the field of *environmental science* is the most important in environmental medicine, both in output as well as in impact, followed by toxicology, public health, and pharmacology. Generally, in environmental medicine as a whole, public health publications have a low impact. Figures 4–7 represent the profiles of Düsseldorf, Bayreuth, Wageningen, and Karolinska.

Figure 3: Environmental Medicine Research Profile: 1995–1998

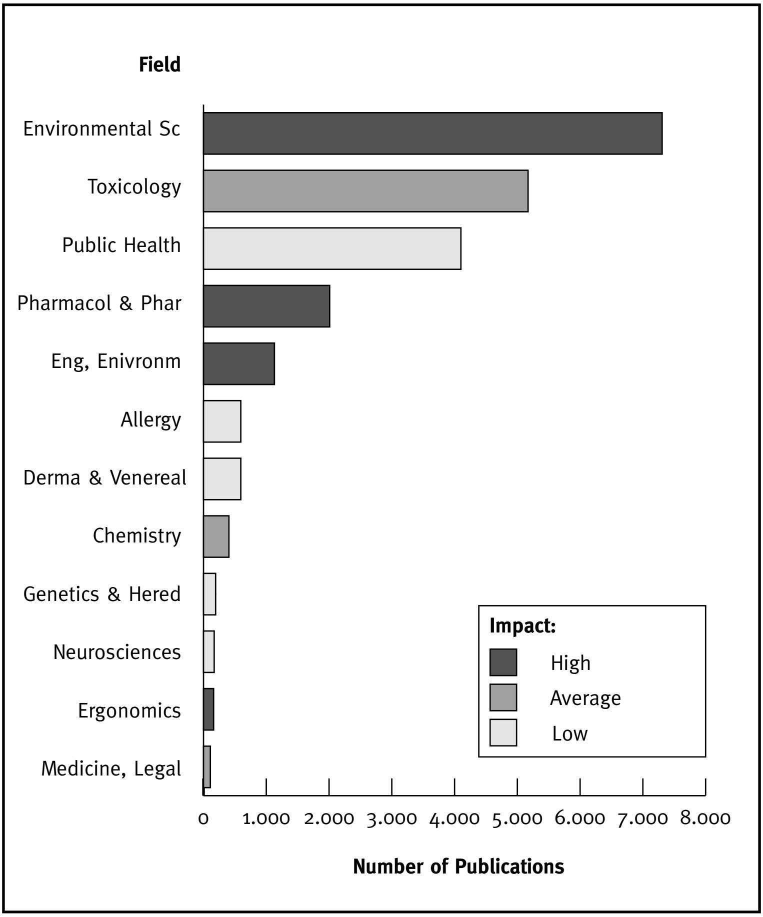


Figure 4: University of Düsseldorf Research Profile: 1995–1998

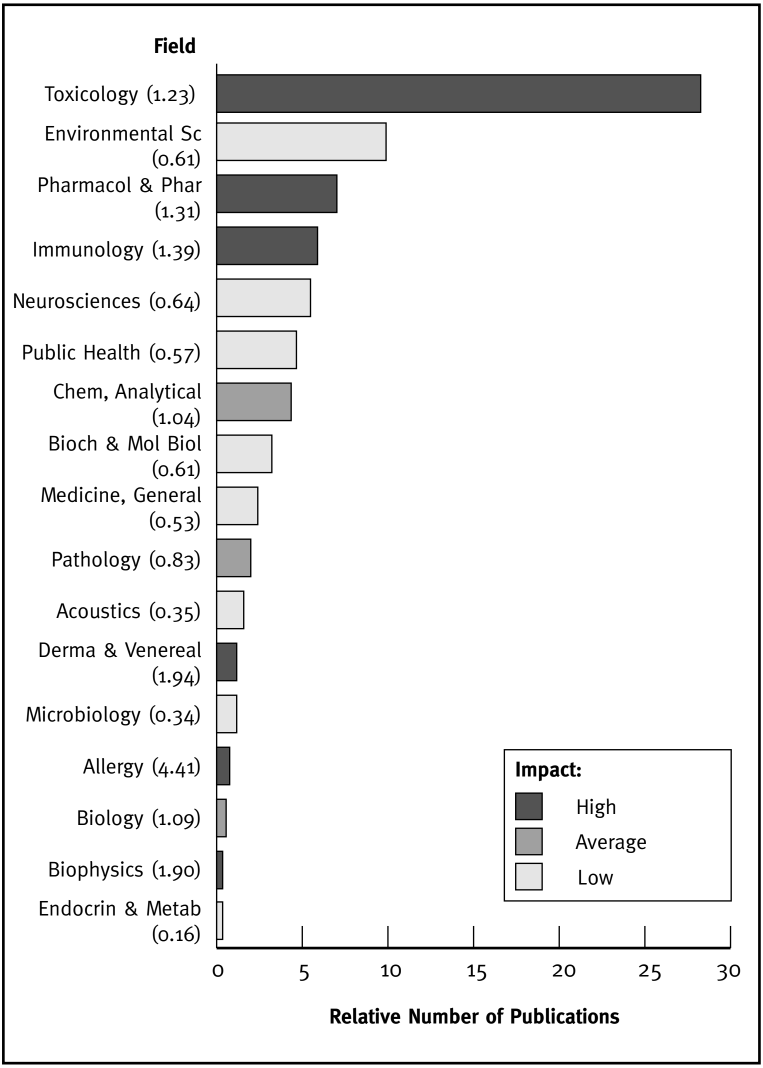


Figure 5: University of Bayreuth Research Profile: 1995–1998

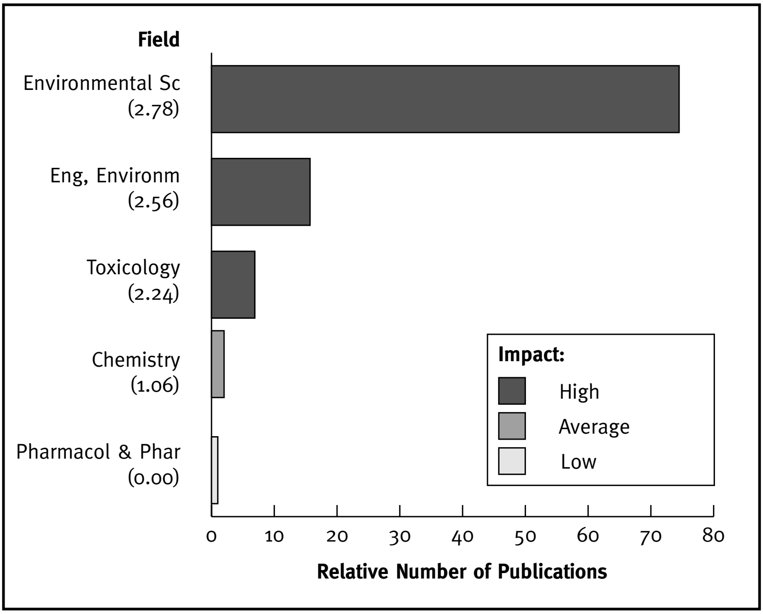


Figure 6: University of Wageningen Research Profile: 1995–1998

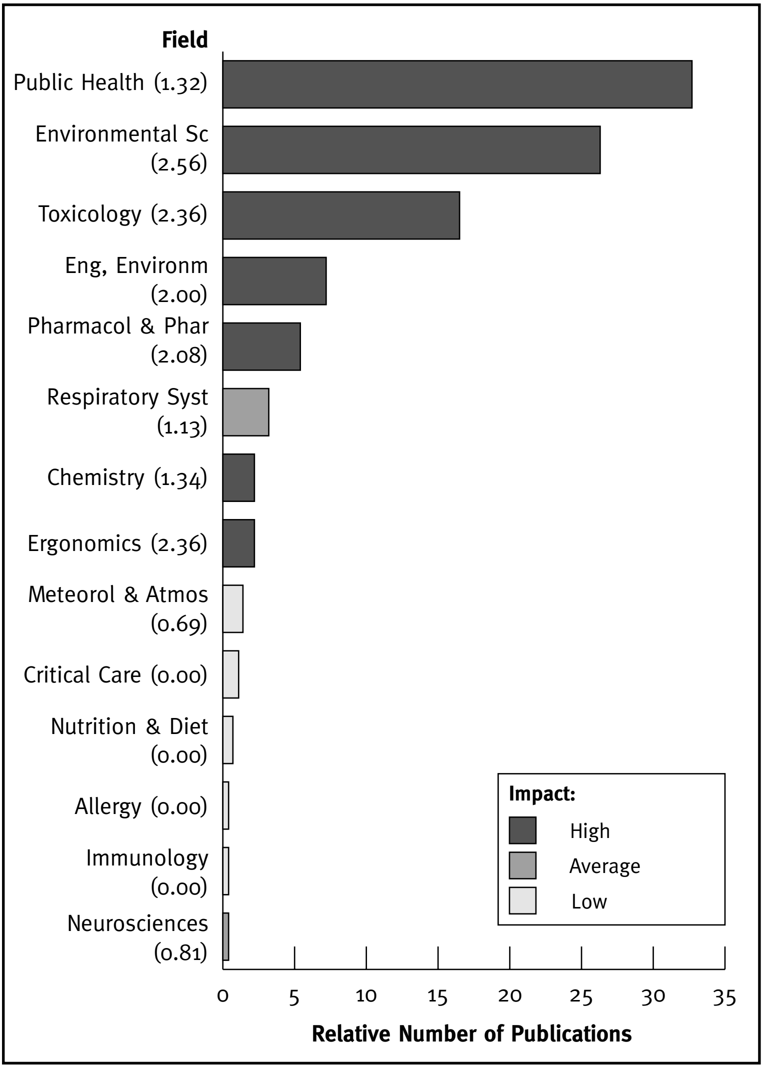
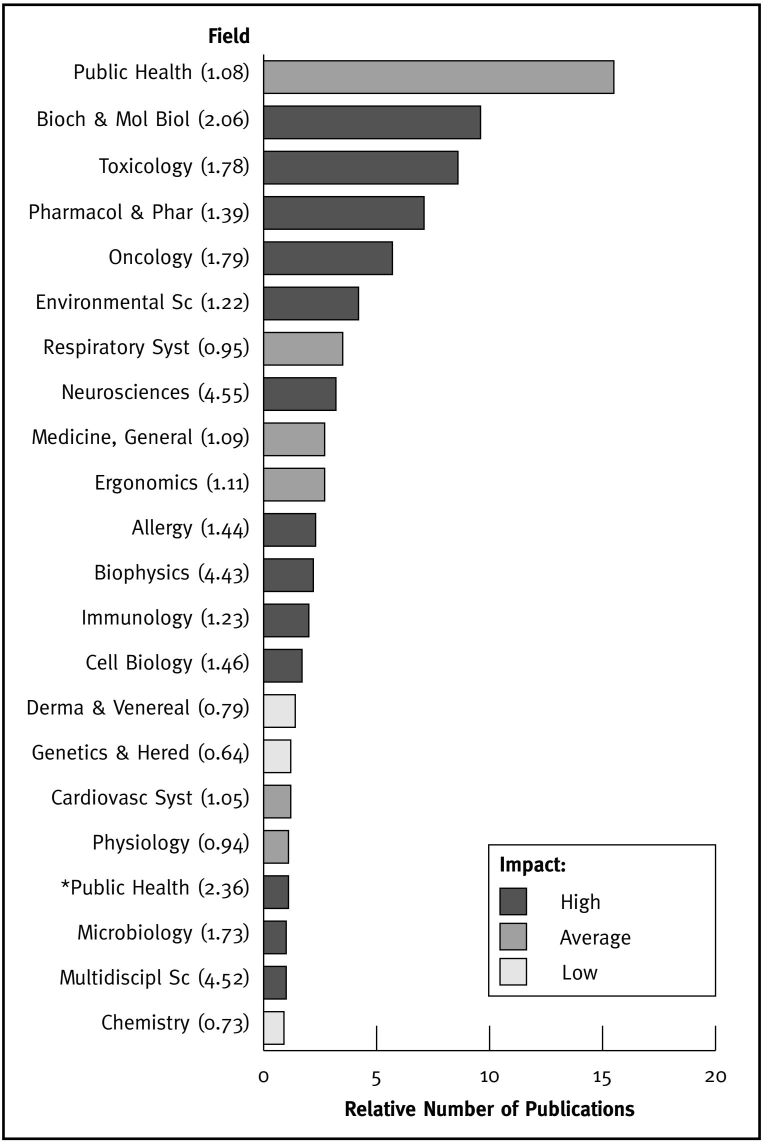


Figure 7: Karolinska Institute Research Profile: 1995–1998



These profiles once again reveal that environmental science, toxicology, and public health were the most important 'component parts' of environmental medicine. However, interesting differences between the various groups and institutes also emerged. Particularly significant 'deviations' in the profile of outstanding groups from the mainstream profile (Figure 3) may indicate important developments.

The *Düsseldorf* group (Figure 4) has a good (above international average) performance in its major field of output, toxicology. Its typical environmental science work showed a lower impact. Work in pharmacology and pharmaceutics, as well as in immunology was above international level. The profile shows that Düsseldorf was characterized by considerably more neuroscience-related activities (in terms of publication output) compared with the mainstream. The international impact of the neuroscience work was, however, lower than in that of other fields.

Bayreuth (Figure 5) was a relatively small group (in terms of number of publications) and therefore its profile was rather narrow. As already noted above, this group showed a very good performance, particularly in its major field, environmental science. *Wageningen* (Figure 6) clearly showed a strong profile. Its most important fields were public health, environmental science, and toxicology – all with a high to very high impact. In particular, the public health work at Wageningen was much stronger than in the mainstream of environmental medicine (Figure 3).

The *Karolinska* Institute in Stockholm (Figure 7) showed a strong and also very broad profile, which was to be expected given its very large size (particularly in terms of publications). The largest field was, similar to Wageningen, public health, with an impact around international level. The most striking observation for Karolinska, however, concerned its neuroscience work. This field took a much more prominent place in the institute's profile compared with the mainstream. But even more important was the extremely high impact of its neuroscience publications. This finding was a strong indication that neuroscience-related research is a theme of growing importance and, most of all, scientific influence in environmental medicine. As already noted above, a large institution such as Karolinska should be split up into different departments. In such cases, it is most appropriate to conduct a more extensive research performance measurement, as we do on a regular basis at our Center.[2]

International Scientific Cooperation

A further part of the analysis was to break down research performance into *types of cooperation*. We distinguished between articles originating from the group or institute only ('no cooperation'), from the group or institute with another group in the same country ('national cooperation'), and from the group or institute with a group outside its own country ('international cooperation). Results are reported in Table 4 for the groups at Düsseldorf, Bayreuth, Karolinska, and Wageningen.

Table 6: Bibliometric scientific cooperation data, institutes active in environmental medicine, 1995–1998

Country; Institution of Group/Institute	P	C	CPP	CPP ex	%	Pnc	CPP/ JCSm	CPP/ FCSm	JCSm FCSm	JCSm/ FCSm	% Self Cit.
University of Düsseldorf											
Institute only	50	133	2.66	1.70	0.36	2.43	2.78	1.10	0.96	0.87	0.36
National	60	135	2.25	1.43	0.37	2.70	2.80	0.83	0.80	0.96	0.36
International	21	104	4.95	3.14	0.29	6.13	4.67	0.81	1.06	1.31	0.37
University of Bayreuth											
Institute only	27	172	6.37	4.22	0.15	2.91	2.09	2.19	3.05	1.40	0.34
National	9	41	4.56	3.22	0.22	1.86	2.12	2.45	2.15	0.87	0.29
International	15	66	4.40	2.60	0.40	2.39	1.98	1.84	2.23	1.21	0.41
Karolinska Institute											
Institute only	114	859	7.54	6.27	0.25	3.68	4.02	2.05	1.88	0.91	0.17
National	250	842	3.37	2.37	0.41	2.94	3.03	1.14	1.11	0.97	0.30
International	245	2,036	8.31	6.74	0.31	3.84	3.63	2.17	2.29	1.06	0.19
Agricaltural University of Wageningen											
Institute only	29	51	1.76	0.93	0.48	1.70	1.36	1.03	1.29	1.25	0.47
National	57	150	2.63	1.56	0.47	2.06	2.06	1.28	1.28	1.00	0.41
International	53	342	6.45	4.11	0.32	3.14	2.32	2.05	2.79	1.36	0.36

A general phenomenon was that publications involving international cooperation revealed a higher impact than publications from the group or institute only, or in national collaboration (cf. Narin/Withlow 1990). Indeed, for Düsseldorf, publications based on international cooperation attained a relatively high impact. It was strikingly visible with the indicators *JCSm* and *FCSm* that the journals (and the fields) involved in international cooperation had a considerably higher level of impact. For Wageningen, the international publications showed a very high impact. For the Karolinska Institute in Stockholm as well, international co-publications were the ones with the highest impact. Remarkably, this was not the case for Bayreuth. Here the publications of the group 'on its own' showed the highest impact, at an excellent level. This is often proof of a strong and very successful focus on the development of an own, important specialty.

Concluding Remarks

We have tackled the problem of what the field of environmental medicine looks like, how it can be defined, and how it can be delineated by combining two approaches based on bibliometric methods, that is, methods exploring in an advanced way data originating from the scientific literature. The first approach is based on identifying the most important international journals for publications in environmental medicine. The second one is based on identifying institutes with names in which environmental medicine and directly related fields are mentioned. This procedure appears to be successful: Once the field is defined, we are able to analyze it and to discover its main characteristics and, in particular, its main 'players.'

Environmental medicine appears to be a typical interdisciplinary field, 'composed' of quite a broad spectrum of established fields such as environmental science, toxicology, public health, pharmacology and pharmaceutics, environmental engineering, allergy, dermatology, chemistry, genetics and heredity, and neurosciences. Looking at the level of activity in different countries, we find that German activity in environmental medicine is comparable to the average German share in science as a whole. Hence, there is no strong 'over-activity' or 'under-activity' of Germany in environmental medicine. In contrast, countries such as Sweden, the Netherlands, and Finland are significantly 'overactive' in environmental medicine. We stress, however, that the larger a country, the less it will show typical 'over'- or 'under-activity,' because activities in most fields tend increasingly toward average values.

Our analysis reveals the most prominent European groups and institutes, both in terms of publication output as well as scientific influence, measured in terms of their 'impact' revealed by bibliometric performance analysis. This identification of most prominent groups and institutes is a crucial part of the study: First, it allows us to find 'benchmark' groups or institutes; and second, it allows us to find significant 'deviations' from the 'mainstream' in the research themes of excellent groups. Benchmark groups or institutes are important for comparisons with German groups. Because these benchmarks are outstanding groups, they can be drawn on as examples when considering how a specific group or institute could be restructured or reorganized.

The identification of research themes that deviate considerably from the mainstream is essential for monitoring important, and possibly emerging 'hot' research topics. As indicated above, environmental medicine is an interdisciplinary field composed of many different basic fields. The interesting point here is that our bibliometric methods allow us to establish what basic fields are the most important 'components' of environmental medicine as a whole. We have mentioned these fields already, for instance, toxicology, public health, and allergy. This may be called the 'research profile' of environmental medicine mainstream research. Using the same analytical instrument, we can also construct a research profile for each of the most prominent European group or institutes, and see whether a group's research profile deviates significantly from that of the mainstream. For the Karolinska Institute, we find that neuroscience research with very high impact is part of the environmental medicine research profile.

Another 'deviation' from the mainstream is given by very frequently cited publications. These are generally the publications that attract exceptional attention in the research community. Therefore, careful identification of, for instance, the 100 most cited publications in environmental medicine is an interesting method for ascertaining which topics and themes are regarded as very important.

We have not investigated the performance of all European groups in an extensive way, because this would be far beyond the scope of the present study. We have compared research performance in a few selected groups / institutes with a field-specific international standard impact level and focused on performance in more detail through research profiles.

Finally, we have investigated scientific cooperation. A general phenomenon is that publications based on international cooperation show a considerably higher impact than publications from the group or institute 'alone' or in national collaboration. Remarkably, this is not the case for Bayreuth. Here the publications of the group 'on its own' show the highest impact at an excellent level. This is often proof of a strong and very successful focus on the development of one's own, important specialty. Once again, we have to emphasize that these findings are the outcome of a still preliminary survey. In particular, conclusions on research performance must be supported by further findings from more detailed studies.

Notes

1 In research profiles, fields were defined on the basis of standardized sets of journals; discussed on p. 105.
2 Cf., for example, van Leeuwen et al. 1996, available via our website http://www.cwts.leidenuniv.nl

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Appendices

Appendix 1: Core journals of environmental medicine
Environmental Toxicology and Chemistry
Chemosphere
Archives of Environmental Contamination and Toxicology
Environmental Science & Technology
Bulletin of Environmental Contamination and Toxicology
Ecotoxicology and Environmental Science
Science of the Total Environment
Archiv für Toxikologie
Environmental and Molecular Mutagenesis
Mutation Research – Fundamental / Genetic Toxicology
Environmental Health Perspectives
Neurobehavioral Toxicology and Teratology
Toxicology and Applied Pharmacology
Archives of Toxicology
Journal of Toxicology and Environmental Health A
Fundamental and Applied Toxicology
Neurotoxicology
Critical Reviews in Toxicology
Toxicology
Regulatory Toxicology and Pharmacology
Toxicology Letters
Human & Experimental Toxicology
Journal of Occupational Medicine
American Industrial Hygiene Association Journal
Environmental Research
American Journal of Industrial Medicine
International Archives of Occupational and Environmental Health
British Journal of Industrial Medicine
Journal of Occupational and Environmental Medicine
Scandinavian Journal of Work Environment & Health
Archives of Environmental Health
Contact Dermatitis
Inhalation Toxicology
Occupational and Environmental Medicine
Industrial Health

Appendix 2: Journals that frequently cite environmental medicine core journals (1995–1998)
Water Environment Research
Analytical Chemistry
Drug Metabolism and Disposition
Environmental Pollution
Carcinogenesis
Journal of Chromatography A
Ecotoxicology and Environmental Safety
Environmental and Molecular Mutagenesis
Atmospheric Environment
Toxicological Sciences

Appendix 3: Top-20 journals for all groups worldwide (1995–1998) with environmental medicine or related terms in their name
Environmental Health Perspectives
American Journal of Industrial Medicine
Occupational and Environmental Medicine
Journal of Occupational and Environmental Medicine
Toxicology and Applied Pharmacology
International Archives of Occupational and Environmental Health
American Journal of Respiratory and Critical Care Medicine
FASEB Journal
Carcinogenesis
American Journal of Occupational Therapy
American Industrial Hygiene Association Journal
Journal of Applied Physiology
Scandinavian Journal of Work Environment & Health
American Journal of Epidemiology
Toxicology
Toxicology Letters
Chemosphere
Archives of Environmental Health
American Journal of Physiology-Lung Cellular and Molecular Physiology
Science of the Total Environment

Appendix 4: Top-10 journals in 1995–1998 for German groups with environmental medicine (or related terms) in their name

Naunyn-Schmiedesberg Archives of Pharmacology
Zentralblatt für Hygiene und Umweltmedizin
Int. Archives of Occupational and Environmental Health
Chemosphere
Toxicology Letters
Fresenius Journal of Analytical Chemistry
Radiation and Environmental Biophysics
Deutsche Medizinische Wochenschrift
Stem Cells
Environmental Health Perspectives

Science Policy

Making Universities Cope with Science Today

Universities are the basic building blocks of the science system in most industrialized nations. Nearly all scientists have been educated in universities, and in many countries universities form the largest sector within the national research system. The integration of higher education and scientific research into one academic institution determines the hybrid character of the university. In the past, numerous studies have focused on different aspects of this most influential type of institution in the academic world. Each year some 200 new articles on universities get published in international top journals as covered by the Science Citation Index or Social Sciences Citation Index.

As science itself is undergoing rapid and far-reaching changes (UNESCO 2000), especially during the last two decades, these changes have a strong impact upon the universities. After a period of dramatic quantitative expansion until the mid-1970s, budget restrictions in many nations limited the growth curves and led to a *steady state* (Ziman 1994) in the 1980s and 1990s. Now there is no more additional funding to realize new developments; any new initiative has to be paid through internal cutbacks.

Whether or not the terms 'mode 1' vs. 'mode 2' characterize discrete forms of knowledge production and, what is more, whether or not 'mode 2' will eventually replace 'mode 1,' that is, the former thus charactizes a historical change in the science system (cf. Gibbons et al. 1994, and pp. 13–14, 130 in this volume) – this may be debated controversely (Weingart 1997). It is obvious, however, that universities will undergo severe structural and organizational transitions throughout the next decades. As Wilhelm Krull points out on the following pages, a number of critical issues are affecting the future development of the universities: there will be less state but more private funding; due to the possibilities of the world wide web and multimedia technology there is a significant trend towards virtual colleges; traditional disciplinary specialization will decrease while inter- and transdisciplinarity will increase; funding will be linked closely to assessments of performance, and indicators for 'outputs' of research and teaching will get more and more attention (Weingart 1996); in the era of globalized markets internationalization will be of growing importance.

Universities will have to change themselves into learning organizations; academic 'self-mystification' will be reduced while controlling and management procedures will be introduced on all levels to enhance the productivity of the organization and its members.

With Germany as an example, Krull explains the most important dimensions of change – in fact, the challenges – for universities today; his observations, however, are of relevance far beyond the German case.

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German Universities on the Threshold of the Twenty-First Century[1, 2]

Wilhelm Krull

Major changes in the calendar such as new millennia are also an occasion for looking back and taking stock, engaging in a critical or fond inspection of what has been achieved so far, but, above all – as a glance at the world in 1900 or even in 1000 would show us –, an occasion for speculations and visions, for promising utopian scenarios as well as prophecies of catastrophe and doom. Things are no different at the threshold to the 21st century or the third millennium.

Nothing ages so rapidly as long-term predictions, and I take to heart Peter Medawar's comment that everybody cultivates expectations regarding the future but only fools allow themselves to make predictions. Therefore, I fear that I shall have to disappoint all those who finally wanted to know which catastrophes await us and what German universities will look like in 50 or even 100 years time (should they still exist). Whatever, there has been no shortage of predictions of gloom and negative trends in the German higher education policy debates of recent years.

As far as German universities are concerned, critical reports and visions of doom were on the agenda long before the appearance of the current millennium. Diagnoses on their state of health have also long been bad. Even 10 years ago, Jürgen Mittelstraß, a philosopher at Constance, coined the metaphor of the "university as patient." He considered this patient to be suffering from "being overcrowded and underfinanced," to be struck down, and that neither science nor politics would seem capable of developing convincing proposals for treatment, let alone providing effective help: "The patient's coma has long since spread to the physicians as well" (Mittelstraß 1993: 27, translated).

Even a quick glance at the headlines on university policy in recent months reveals little change, at least in the public perception of the situation. Not only the unfortunate discussion on "lazybones professors," but also headlines such as "Stupidity: Higher education policy doesn't know what to do," "A desert in cultural policy?" "Universities: The major revival has failed to materialize," "German universities: Not good enough for Nobel prizes?" or "Musty gowns: Professors should be paid according to their productivity" have further damaged the image of German universities. This has been particularly encouraging to those powers that have long considered that the critically ill patient needs drastic surgery, to some extent, to be put under the knife, and thus ensure a return to health through intervention and external regulation. Particularly critical observers, most of whom prefer a shipping metaphor, are already seeing the approaching death of many universities: The ship is sinking, the "university for the masses" tanker is foundering, but what then? This is as far as the view of the pessimists goes!

Optimistic observers of the situation and policymakers, in contrast, point out that the current state of German universities is, to a major extent, the outcome of the administered university world of the 1970s and 1980s, in other words, of a period in which the many self-appointed healers of the patient university had almost driven it to its death. They argue that belief in the self-healing powers of the universities should not be abandoned. They do not deny that it will be very difficult to create the preconditions for an effective growth of self-healing powers in light of the continuing diverse ties to politics and the accompanying lack of clarity in the allocation of responsibility. However, this will be essential if the universities are to act as self-determined institutions. Granting autonomy simultaneously implies a clear assignment of responsibilities.

The Volkswagen Foundation is one of those – along with other private supporters of higher education and research – that have not abandoned the hope that universities will have the power to heal themselves. This is apparent already in the title of their program 'Efficiency Through Autonomy.' It is supporting a total of 10 universities with more than 23 million German Marks. Of course, this support does not mean that the Foundation has shut its eyes to the problems and risks associated with such a path toward greater action scope and greater autonomy. I shall deal with this below (cf. the sections on "New Goals and Tasks" and "Problems and Perspectives"). However, I shall first sketch my assessment of the current situation of universities and the challenges facing them, because I think that this will clarify not only the difficulties but also the needs for change.

The conditions for successful university activity are changing decisively in line with the rapid change in the international division of labor from hands, tools, and machines to brains, computers, and laboratories. For a number of years, the transformation from the traditional industrial to an information or knowledge society has not just been evoked in politician's speeches. In a world of 'global sourcing,' scientific knowledge – it is maintained widely – becomes increasingly more crucial in working out concrete problem solutions. At the same time, many advanced countries are revealing an unmistakable trend toward allocating less rather than more public funds to those institutions whose central function lies precisely in the training of future generations of researchers.

I shall now concentrate on five particularly marked changes that may be summarized under the following headings:

- Less state, more private sector.
- Less university attendance, more virtual college.
- Less specialization, more inter- and transdisciplinarity.
- Less input orientation, more assessment of performance.
- Less bilaterality, more globalization.

Less State, More Private Sector

The data on university funding are sufficiently well known. Even when conditions differ from federal state to federal state, there is an unmistakable trend toward declining or, at best, stagnating budgets. What is particularly conspicuous here – in a European comparison as well – is the dramatic cuts in spending per student since the mid-1970s (by almost 50 percent). At the beginning of the 1990s, a student cost the German public budget DM 6,318 per annum. The Netherlands were spending DM 9,540 per student; Great Britain, DM 12,177. The much promised 'contingency of planning' proves to be a mere certainty of advance warning on how much less money will be available over the next 3 to 4 years.

As a result, German higher education and research policymakers have recently also started considering the need for a new 'public-private partnership.' Alongside improving the fit between publicly funded and private sector research, this particularly means a need for new funding models in order to maintain the efficiency of the training and research domains funded traditionally by the public sector. Simultaneously, this places completely new demands on management.

It seems as if we are inexorably following a trend here that has become increasingly dominant in English-speaking countries since the beginning of the 1980s. There are now a number of initiatives and concrete plans in which the interface between public and private areas of responsibility has been shifted far into the field of commerce, and this does not just apply to college building, the provision of high-power computers, and so forth, but also in the joint establishment and funding of research institutions. The demand that more attention should be paid to foundation and innovation management in research and teaching and also to use the university as a training ground for entrepreneurs has gained – and this is a welcome trend – far more acceptance than in the 1980s (cf. Krull 1999: 6–9).

At the same time, we cannot overlook the fact that we are still finding it difficult to advance effectively along the path toward privatization. Here, I only wish to recall the seemingly endless debate on introducing student fees, in which rhetorical bouts were carried out with almost religious zeal, but no final breakthrough could be achieved. At present, I also doubt whether more can be achieved on the path toward founding private colleges. There is now a welcome variety of more than 10 private initiatives, and there is also talk about offering approximately 2,000 new student places. However, if we subtract the 1,200 places planned for the new International University of Bremen, it soon becomes apparent that most plans are not for universities but, at best, 'mini-versities' or even simple, one-course colleges (mostly business schools). Despite this criticism, I am, nonetheless, convinced that the current private initiatives to set up universities are a necessary beginning, and, in the years to come, we shall witness a much more dynamic development toward partial privatizations of previously publicly funded institutions and more large-scale foundations of colleges offering a wider range of subjects. First steps toward such partial privatizations can already be observed in some technological universities, for example, at Karlsruhe and Hamburg-Harburg, and no longer just for research but specifically for the international 'marketing' of their courses as well.

Less University Attendance, More Virtual College

Everybody is talking about the knowledge or information society. The data highways seem to be becoming the traffic routes of the future. Thanks to the Internet and e-mail, information is becoming available in increasingly larger amounts and, simultaneously, at an increasingly faster speed. However, it is not just the changes in the transport of information and data affecting all areas of society that are worthy of interest here, but also (or perhaps, above all) changes in the scientific methods and questions that the 'digital revolution' has made possible. The spectrum associated with the 'informatization of knowledge' that is perhaps also leading to a new 'order of knowledge' extends from the mapping of the human genome, across the application of methods of nonlinear dynamics in the natural and engineering sciences, up to historical social research with mass data, to name only three examples. One particular challenge facing universities is that the production, processing, and distribution of new information occur almost simultaneously. Lectures and papers by outstanding professors at Harvard or Stanford, for example, become just as accessible for students at German universities as the lectures of their own German professors. An ever more perfect information network that permanently confronts our scientific understanding with what we already know or should have known is increasingly creating the impression that the information networks have developed more quickly than the research they were designed to serve.

At the same time, more and more virtual colleges are being set up and are moving into the education market with interactive courses. Although the largest Internet college in the USA, the University of Phoenix, has no real campus, it already has more than 200,000 students or subscribers (it is hard to know how to categorize them exactly). Up to now, Germany has followed this trend only hesitantly, and mostly in the technological college domain. However, Bavaria's plan to set up a state-wide Internet college will probably soon be followed by other states. This simultaneously raises the question of what repercussions these electronic, interactive courses will have on studies at a solid university building.

Less Specialization, More Inter- and Transdisciplinarity

For a long time, the organization into subject fields was the pride and joy of German universities. This was justified, because in many subjects – not least the 'classic natural sciences' such as physics, chemistry, and biology – their researchers were among the best in the world. Interdisciplinary research was already proposed repeatedly in the 1960s and 1970s and almost sounds old-fashioned today. However, for some time now, it has been experiencing a renaissance under new labels, because in many areas (not just in environmental research in which it has been apparent for a long time) the emerging problems can be solved only through cooperation between outstanding researchers from various disciplines.

Leading international science researchers like Michael Gibbons, Camille Limoges, and Helga Nowotny et al. have tried to describe these decisive changes in a book entitled 'The New Production of Knowledge' (Gibbons / Limoges / Nowotny 1994). They have proposed a heuristic discrimination between the traditional 'Mode 1' (disciplinary, primarily innerscientific context, homogeneous research questions, etc.) and 'Mode 2.' Mode 2 is defined particularly by the following elements: (a) The social and economic context is of great importance for a wide-ranging, mostly transdisciplinary research. (b) New research questions often originate outside of the science sector. (c) A common basis for the ability to communicate scientifically first has to be established between the experts involved, new methods have to be worked out together, and, frequently, standards can be defined only at the end of a project. (d) The relation to applied science and practice is often in the foreground. The final concern is to link together the previously all too often separated domains of theoretical knowledge, applied knowledge, and practical knowledge in new ways. In their new book, Helga Nowotny, Peter Scott, and Michael Gibbons also ask how the increasing demand for 'socially robust knowledge' can be met in the future (cf. Nowotny / Scott / Gibbons 2001).

Less Input Orientation, More Assessment of Performance

Up until well into the 1980s, science policy was almost exclusively input-oriented (and not just in Germany). The focus was on increasing the number of student places (without doubt, a necessity since the number of enrollments had doubled). From the very outset, the increase in the numbers of first-year students, which was also motivated by labor market policy, was accompanied by an almost complete neglect of the number of graduates and the other activities of the university. Since the quantitative expansion of the education and research system has come to a halt, and, in this 'steady state' situation, new training courses and research institutions can be attained only through discontinuing old or outmoded workfields and closing departments, faculties, or institutes, the search for 'objective evaluation standards' has been stepped up throughout the world.

Numerous countries can offer a wide range of experiences with different structures, procedures, and institutional forms for evaluating university teaching and research. Terms such as evaluation, quality assessment, and productivity-related fund allocation are on everyone's lips. Of course, previous experiences have also shown the importance of a balance between quantitative and qualitative methods and how urgently an effective framework for the external quality evaluation of research and teaching needs to be established. If evaluations have no consequences – either intentionally or not – and, as a result, no structural changes and relocations of resources can be made, they soon lose their credibility and degenerate into frivolity (cf. Krull 1998: 151). In Germany at present, the reverse would seem to be true, with, in many cases, a fear of any kind of evaluation. It is repeatedly astonishing to see what reservations are raised – and, in particular, how – with regard to any assessment of performance.

In the future, too, the function of the university will remain the same, namely, to acquire, impart, and generate knowledge (as well as the technologies that may be necessary for this). However, the following aspects will become increasingly important for their efficiency: the underlying ideal along with all its attendant goals and visions, the culture of teaching and learning, the organizational structures and control mechanisms, and, not least, the available financial and staff resources.

Recent studies (including those of the American science researchers Rogers J. and Ellen Jane Hollingsworth 2000: 215–244) nonetheless confirm impressively that the decisive breakthroughs that receive Nobel prizes and comparable awards tend to occur at medium-sized universities with a broad spectrum of interacting disciplines, a minimum of hierarchies, and a high degree of horizontal communication offering a multitude of opportunities of interacting with the fields of practice. They also have a strategically focused college management with effective procedures of quality assurance that assign a particularly high status in every sense to research achievements. In the United States, this applies, for example, to Rockefeller University (where last year's Noble prize winner Günter Blobel is to be found) and the University of California at San Francisco to a much higher extent than, for example, to the University of California at Berkeley.

Less Bilaterality, More Globalization

"Internationality belongs to the essence of science." This is the first sentence of the Science Council's recommendation for the internationalization of scientific relations (Wissenschaftsrat Köln 1992: 5). This particularly means the, so to speak, constitutive international character that cannot be held back for any length of time by historically given or politically ordained borders. However, in the context of a world-wide market, not just for research- and technology-intensive products, global networking, and multinational companies, the international dimension of science gains a new importance. As worthy as it may have been in individual cases for German universities to have supplied themselves well with partnerships and cooperation agreements, this can scarcely distract from the fact that new efforts are required (and have actually been implemented at many universities) if they are to hold their own in the international competition between colleges. These include, among others, basic improvements in study conditions in numerous faculties, particularly in overcoming the lack of communication between the natural and engineering sciences, and recognizing internationally comparable qualifications. Through designing study courses in modules up to a first university qualification and setting up 'international graduate schools' together with leading international universities in other countries, German universities have gained completely new opportunities to demonstrate their efficiency and recapture some of their earlier reputation. By the way, the participation of Rice University at Bremen or Purdue University at Hanover is essentially due to the fact that it enables not only students but also teaching staff at both universities to gain additional international experience and, hence, intercultural competence.

Efficiency Through Autonomy

As already mentioned above, with its program 'Efficiency Through Autonomy,' the Volkswagen Foundation is supporting 10 universities to the tune of more than 23 million DM. The universities are the Free University of Berlin, the Humboldt University at Berlin, the University of Bremen, the Technological University of Clausthal, the University of Dortmund, the University of Göttingen, the University of Hamburg, the University of Heidelberg, the University of Kassel, and the University of Mannheim. These institutions differ greatly in terms of age, size, structure, and framing conditions. It is particularly pleasing to see that, thanks to the Humboldt University, the program does not just include universities from former West Germany.

The central goal of the Volkswagen Foundation program is to improve the efficiency and effectiveness of universities by strengthening their autonomy. Hence, the concern is not with university reform in a general and comprehensive sense, but to start off in a very concrete way, namely, with the university management and decision-making structures, and target these for specific reforms. As a funding institution, the Volkswagen Foundation usually contributes to strengthening research structures. In this case, it is concentrating particularly on promoting the organizational and administrative preconditions for successful research and teaching. The idea is to support universities in their efforts to examine and improve their structures, procedures, and processes on various levels; to reorganize areas of competence and responsibility and allocate them more meaningfully; to try out corresponding new rules and then implement them effectively. However, this should not be an end in itself. Merely focusing on technocratic and administrative measures would not go far enough. The final concern is for universities to develop structures and procedures that create the preconditions for carrying out their genuine tasks as well as possible with a minimum of administration and friction loss, namely, science in the form of research, teaching, training, and knowledge transfer.

To attain the goal of higher efficiency, it seems essential to follow the path toward more autonomy. Some implications of this are:

1. Responsibility should no longer be socialized diffusely but be made identifiable and attributable.

2. It must be ensured that responsibility is not without consequence for those bearing it.
3. Decision-making competencies and obligations must be allocated to those who can and must take responsibility for the consequences.
4. It is necessary to promote an awareness among members of the university that it is *their* university in which they are working.

Higher efficiency initially means that:

1. Resources are exploited and used better.
2. The available means are applied more effectively.
3. Procedures and processes are simplified and speeded up.
4. Communication and cooperation are intensified on the various levels and between the individual units.

The reform plans supported by the Volkswagen Foundation reveal a multitude of different concepts and approaches. This places them in line with the Foundation's goal of every university having to find the best possible solution to fit its own framing conditions (rather than making the often exaggerated claim in advance of developing models for a German university reform in general). I shall now sketch three reform approaches in more detail.

The approach at the *University of Bremen* aims toward a comprehensive reorganization in the sense of a universal contract and quality management. Under the heading "We are changing our university," some of the concerns are:

1. An achievement orientation based on agreements over goals and contracts.
2. Quality development in teaching and research for the extension and control of the faculties (drawing up contracts between the university administration and faculties).
3. Development of new forms of participation in decision making and autonomy (between the rector's office and the faculties as well as between faculty speaker, academic self-administration, and faculty administration in the faculties themselves).
4. Achievement- and obligation-oriented allocation of funds.
5. Teaching contracts between staff and students.

6. The establishment of a 'learning organization' (through, among others, 'staff-supervisor discussions').
7. Development of guiding principles and a management concept for the university (the university as an enterprise).
8. Development of a new contractual relationship between the university and the city state of Bremen.

Following principles of process-oriented and systemic organization development, the *University of Hamburg* is striving toward a design and control of its administration and self-government focusing on tasks and goals as well as an improvement of internal university communication. On the basis of an agreement over the goals and profile of the university, increases in efficiency and efficacy should be achieved through more effective links between planning, decision-making, and executive activities. Responsibilities for decision making and action are being delegated from the central administration to decentralized units while simultaneously strengthening the service concept. Alongside the topics "Setting goals and forming profiles" and "Team discussions," work is being carried out on the following sub-projects:

1. Developing and testing internal agreements on goals.
2. Strengthening the faculties.
3. Reorganizing central administration.
4. Developing a university report and controlling system.

The goal of the project at the *University of Heidelberg* is to improve the deployment of available resources throughout the university and to create a new awareness for costs and efficiency. The institutes are being assigned a global budget with a large degree of freedom in allocation (with corresponding accountability). The existing resource allocation accounting processes are being developed into new funding modes and supplemented by a business accounting system. This makes it possible to set up an internal services and resources market in the university. The university management will thus be enabled to pay more attention to long-term strategic objectives and the necessary structural decisions. This will be oriented toward cost and productivity data developed within the framework of an internal information and reporting system that will also be made available to the institutes.

New Goals and Tasks

In a paper on structural plans for the University of Constance, the need to strengthen priority setting and profile formation is expressed as follows:

> Nowadays, no university can function as an institutional expression of all branches of academic knowledge, especially when it comes to generating, processing, and conveying this knowledge. In this sense, there are no longer any complete universities in line with the ideal of the old university. This process is due not only to modern developments in academia but also to financial and organizational constraints, and it compels us to form more specialized profiles from the perspective of a limited, 'finite' universality that is now replacing the unlimited, 'infinite' universality claimed by the universities before. These have to be expressed not only from scientific but also from structural and organizational perspectives. As a result, the special character of the modern university of the future will be revealed less in its variety of disciplines but far more in its special profile and corresponding specializations (Strukturkommission Universität Konstanz 1998: 32).

However, the profile formation demanded here requires each university to have a clear concept of its goals and tasks. It calls for intensive deliberations, tests, negotiations, and decisions over what should be the priorities – and all against the background of a reliable quality assessment of respective strengths and weaknesses as well as a prospectively based strategy. It is my belief that a structure and development planning designed in this way cannot be worked out in each institution by itself. It requires interaction with the external world and can finally function only if it is based on an extended concept of autonomy – in the sense of a responsibility of the scientific community at large and no longer just the individual university.

The development of a new concept has to be accompanied directly by a strengthening of inter- and transdisciplinary research and teaching. Nowadays, in terms of science policy, it is almost a truism to say that research within each area moves increasingly more frequently at the borders of the traditional subjects and disciplines, and that, outside of academia, it is expected to contribute to solving problems in, for example, the realms of energy, the environment, and health that cannot help but stray over the borders of subjects and disciplines.

One expression of the transdisciplinary orientations that are becoming effective wherever a solely disciplinary definition of problem states and problem solutions no longer works, is the new scientific centers emerging, or already working nowadays, like those at Harvard (Center for Imaging and Mesoscale Structures) and Stanford (Bio-X) in the United States, but also at the University of Munich (Center for Nanoscience). These centers are no longer organized along the traditional lines of physics, chemistry, and biology institutes, but from a problem-oriented perspective that, in these cases, follows the current trend in science. Transdisciplinarity proves to be a new and highly promising principle of research. Where it works, the old institutional structures pale in comparison (Mittelstraß 1999: 3).

To develop a specific, not just regional, but, above all, also international profile, it is essential for each university to reconfigure its previous efforts toward internationalization that were generally based on cooperation agreements. Alongside a universal application of international standards for the recognition and comparability of student credits and qualifications, not least in the ECTS (European Credit Transfer System), this includes the formation of a network for research and teaching that particularly emphasizes combinations of competencies that may be used to supplement the resources of one's own university. As long as attention is paid to the criterion of scientific excellence and, for example, internationality is not promoted for its own sake, such 'strategic alliances' and the resulting combinations of competencies are exceptionally suitable for giving the specific profile an additional productive focus.

The rapid advances in new information and communication technologies make new teaching and learning methods based on multimedial forms of imparting knowledge enormously important. As mentioned above, the 'digitalization of knowledge' implies that new knowledge is generated, processed, made available, and imparted almost simultaneously. This does not just mean an extension of the traditional correspondence course model. Nowadays, it is far more the case that each university has to ask itself how it intends to react to the major relaxation of the constraint of teaching and learning to one time and place ('learning anytime, anywhere'). Future courses can be carried out in a joint division-of-labor process by several faculties or several universities on both a national as well as international level. As mentioned above, the situation is similar for research: Here as well, multimedial information and communication technologies have long played an important role and have led to the development of new forms of work and organization.

To the extent that the forms and paths by which new knowledge is created are developing from a relatively homogeneously structured, institutionally anchored process shaped by the discourse within the individual disciplines to a more open process shaped by a service character and a firmly applied reference, the demands on colleges and research institutions on the one hand and industry on the other are also changing. Both need to work together more intensively than before in training and research processes. This is why German higher education policymaking has recently been talking more about the need for a new public-private partnership. Alongside improved coordination between publicly and privately funded research, this particularly means the need for new funding models if the traditionally publicly funded research and training institutions are to retain their efficiency. German universities still find it difficult to cope with such an idea. They particularly fear a loss of independence in research and teaching. However, the need is to develop new forms of coexistence in the sense of a "purposeful scientific cooperation between two equally entitled partners with different abilities and goals" (Stock 1990: 10, translated). The goal of a public-private partnership is not a reciprocal transformation into the other but an optimal exploitation of the different competencies and strengths of both partners. The main concern is to improve knowledge networking through binding cooperation schemes, in-service training, and the like. This means that it is necessary to develop the ability not only to produce relevant knowledge but also to register this knowledge. Whether this will simultaneously lead to a 'denationalization' of tertiary education does not need to be discussed further here. However, it can be anticipated already that these new forms of coexistence will also lead to changes in the financial interfaces between the public and private sector.

The more strongly universities are controlled on the basis of contracts, thus through productivity agreements, the more important the quality evaluation of the entire performance spectrum of a university will become as well. Internal and external evaluation need to be coordinated better and developed further. Germany has a lot of catching up to do, particularly in evaluating teaching and university services. Of course, the quality of appointments remains of decisive importance. However, even in this context, reforms based on new internal and external checks and balances would be desirable that could be oriented toward the practices developed at leading non-German universities.

Promoting science and research with public funds is justified by the crucial importance of producing new knowledge and passing it on to society, above all, the young generation. It is a particular task of the university to produce excellently qualified young persons who will be able to take over leading roles in academia, business, and society. With a view to future generations of university teachers, there is a need to enable early scientific independence so that creative potential and motivation can be exploited optimally. This requires a new personnel structure and also discontinuation of Germany's postdoctoral habilitation system. It should be replaced by demanding other proof of special scientific aptitude for university posts that goes beyond an excellent Ph.D. In my opinion, the quality assurance for high posts at universities and research institutions is not a job for the institute of origin but – with the involvement of external experts – for the institution with the post to fill.

At the present time, more than 60 percent of the postdoctoral graduates who transfer to English-speaking countries – predominantly the United States – do not return to Germany. This is something that needs to be thought about. From many personal conversations, I know that the lack of assistant professorships and posts for junior research group leaders is a particularly major handicap. At the Volkswagen Foundation, we have set up a special program for 'junior research groups at universities' in order to provide an opportunity for particularly highly qualified members of the next generation to run their own research groups. It remains to be hoped that the current Education Minister's openness to reform will be crowned by the successful introduction of a basically new personnel structure during the further course of the present parliamentary term.

Problems and Perspectives

A spirit of optimism can be found in many German universities. Dynamics of change have been set in motion that are also forcing previously more reticent universities into action. However, this does not mean that the desired goals can be attained straightforwardly. I should like to sketch some of the major problems that I consider to be present in the various reform plans:

1. The tense relation between the top-down and bottom-up orientation in the development and implementation of new management and decision-making structures; the strategic versus participative approach in plans; the integrative function of the discussion on ideals and the associated goal of strengthening corporate identity ("We are changing our university"); the need for structural reform versus much-loved habits or committee traditions; questioning the legitimacy of reform plans outside traditional procedures and – losely linked to this – the necessary gaining of trust through intensive public relations work.
2. Potentials and limits of achievement-related allocation of funds as an instrument for controlling resources; tendencies to self-block reform plans through excessive adjustments to data and so forth; premature restrictions of the breadth of funds to be included and difficulties in obtaining acceptance for the new instrument; the problems of implementation: Differentiation of achievement and internal university integration seem to be hard to reconcile; the necessary complementary function of negotiated goals and corresponding forms of productivity-related allocation of funds.
3. The coordination of organization development and personnel development; not only technological and administrative abilities require further development; the willingness to take over responsibility for resources does not grow by itself; in individual institutions, the projects have to tackle staff problems – both in terms of fluctuations in team members as well as intended transfers into university management.
4. The decentralization of responsibility for resources and the accompanying administrative processes require rethinking the interface between central administration and faculty or institute administrations; the danger of extra work and over-organization is in no way banished.
5. The relation between the university and the state, between university administration and government ministries has yet to be clarified satisfactorily; many contracts and new models for productivity and obligation-related allocation of funds are a pretext for new modali-

ties of fine control that tend to encourage more rather than less state intervention.

Final Comment

Finally, I should like to reemphasize three points:

1. The 'tertiary teaching and research institution' of the university has to change itself – in a comprehensive sense – into a learning organization, to stimulate its staff repeatedly to enhance productivity, and convey a sense of membership to all.
2. The changes sketched at the beginning and the associated challenges have to be accepted. New concepts of strategic control and everyday management also have to be developed as well as new forms of imparting knowledge, curriculum organization, and research itself.
3. For a long time, the academic world was engaged in a sort of 'self-mystification' (cf., also, the labyrinth as a metaphor for scientific work). Although the goal is more success, 'self-enlightenment,' learning from mistakes, reorganization of procedures, and so forth are essential milestones along the way.

It should also be borne in mind that not all therapeutic steps will succeed, and that the patient will have to anticipate relapses. However, in all, I am confident that the outcome will be reform of the more efficient institutions, and that the end of the story will not be, to quote Francis Bacon, "Cure the disease and kill the patient."

Notes

1 I am grateful to Jonathan Harrow, Bielefeld, for translating the manuscript from German to English.
2 This contribution is based on a talk the author held at the University of Ulm and the University of Marburg, Medical Faculty, on occasion of the *Dies Academicus.*

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Evolutionary Theory and the Social Sciences

Increasingly a Mutual Exchange

Ever since the constitution of sociality as a matter sui generis, social scientists have, at best, ignored the biological sciences as irrelevant or, at worst, fought against them for fear of reductionism and/or racist underpinnings. As a consequence, social scientists avoided to meet the challenge of seriously considering the biological aspects of culture. Repelled by the bold claims of sociobiologists (instigated be E.O. Wilson in 1975), they failed to realize the more substantive contributions among biologists as well as the usages made by their fellow social scientists: Among these were Donald Campbell (psychologist), Napoleon Chagnon and William Irons (anthropologists), Richard Nelson and Sydney Winter (economists). Notably in the last 15 years the efforts have become ever-more intense and less exploratory (for overviews, cf. Barkow/Cosmides/Tooby 1992, Smith/Winterhalder 1992; Weingart/Richerson/Mitchell/Maasen 1997).

Basically, scholars pursue two different research strategies: either a homological or an analogical strategy. On the homological account, one argues that culture does have a direct impact on genetic fitness and one appeals to the theoretical resources developped in the biological investigation of nonhuman behavior. As the genetical bases of human social behaviors are not (well) known, two assumptions are required: first, the phenotypic gambit (Grafen 1991) according to which for each trait under study there is some mapping onto the genetic level. Hence, one can ignore the latter and still presume that fitness consequences will have evolutionary effects. The second assumption is called the natural origin argument: It holds that even the most clearly culturally variable behavior that is not directly genetically controlled, can be treated as if it were. According to this perspective, any cultural learning mechanism that survived an initial selective competition will lead to behaviors that increase genetic fitness. Another way to make use of biology is its theoretical structure in order to build analogous models for cultural change. The analogical strategy rests on the assumption that evolution is a historical process: Human cultures are historical entities, changing over time, but they also carry with them vestiges of their past. Analogous reasoning acknowledges that the relation between culture and evolution is one of similarity, and not identity, thus illuminating both the similarities and differences between biological and cultural processes.

Most prominent accounts along the line of dual inheritance or co-evolutionary models have been given by William Durham (1991) as well as Robert Boyd and Peter Richerson (1985), respectively. In the following chapter Richerson and Boyd will argue that cultural evolution can indeed create social institutions that in the long run shape important aspects of even the innate components of human biology. The long-cherished division between the biological and the cultural (or nature / nurture) is seriously challenged by this type of evolutionary reasoning and so are the boundaries between the biological and the social sciences.

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Culture is Part of Human Biology. Why the Superorganic Concept Serves the Human Sciences Badly

Peter J. Richerson and Robert Boyd

Introduction

Rates of violence in the American South have long been much greater than in the North. Accounts of duels, feuds, bushwhackings, and lynchings occur prominently in visitors' accounts, newspaper articles, and autobiography from the eighteenth century onward. According to crime statistics these differences persist today. In their book, Culture of Honor, Richard Nisbett and Dov Cohen (1996) argue that the South is more violent than the North because Southerners have different, culturally acquired beliefs about personal honor than Northerners. The South was disproportionately settled by Protestant Scotch-Irish, people with an animal herding background, whereas Northern settlers were English, German and Dutch peasant farmers. Most herders live in thinly settled, lawless regions. Since livestock are easy to steal, herders seek reputations for willingness to engage in violent behavior as a deterrent to rustling and other predatory behavior. Of course, bad men come to subscribe to the same code, the better to intimidate their victims. As this arms race proceeds, arguments over trivial acts can rapidly escalate if a man – less often a woman – thinks his honor is at stake, and the resulting 'culture of honor' leads to high rates of violence. Nisbett and Cohen support their hypothesis with an impressive range of data including, laboratory data, attitude surveys, field experiments, data on violence, and differences in legal codes.

Their laboratory experiments are most relevant to our argument here. Cohen and Nisbett recruited subjects with Northern and Southern backgrounds from the University of Michigan student body, ostensibly to work on an psychological task dealing with perception. During the experiment, a confederate bumped some subjects and muttered "asshole" at them. Cortisol (a stress hormone) and testosterone (rises in preparation for violence) were measured before and after the insult. Insulted Southerners showed big jumps in both cortisol and testosterone compared to uninsulted Southerners and insulted Northerners. The difference in psychological and physiological responses to insults was manifest in behavior. Nisbett and Cohen recruited a 6'3" 250 lb (190 cm, 115 kg) American style football player whose task was to walk down the middle of a narrow hall as subjects came the other direction. The experimenters measured how close subjects came to the football player before stepping aside. Northerners stepped aside at around 6 feet regardless of whether they had been insulted. Un-insulted Southerners stepped aside at an average distance of 9 feet, whereas insulted Southerners approached to an average of about 3 feet. Polite but prepared to be violent, un-insulted Southerners take more care, presumably because they attribute a sense of honor to the football player and are normally respectful of others' honor. When their honor is challenged, they are prepared and willing to challenge someone at considerable risk to their own safety.

Nisbett and Cohen's study illustrates the two main points we want to make in this essay.

– *Culture is fundamental to understanding human behavior.* The high rates of violence in the American South are a product of a social heritage. The Southern culture of honor arose and was for a long time maintained by an environment that made it an efficacious means of protecting a family's livelihood. Nowadays, few Southerners are pastoralists, and few Notherners are peasant farmers. Nonetheless, these striking differences in behavior persist.
– *Culture causes behavior by causing changes in our biology.* An insult that has trivial effects in a Northerner sets of a cascade of physiological changes in a Southerner that prepare him do violent harm to the insulter and to cope with the likelihood that the insulter is prepared to do equal harm in return. We argue that this example is merely a single strand in mass of connections that so thoroughly web culture into other aspects of human biology that any separation of them into distinct phenomena is impossible.

We can certainly make an analytical distinction between genetic and cultural influences on our behavior, and the influences of non-cultural forms of environmental influences. However useful, this analytical distinction emphatically does not license is an ontological separation of culture and biology separate levels of organization with only simple biological 'constraints' on cultural evolution and diversity. Culture is as much part of human biology as bipedal locomotion, and cultural and genetic influences on human behavior are thoroughly intertwined.

Most of the important threads of twentieth-century social science have rejected one of these two principles. Some traditions within the social sciences, for example rational choice theorists, many psychologists, and human sociobiologists, place little emphasis on culture as a cause of human behavior, and sometimes view cultural explanations as limited to historical-descriptive accounts devoid of real explanatory power. While we sympathize with critics of current culture studies, this state of affairs is not inherent in the culture concept. The effects of culture on human behavior can readily be addressed with the methods of the so-called hard sciences (e.g., Cavalli-Sforza/Feldman 1973, 1981; Lumsden/Wilson 1981; Boyd/Richerson 1985; Richerson/Boyd 1989). We want to convince you that a Darwinian science of culture is a respectable and promising pursuit and that the easiest way to see why is to place culture squarely in the middle of human biology.

Many social scientists have objected to moves of this ilk for fear that the result would be to 'reduce' culture to biology. Many biologists interested in humans have encouraged such fears. E.O. Wilson (1975, 1998) argues that disciplines stand in a reductionistic relation to one another, and that the ultimate fate of the social sciences is to be reduced to sociobiology. The project we champion differs significantly from Wilson's. Part of the payoff for locating culture in biology is that we can model the influence that culture has on genes as well as the 'reductionistic' influence of genes on culture. If we imagine that genes and culture are two inheritance systems that interact on the *same level* to produce human behavior we can make 'coevolutionary' or 'dual inheritance' models of the basic processes by which this interaction takes place. These models have the virtue of reducing to more conventional positions such as rational choice theory, various kinds of human sociobiology, and, most interestingly, Sahlins' (1976) cultural reason, under different simplifying assumptions (Boyd/Richerson 1985: chapter 8). Under a broad and reasonable range of assumptions, evolving genes, evolving culture and environmental contingencies all conspire to affect human behavior.

For some students of culture, locating culture in biology may still seem a risky strategy. The powerful theories and intimidating empirical methods of the natural sciences might overwhelm culture as if science is somehow inherently biased against cultural explanations. We believe the opposite. Cultural explanations of human behavior are likely to prove exceedingly robust. Human nature itself may be substantially socially constructed by the processes of cultural evolution, not just our ideas about it. Culture, on this hypothesis, has the fundamental role in human behavior long claimed for it by cultural anthropologists and many other social scientists and humanists. Cultural evolution can create social institutions that in the long run shape important aspects of even the innate components human biology. Innatists run a real risk that some of their genes will be 'reduced' to culture!

The Poverty of Superorganicism

Most social scientists treat culture as a "super-organic" phenomenon. As A.L. Kroeber put it in trying to explicate the superorganic concept "particular manifestations of culture find their primary significance in other cultural manifestations, and can be most fully understood in terms of these manifestations; whereas they cannot be specifically explained from the generic endowment of the human personality, even though cultural phenomena must always conform to the frame of this endowment" (Kroeber 1948: 62). Theodosius Dobzhansky, an evolutionary biologist very sympathetic to the twentieth-century social sciences of culture, states it: "In producing the genetic basis of culture, biological evolution has transcended itself – it has produced the superorganic" (Dobzhansky 1962: 20). Social scientists have long used rhetoric like this to dismiss the need to incorporate biology in any serious way into their study of human behavior. Humans cannot fly by flapping their arms or swim naked in polar seas, but outside of obvious framing constraints of this type, things biological had no explanatory role in explaining things cultural. On this view, biology is important, of course, because we need bodies and brains to have culture. But biology just furnishes the blank slate on which culture and personal experience write. This idea goes back to the turn-of-the-twentieth-century pioneers of the sociology and anthropology. For example, the French sociologist Gabriel Tarde's (1903) book *The Laws of Imitation* prefigures in many ways the ideas in this essay, but he rejected any considerations of biology as a practical matter of disciplinary specialization. Dobzhansky's usage was probably inspired

Kroeber and kindred influential social scientists of his period. Dobzhansky was recognizing a *fait accompli* we believe. If biologists of his day wanted harmonious relations with social scientists rather than destructive nature-nurture disputes, they had to make obeisance to the superorganic concept. Yet Dobzhansky went right on to say: "Yet the super-organic has not annulled the organic" (1962: 20). He never satisfactorily resolves the tension between these two statements. Ingold provides a discussion of three different senses of "superorganic" used by social scientists over the years about which he summarizes "the superorganic has become a banner of convenience under which have paraded anthropological and sociological philosophies of the most diverse kinds" (Ingold 1986: 223ff.).

In our view, superorganicism is wrong because it cannot deal with the rich interconnections between culture and other aspects of our phenotype, as exemplified by the Southern culture of honor. Superorganicism may have served a useful function in helping the social sciences get on their feet (after a couple of beers – you buy the first round – we'll be happy to dispute even that). Better to grasp the nettle: *Culture is a part of human biology,* as much a part as bipedal locomotion or thick enamel on our molars. Because of culture people can do many weird and wonderful things. But in all cases the equipment in human brains, the hormone producing glands, our hands, and the rest of our bodies play a fundamental role in how we learn our cultures and why we prefer some ideas to others. This is a minority, even heretical, position among human scientists, albeit one with a long pedigree. Freud was a defender of it (Sulloway 1979) as are many modern psychologists, some of whom we discuss below.

Suppose we define culture like this:

> Culture is information capable of affecting individuals' phenotypes which they acquire from other conspecifics by teaching or imitation.

In the taxonomy of definitions of culture, ours is in a category that emphasizes the psychological aspects of the phenomenon (Kroeber/Kluckhohn 1952). Culture is taught by motivated human teachers, acquired by motivated learners, and stored and manipulated in human brains. Culture is an evolving product of populations of human brains. Humans are adapted to learn and manage culture by the way natural selection has arranged our brains. Human social learners in turn arrange features of their brains as they learn from others and the environment. Culture is a major aspect of what the human brain does, just in the same way as smelling and breathing are what noses do. Culture-making brains are the product of more than two million years of more or less gradual increases in brain size and cultural complexity. During this evolution, culture must have increased genetic fitness or the psychological capacities for it would not have evolved. Indeed, anthropologists long interpreted much of culture in adaptive terms (e.g., Steward 1955). Rather than a neat, narrow boundary between innate and cultural processes that can be characterize by a short list of simple biological constraints on human behavior, we imagine a wide, historically contingent, densely intertwined set of phenomena *with causal arrows operating in both directions.* If we think of human culture as a part of human biology in this way we simply don't need to try to unpack what 'superorganic' could possibly mean.

We are a bit sensitive on this point because the style of analysis of the cultural phenomenon we advocate has collected its share of brickbats from both sides of the superorganic divide. From the evolutionary biology side, Richard Alexander (1979: 79–81) and others have supposed that the analysis of culture as an inheritance system is an attempt to defend the superorganic concept against evolutionary analyses of human behavior. On the other, some social scientists have treated our work as yet another attempt to 'reduce' culture to biology (e.g., Ingold 1986: chapter 7). In our view, culture and the rest of human biology interacted in complex ways in the evolutionary past to produce an extraordinary ability to imitate. Genes and culture continue to interact in the everyday world of human behavior in most complex ways. Functional MRI and the other brain scanning techniques are even beginning to give us a real-time picture of how these interactions take place in the brain. In some ways these processes resemble the claims of the conventional social sciences, and in some ways the proposals of human sociobiologists and innatist psychologists. Very often the processes don't resemble the proposals of either. There are some fascinating scientific puzzles to solve here. We doubt there will ever be any use for the superorganic concept, but if one is found we'll take it in stride. In the meantime, we find it liberating just to drop it from our vocabulary. If you'll try it, we think you'll like it too!

Culture is a Derived Human Trait

We as yet know precious little about exactly how genes, culture and external environment play upon the brain to produce our behavior. We do know that without a human brain, you can't acquire human culture. Recent comparative primatology is beginning to describe the nature of our capacity for imitation relative to other apes in some detail. Groups led by Andrew Whiten and Michael Tomasello have studied the social learning of apes and human children in a tightly comparative framework (Whiten/Custance 1996; Tomasello 1996). For example Tomasello's group used human demonstrators of a raking technique to test the social learning of juvenile and adult chimpanzees and 2-year-old children. The demonstrators used two different techniques of raking to obtain otherwise unreachable, desirable objects. Control groups saw no demonstrator. The demonstrator had a big effect on the use of the rake by both children and chimpanzees compared to control groups, but the interspecific difference was also large. The children tended to imitate the exact technique used by the demonstrator but the chimpanzees did not. In similar experiments with older children Whiten and Custance report rapid increase in the fidelity of imitation by children over the age range 2–4 years, with adult chimpanzees generally not quite achieving the fidelity of 2 year old humans. Human children already at quite young ages are far more imitative than any other animal so far tested, although a very few other animals, such as parrots, are also about as good as chimpanzees at imitative tasks (Pepperberg 1999).

What is the biological underpinning of our hypertrophied social learning system? Tomasello (1999) gives an account based on a considerable body of observational and experimental evidence. He argues that the most important unique feature of human cognition is what is called 'joint attention.' Human children, beginning at about nine months of age, begin to pay attention to the attention of other people and to call the attention of others to things of interest to themselves. For example, in Western cultures, children interact with their caregivers in little word-games where both the child and the adult pay attention to the same object, typically a toy. The child may hand the toy to the adult and then look to the adult for some reaction or vice versa. The adult often articulates the word for the toy – 'ball,' 'dolly,' 'truck.' In this way children learn their first words and use the joint attention situation to try out their new words. Or the adult operates the toy – throws the ball, dresses the doll, runs the truck on its wheels – and the child learns these skills. Tomasello dissects joint attention into nine separate skills emerging between nine and twelve months of age. The early maturation of these skills and the apparent necessity of having them before substantial imitation can occur argue for a large element of innate specification of the joint attention system. All of these skills are specific to normal humans and are sufficient to account for the differences in imitative capacities of children and chimpanzees. Autistic children seem to have specific deficits in joint attention and are greatly handicapped in learning language and acquiring other culturally transmitted skills. At the end of the normal developmental sequence, children understand that other people are intentional agents with motivations like their own. Thus, the actions of other are cues as to how one can take advantage of the experiences and skills of others to accomplish one's own goals. From this age onward children are efficient imitators, and begin to rapidly build their cultural repertoires. According to Tomasello's hypothesis, the same joint attention skills underpin the learning of all aspects of culture from language to subsistence skills. Many evolutionary psychologists prefer modular hypotheses, imagining many separate mental 'organs,' most famously for language learning (Pinker 1994). The evidence on these problems is far from conclusive. The very existence of a seemingly rather unusual and highly organized capacity (or capacities) for imitation does argue that an understanding of it (them) is part of evolutionary psychology correctly considered.

Evolved Human Nature Versus Gene-Culture Coevolution

Most evolutionary theories of *human* behavior inspired by Darwin underestimate the importance of culture in the evolution of human behavior, much as superorganicists underestimate the role of genes. Typically, biological theorists assume that natural selection first built human biology and then that this evolved biology controls human behavior. In such theories, the ultimate determinants of human behavior are the product of selection on genes. Any role for culture is proximate and can be thought of as implementing structures built into the genes. The distinction between proximate and ultimate causation is Ernst Mayr's (1961) borrowing from Aristotle. Mayr argues that in biology, proximate causes are typically physiological. Birds migrate equatorward when day lengths shorten because the brain converts short day length into hormonal signals that activate migratory behavior. The ultimate cause of migratory behavior is natural selection. Migration is an evolved strategy to exploit the favorable season at higher latitude while passing the harsh winter in undemanding habitats. Selection has shaped the reaction of the brain to daylength and all the downstream physiological and behavioral machinery to accomplish the migratory adaptation. Much of the dispute over the role of culture in human behavior is understandable in terms of the proximate / ultimate distinction.

Most Human Sociobiology Unduly Neglects Culture

Most students of human behavior inspired by evolutionary biology prefer to keep things simple and neglect or deny the possibility that culture has a fundamental role to play in human adaptation and especially that it has any component of ultimate causality. The classic paper by Richard Alexander in 1974 and the final chapter on humans in Edward Wilson's landmark treatise *Sociobiology* in 1975 caused considerable interest in applying evolutionary ideas to human behavior. Two traditions that grew up in the wake of Alexander's and Wilson's work are human behavioral ecology and evolutionary psychology. The bedrock of the evolutionary analysis conducted by scholars in these traditions is the concept of natural selection acting on genes. They argue that selection over the course of human evolution would have favored decision-making capacities, including decisions about what cultural behaviors to adopt, that increased genetic fitness. How could our large, complex, expensive brain have evolved to support human capacities for learning, including the learning of culture, unless the resulting behaviors increased fitness? Natural selection is the only process of design operating in the world, and the complex capacities of the human brain must therefore have arisen by its operation.

We call this the 'principle of natural origins.' In our view, the principle of natural origins is an exceedingly important idea. It has been attacked vigorously by critics from Darwin's time forward and has proved quite robust (Dawkins 1985). Most Darwinians no longer think detailed defense of it is necessary and just use natural origins as a metatheoretical precept to use to discover adaptations. That is, Darwinians very frequently use the principle of natural origins to formulate hypotheses about what would be adaptive if it is true, rather than testing the dominant role of selection as a hypothesis. This usage has famous critics among evolutionists not to mention anti-evolutionists (Gould / Lewontin 1979), but we are not among their number. The alternative metatheory of the evolutionist critics has not enjoyed much success (e.g., Carroll 1997) compared, say, to the universal Darwinism of Campbell (1965), Dawkins (1976), Dennett (1995), Cziko (1995), and Sober and Wilson (1998). Universal Darwinists see selection as producing adaptations on diverse heritable substrates, including culture, and at diverse levels ranging from individual genes and memes to groups. Some of the most exciting recent work in population genetics is that showing how wide a variety of Dawkins' selfish genes exist in the genome. Given selection falling at different levels or on different sexes, intragenomic conflicts of various kinds arise, giving adaptationism a neat, built-in theory of maladaptations (Rice 1994). Selection at one level can produce maladaptations at another. The creation of new levels on which selection might act occasionally lead to breakthrough adaptations like multicellularity, when formerly intensely competing individuals are welded into larger units (Maynard Smith / Szathmáry 1995).

Our problem is not with the principle of natural origins itself but with its persistent misapplication in the human case. Human sociobiologists with otherwise diverse beliefs have taken certain contingent generalizations from evolutionary biology on board as metatheoretical presuppositions to guide hypothesis formation that we believe should be left in the realm of hypothesis to be tested (cf. Miller 2000 for a view something like ours). Among the most problematical are: (1) we can deduce adaptations directly from what would maximize individual or inclusive genetic fitness, (2) cultural causes are always proximate, and (3) group selection plays no role in the evolution of human social institutions. We think the proper use of the principle of natural origins is *methodological*, not substantive. If culture itself has the attributes of an inheritance system, then it makes sense to apply Darwinian analytical methods to that system of inheritance as well as to the genetic and see where the exercise leads. Will cultural evolution generally lead to genetic fitness maximization? Can cultural variation itself create heritable variation on which selection can act? Can enough of this variation be expressed at the group level for group selection to be an important force? These are among the most interesting *hypotheses* we want to use the analysis to address and to imagine that the principle of natural origins dictates certain answers to them is, in the human case, to badly mis-locate the boundary of Darwinian metatheory and hypothesis. The human/chimpanzee comparative data on imitation, not to mention a mass other data indicating how important culture is in humans, makes importing the unvarnished adaptationist metatheory from evolutionary biology a very risky proposition.

Human behavioral ecologists start with the idea that natural selection ensures that humans act, to a decent first approximation, as general-purpose genetic fitness maximizers. Considerations of cultural evolution and gene-culture coevolution have a strictly secondary role, and for most practical purposes they can be neglected in the view of most human behavioral ecologists. As Alexander puts it, "Cultural novelties do not replicate or spread themselves, even indirectly. They are replicated as a consequence of the behavior of vehicles of gene replication" (Alexander 1979: 80). Or, as Betzig says in reaction to claims for the importance of culture: "[E]verything we think, feel, and do might be better understood as a means to the spread of our own – or of our ancestors – genes", and "I personally, find culture unnecessary" (Betzig 1997: 2, 17).

Very often the strategy of asking what behavior would optimize fitness leads to useful insights. For example, consider mating strategies. When should females mate polygynously with a male that already has a mate, and when should they seek an unmarried mate? In the case of species where males defend territories with resources on them, females should mate polygynously if the extra resources available on an already mated male's territory exceed those available on the best available unmated male's territory. Such 'polygyny threshold' models were first applied to birds and non-human mammals, and they often work quite well. Borgerhoff Mulder (1992) showed that one human population, Kipsigis farmers of Kenya, also followed the polygyny threshold model quite well. Women tend to select husbands on the basis of the land they can offer a new wife to cultivate rather than other criteria. The success of such models should not surprise us. Humans are a successful species and much of our behavior must be pretty adaptive most of the time to account for this success. At minimum, fitness optimizing models provide a convenient benchmark against which to judge competing hypotheses. But cultural evolutionary competing hypotheses exist! For example, the basic subsistence adaptations of humans have been evolving rapidly, relatively speaking throughout the history of our species. Most of these adaptations seem to have a large cultural component and how we get from one to another, optimally or not, is certainly of interest. To ignore our most dynamic system for achieving our adaptations on an 'argument' such as Betzig's is stubborn and willful ignorance!

A second important branch of human sociobiology is evolutionary psychology. The influential school of evolutionary psychology represented by the authors in Barkow, Cosmides and Tooby (1992) argues that fitness optimizing arguments are directed at the wrong target by human behavioral ecologists. The real adaptations to focus upon are the attributes of the mind that optimally adapted us to live in the Pleistocene environments of the past. Contemporary environments have changed so radically that it is vain to hope that behavior will be fitness maximizing today. Evolution is too slow to readapt the human mind significantly in the last few thousand years. The human mind is best conceived of as a collection of adaptations designed to solve specific adaptive problems of Pleistocene life, our 'environments of evolutionary adaptedness,' not a general-purpose fitness maximization system. (The fact that people are even more successful in the Holocene than the Pleistocene is puzzling on this argument, but the fact that we did evolve under Pleistocene conditions is likely important.) These scholars model the mind as a large collection of rather narrowly specialized content rich algorithms that solve a series of narrow problems. For example, human adaptations to the Pleistocene were social. To judge from contemporary hunter-gatherers and from archaeology, small bands of people collaborated to gain subsistence, with a great deal of sharing within and between the constituent families of the band. Bands were linked into a larger social sphere, the tribe among whom mates were sought and help elicited in emergencies. The exchange economies of even the simplest human societies are greatly expanded compared to ancestral primates. Among the adaptations to life in such societies must have been the ability to detect violators of complex social contracts.

Evolutionary psychologists want to use this Pleistocene-limited version of the natural origins principle to inspire hypotheses about evolved cognitive architecture that can be tested experimentally (Tooby/Cosmides 1989). As with the empirical program of human behavioral ecologists, the results of these experiments are often quite convincing. For example, the classic work of Cosmides (1989, cf. also Gigerenzer / Hug 1992) showed that humans are much better at solving logical problems posed as violations of social rules than posed as abstract logical problems, *and* better at solving the social rule problems than with other familiar, concrete content. Cosmides argues that this data is consistent with the hypothesis that humans' social adaptation has equipped them with a powerful innate mental organ for detecting cheaters.

The main problem, from our point of view, with this form of evolutionary psychology is again that the principle of natural origins has been misapplied. Now it seems to be licensing as metatheoretical assumptions the innateness of the important adaptations as well as fitness optimization (in past but not present environments). Several of the leading figures in evolutionary psychology are radical innatists who believe that the role of culture is greatly exaggerated by most social scientists. John Tooby and Leda Cosmides, for example, argue that social scientists have failed to distinguish between what they call *evoked* and *transmitted* culture (Thornhill et al. 1997: 230–234). Transmitted culture is what we call culture here, the product of human social learning. Evoked culture is the innate information that resides in human heads and which is expressed contingently in different environments. Tooby and Cosmides (1989) introduced the term evoked culture to make the point that innate mental organs can be environment-contingent rules, and hence can produce patterns of variation in space that would be difficult to distinguish from transmitted culture. As a hypothesis to explain any given pattern of human behavior, 'evoked culture' is a perfectly good candidate. No doubt, adapted genes play a large role in human behavior much along the lines such innatists suggest. For example the impressive rate at which we can encode and decode speech is the product of specialized auditory and motor pathways (Friederici 1996). In general, however, testing ideas about less peripheral aspects of speech processing and language learning, such as how grammar develops, has proven rather difficult, and hypotheses like Tomasello's (1999) giving a large role to transmitted culture are currently as viable as much more innatist views, such as those of Pinker (1994). Given that humans live in intensely social groups structured by culturally transmitted institutions, and given that culture and individual learning generally lead to adaptive behavior, the bare finding that people are very good at social tasks does not speak very loudly about the proximal causes of social behaviors. The innatist interpretation of the results of Cosmides' experiments seems to based upon the assumption that at least in the ultimate sense, the products of natural selection all reside in the genes on the principle of natural origins. This application of the principle at the psychological level makes no more sense than at the phenotypic. Experimental work by psychologists such as Nisbett, Cohen, and Tomasello shows that culture is an important part of human psychology and to attempt to marginalize it *a priori* is just not a good bet as a research strategy, much less a legitimate deduction from the principle of natural origins.

We think that psychobiology brings plenty of evidence to the table to rule out an extreme *tabula rasa* hypothesis but not nearly enough to rule out an important role for culture. Cultural scientists bring plenty of evidence to the table to rule out a strong version of the evoked culture argument but not nearly enough to rule out a detailed role for evolved innate mechanisms in the acquisition and management of culture. For example, even if the diversity of human behavior in space is explicable on the basis of only an innate human nature and environment, its diversity in time is harder to account for in this way. Over the last 10,000 years, human subsistence behavior and social organization have changed quite radically even though neither genes nor environments have not changed much at all. Even if almost all of the middle ground where the failure of the extreme hypotheses shows the real answers to lie is poorly understood, we know that they are not very close to either extreme.

In the remainder of this essay, the nettle of biology tightly in our grasp, we illustrate the consequences of taking both the principle of natural origins and the importance of culture seriously with two example hypotheses. The classic claim of mid-twentieth-century cultural ecologists (e.g., Steward 1955) was that the human adaptation has two basic components, technology and social organization. Humans adapt to environments by evolving elegant tools to exploit the most diverse sorts of resources the earth has to offer. Human adaptations are social. Human populations take advantage of the principles of cooperation, coordination, and division of labor to a degree otherwise only known among the social insects and a few other lineages. Even by the Middle Pleistocene we were an unusually widely distributed species and for the last 50,000 years or so we have been fairly abundant over most of our range. Let us imagine our nearly acultural chimpanzee like ancestors. What sort of selective pressures would have led to the evolution of accurate imitation of food-gathering strategies? What sort of adaptation is technology? Why is it rare? In this example, we stick to conventional sociobiological assumption that culture is a proximal system of adaptation. Even so, to understand how culture works as a genetic adaptation requires taking the properties of cultural evolution seriously. What of the evolution of the social component of our adaptation? How might we come to cooperate in groups composed of distantly related individuals? Evolutionary theory makes strong predictions about cooperation and the standard sociobiological theory well predicts all but a handful of cases. We are perhaps the most glaring exception, cooperating in large groups of distantly (genetically) related individuals. Our hypothesis is that natural selection has a stronger purchase on cultural than genetic variation and that the social component of our behavior is substantially the result of culture participating in evolution as an ultimate cause, not just a proximate one.

How Technology Works

The principle of natural origins encourages us to ask why natural selection might have favored our capacity for culture. The imitative capacity psychologists have described, and the cultural traditions the capacity it apparently supports, could only have evolved if they were adaptive. The capacity to acquire, store, manage and use technological practices is at least one of the functions of our large brain. Most accounts of human origins take our current ecological dominance as evidence of a qualitatively new and superior form of adaptation and ask what evolutionary breakthrough led to this revolutionary new adaptation. For example, Lumsden and Wilson (1981: 330) remark that "[*Homo*] overcame the resistance to advanced cognitive evolution by the cosmic good fortune of being in the right place at the right time." Our current ecological dominance is undeniable, although perhaps precarious, but the principle of natural origins encourages to ask quite detailed questions about just what selection pressures would have operated leading up to any breakthroughs.

Cultural Evolution is Fast and Cumulative

The human brain is a serious adaptive puzzle. It is a very costly organ (Aiello / Wheeler 1995). Human brains account for about 10 percent of our total energy budget versus something like 1.5 percent for average mammals. Aiello and Wheeler argue that one consequence of our expensive brain is that to pay its overhead we evolved a smaller gut (gut tissue is also costly per unit weight). A short gut means that we have to eat more energy-intensive foods than our ancestors. A costly brain and a short gut meant that humans had to hunt, gather, and conduct their social life with some efficiency to support their brains under quite hostile physical conditions in competition with other predators, scavengers, and plant eaters with much more economical brains and more efficient guts. At least during the last glaciation, climates were not only colder, but drier and much more variable than during the Holocene. We believe that culture is most likely an adaptation to the Pleistocene climate variation (Richerson / Boyd 2000). During the last glacial, and by inference during most of the rest of the Pleistocene, climate did not vary only the 100,000 year time scale of the classic ice ages. Climates were also spectacularly variable on time scales ranging from a few years to a few thousand years. For example, from 80,000 to 10,000 years ago was punctuated more than 20 abrupt (~1° C per decade!) warmings to about half of interglacial temperatures, not to mention considerable variation at both shorter and longer time scales (Ditlevsen et al. 1996; Broecker 1995).

Our mathematical modeling studies show that a likely adaptive advantage of culture is the ability of this system of adaptation to respond more rapidly to changing environments better than genes (Boyd / Richerson 1985). This ability comes from coupling adaptive decision-making systems to the transmission system made possible by accurate, fast imitation. Take the two simplest kinds of models. One feature of culture is that it is a system for the inheritance of acquired variation. Individuals can imitate the behavior learned by others. If the rules that guide learning tend to be adaptive, then two forces, natural selection and learning, act together to favor the accumulation of adaptations. In the world of models at least, this system is especially suited to adapting to environments that vary a lot, but with an appreciable, but not too large, resemblance between parents' and offsprings' environments. If environments vary too fast, then Mom's and Dad's behavior may be out of date, and individuals should learn for themselves. If the vary too slowly, selection on genes keeps up well enough, and the costly overhead of brain tissue consumptive culture weighs against it. The Pleistocene was rich in just the kind of variation that favors the inheritance of acquired variation.

A second trick we can do with culture is use pre-existing cultural variants rather than our own random trials or inventions. Suppose we observe not only how Mom gathers, but also the techniques of several other gatherers. Suppose we observe two or three variants. As we begin to practice gathering we can try each variant a few times and retain the one that seems best. Further, throughout our life we may continue to observe and try out any likely variant techniques that seems promising. Depending upon how accurately people can discriminate among different techniques and on how many varying techniques one has an opportunity to observe, the biasing of imitation can be a weak or powerful force.

The neat result of the models is that even when decision-making effects are *weak* at the level of individuals, they can be *powerful* at the level of the population. This finding is closely related to the fact that natural selection is a powerful force at the population level even when so weak as to be impractical to measure at the individual level. When any directional force acts in the same direction in an entire population and consistently for more than a few generations, the evolutionary response is swift. For selective forces to operate including both biased imitation and natural selection, variation to select upon must exist. However, coupling individual learning to social learning means that trial and error learning can act as a source of new, generally partly adaptive, variation.

We believe (Boyd / Richerson 1996) that the evidence suggests that our adaptive success also rests decisively on our ability to create cultural adaptations that can accumulate complexity, eventually coming to rival genetic adaptations in the sophistication of their 'design.' Even relatively sophisticated social learners like chimpanzees get only a very general idea of a behavior using social cues. Using this general idea, they refine their actions to a functional behavior using individual learning. This limits the complexity of the socially learned behavior to that which can be supported by individual learning at the individual level. The human ability to imitate accurately means that we can adopt the precise variant of a previous innovator, perhaps tracing back to some long-dead genius, and then add a new wrinkle of our own, which can in turn be imitated and improved by our successors. Eventually human populations heap innovation upon innovation until we reach the limits of human minds to be taught the result. Even the cultures of simple societies accumulate far more genius than even the most brilliant individual innovator could muster. Most likely, the invention of language increased the number and sophistication of abstract concepts we could learn. In simple societies, memory places limits on complexity that more recently have been relieved by the invention of writing and numbers (Donald 1991). At the cutting edge, we again push right up against human cognitive limitations. Most of us now live by skills dearly won in classrooms by great mental exertion on both our and our teachers' parts. The relative rapidity with which we could build up and adaptively modify complex technology is one leg of the adaptation allowed us in the Pleistocene to chase the ephemeral niches left under-exploited as other species lagged behind the kaleidoscopic changes in resources caused by rapid climate change. In the Holocene, the invention of agriculture gave us the tools to deteriorate the environments of competing and pest species faster than they could adapt to our modifications (Richerson et al. 2001).

Thus, we suppose that the environmental deterioration of the Pleistocene is the specific environmental factor that humans exploited to support their large, costly brains (Richerson/Boyd 2000). Interestingly, many mammalian lineages show increased brain size in the Pleistocene. Other species may also have been using social learning to adapt to variable environments. However, no other mammalian species has developed the ability to use rapidly evolving complex tools to exploit variable environments. Probably, our bipedal posture, by freeing the hands to specialize in creating and using tools, was a decisive preadaptation (Tobias 1981). Coupling the capacity to imitate to the capacity to make tools allowed us to rapidly develop adaptations that would otherwise have required slow anatomical modifications. Lacking a flexible way to implement a diversity of cultural adaptations, no other species came to support such a radically enlarged and costly brain.

The promise of explicitly modeling and measuring the processes of cultural change is immense. For example, why has the Holocene witnessed a 10,000 year long raggedly progressive trend to fancier technology and larger societies? What currently regulates rates of change in various components of various cultures? Are current anthropogenic climate changes likely to stress our ability to adapt to them? Ice age climates will presumably return. Can complex societies adapt enough to cope with the very noisy climates that have prevailed during the last couple of million years? The extraordinary dynamism of human societies means that understanding our species using assumptions about equilibrium adaptations to given environments will be less productive than in other cases (Nelson / Winter 1982).

Why Humans Are Ultra-Social

Many critics of the orthodox schools of human sociobiology have argued that the problem is that these investigators leap to adaptation without considering the complexities raised by development. Our critique above is of this form if we take social learning to be a form of developmental process linking the evolving genes to the adaptive phenotypes. While true, this objection bites less sharply than it might otherwise because adaptationists commonly, and commonly successfully, neglect the details of genes and development when studying the evolution of adaptations. The tactic of taking genes and development lightly in the hope that progress can be made without needing to understand proximate causes is called the "phenotypic gambit" (Grafen 1991). The phenotypic gambit is generally necessary when one studies adaptations. Development is a complex and difficult topic all its own, and usually the only practical way to proceed is to assume that selection has managed the developmental processes well enough that adaptations close to what we'd predict from gross functional considerations. We endorse the judicious use of the phenotypic gambit; if *we* can't use it, we'd have to wait until developmental psychologists have delivered a Mercedes model of the imitation process rather than a pick-your-own collection of Amsterdam bicycles. Related scientific programs typically have to cope with weaknesses in their partners and with the intimidating complexity of even well known phenomena. The phenotypic gambit and allied strategies are necessary to finesse ignorance and complexity.

A critique that bites deeper is that human sociobiologists have generally neglected the ultimate role culture has played in human evolution. The coevolutionary concept of an ultimate-cause role for culture is very simple. Culture, like genes, creates patterns of heritable variation. Natural selection will inevitably play upon any pattern of heritable variation that arises in the world as Richard Dawkins (1976) noticed and Donald Campbell (e.g., 1965) had argued earlier. If cultural variation can respond to selection it is just as ultimate a cause as genes! Of course, culture does not stand in isolation; it lives in brains and is no doubt heavily shaped by influences having their roots in genes and selection on genes. But the proximal causal arrow runs both ways, as we've already seen. Our psychology is shaped by our culture. Culture acts as a selective environment to which our genes will, in the long run, adapt. The term coevolution classically derives from the interacting evolution of pairs of species like predators and prey, diseases and hosts, and mutualists. In the present case we imagine that our culture is something like a symbiont. It lives in the same body as our genes, but has a different life cycle and thus responds somewhat differently to evolutionary forces. In our species, culture and genes are obligate mutualists – an individual cannot even survive without tolerably good genes and tolerably good culture.

We hope that the gene-culture coevolutionary idea seems perfectly intuitive to most of our readers. Be warned, however, that you are being invited down what many evolutionary social scientists believe is a garden path. The issue is whether or not gene-culture interactions in humans are *fully* or *only partially* coevolutionary. The more prominent hypothesis is that the gene-culture system is a degenerate example of coevolution. Genes have no doubt evolved to constrain the evolution of cultural variants in ways that favor the fitness of the evolving gene. This dynamic is what Charles Lumsden and Edward O. Wilson called the "full coevolutionary circuit" (Lumsden / Wilson 1981: 303). They emphasized evolution of evolved genetic 'leashes' on cultural evolution. We think Lumsden and Wilson's dynamic is incomplete because selection also exists on the cultural variants *and thus evolved cultural institutions can cause changes in the genome that favor cultural fitness.* Culture is on a leash all right, but the dog on the end is big, smart, and independent not a well-trained toy poodle. On any given walk, who is leading whom is not a question with a simple answer (cf. Durham 1991: 223–225 for a similar argument).

Mechanisms by which culture might exert forces tugging in this direction are not far to seek. Cultural norms affect mate choice and people seeking mates are likely to discriminate against genotypes that are incapable of conforming to cultural norms (Richerson / Boyd 1989). Men who cannot control their testosterone storms end up exiled to the wilderness in small-scale societies and to prison in contemporary ones. Women who are an embarrassment in social circumstances are unlikely to find or keep husbands. We believe that with, at minimum, tens of thousands of years to work with, natural section on cultural variation could easily have had dramatic effects on the evolution human genes by this process. Some of these effects no doubt just energize Lumsden and Wilson's limb of the coevolutionary circuit, favoring better genetic leashes. Humans are still in part a wild animal; our cultural adaptations often still serve the ancient imperatives of genetic fitness. However, we think the evidence supports the hypothesis that the coevolutionary circuit is 'doubly full.' The leash works both ways. Humans, we might say, are a *semi-domesticated* species. *Cultural imperatives are built into our genes.* Not only can culture act proximally to constrain behavior via institutions, skills, values, and so forth, but by constraining behavior in similar ways over hundreds of millennia it is a major source of ultimate causes of human 'nature.'

Group Selection on Cultural Variation Selected New Social Instincts by Coevolution

The other major leg of the human adaptation is our complex social organization and our form of social organization is potentially a result for selection on cultural variation and coevolutionary adjustments on the genetic side. The residential bands that most ethnographically known hunter-gatherers lived in are only a little larger than those of chimpanzees (Dunbar 1992), but human social organization includes a tribal level that is unique to our species. In the simpler human societies, typically several residential units, numbering a few hundred to a few thousand people, speak the same dialect, participate in a common ceremonial system, maintain a level of internal peace and security against hostile groups, and aid one another in subsistence emergencies.

Other ultra-social animals, including to one other mammalian example, the naked mole rats of Africa, are based upon creating large societies by multiplying the number of close genetic relatives. The creation of reproductive and sterile castes in the social insects offers examples of several independent origins of this system. Humans have taken a quite different route to ultra-sociality (Campbell 1983). As Campbell observed, human societies have reproductive competition among the cooperators, leading to societies that exhibit considerable self-sacrificial altruism (e.g., heroism in war) and considerable within-group conflict (e.g., feuding). Some societies exhibit *both* extremes of warrior self-sacrifice *and* of extremes internal conflict rooted in sub tribal scale loyalties, a trick that seems to defy the evolutionary law of gravity (Hamilton 1964) as it applies to all other species. The proximal mechanisms by which cultural institutions can harness phenomena like Southerners' touchy sense of *personal* honor to functional large-scale organizations, like the excellent armies of the Confederacy in the American Civil War, are tolerably well understood (Boehm 1984; Salter 1995).

We have proposed what we call the "tribal social instincts hypothesis" to account for our peculiar pattern of social organization (Richerson/Boyd 1998, 1999, 2001). The tribal social instincts hypothesis is based on theoretical analyses suggesting that group selection plays a more important role in shaping culturally transmitted variation than it does in shaping genetic variation. In our simplest model of the process, we imagine that humans come to use conformist biases in acquiring culture (Boyd/Richerson 1985: chapter 7, cf. also Henrich/Boyd 1998). Conformity is adaptive under a wide range of conditions because the commonest thing people are doing in a given environment is frequently a very good thing to do relative to most easy-to-discover alternatives. When in Rome, do as the Romans do. As a *byproduct*, conformity has the effect of preserving between group variation and suppressing within group variation. Most evolutionists doubt that group selection on genes is very often important because it is so hard to maintain variation between groups, particularly variation for traits such as altruism that are selected against within groups.

Almost everyone agrees that human material culture was of essentially modern levels of sophistication by the Upper Paleolithic, 50,000 years ago (Klein 1999). Even if the cultural group selection process did not start until the Upper Paleolithic Transition 50,000 years ago, human minds have been selected for 2,000 generations in social environments in which the innate willingness to recognize, aid, and if necessary, punish fellow group members was favored by co-evolution. That is, cultural group selection produced traditional institutions that penalized genotypes that were hewed too tightly to individual selfishness, Hamilton's kin selection rules, or to reciprocity strategies to deal with non-relatives. If cultural institutions can generate sufficiently costly punishments for deviations from their rules or provide the benefits of group cooperation mainly to cooperators, any genetic variation underlying behavioral dispositions will fall under selection favoring genotypes that avoid the punishments and earn the rewards. We suppose that the resulting tribal instincts are something like principles in the Chomskian linguists' "principles and parameters" view of language (Pinker 1994). The innate principles furnish people with basic predispositions, emotional capacities, and social skills – the principles – that are implemented in practice through highly variable cultural institutions – the parameters. People are innately prepared act as members of tribes but culture tells us how to recognize who belongs to our tribes, what schedules of aid, praise, and punishment are due to tribal fellows, and how the tribe is to deal with other tribes – allies, enemies, and clients.

Because the tribal instincts are of relatively recent origin and because our genes still fall under selection pressures obeying Hamilton's rule, they are not the sole regulators of human social life. The tribal instincts are laid on top of more ancient social instincts rooted in kin selection and reciprocal altruism. These ancient social instincts conflict with the tribal. We are simultaneously committed to tribes, family, and self, even though the conflicting demands very often cause us the great anguish as Freud (1930) described in *Civilization and Its Discontents* or Graham Greene portrayed in novels such as *The Honorary Consul*. So long as reproductive competition among the cooperators exists, people still have to look out for their personal fitness interests even as they try to do their civic duty.

We (Richerson/Boyd 2001) argue that a considerable mass of evidence from a number of domains of knowledge supports that tribal social instincts hypothesis and calls into question competing evolutionary explanations. Nevertheless, much more work needs to be done before any hypothesis regarding the evolutionary origins of human sociality should be accepted as well verified. What we do claim on the basis of the evidence we review is that the tribal social instincts hypothesis, with its active, ultimate role for the process of group selection on cultural variation, is at least as attractive as any current competing hypothesis.

Conclusion

The fast and cumulative hypothesis to explain the original adaptive advantage of imitation in humans is a straightforward application of adaptive analysis. It is a simple argument from the principle of natural origins. However, if it or hypotheses like it are true, culture plays, and has long played, a central role in human evolution and cannot be marginalized. For example, the time scale of cultural evolution is rapid, but not instantaneous. Indeed, 10,000 years after the end of the last big shift in the earth's environmental regime, the Pleistocene-Holocene transition, human cultural change has apparently not equilibrated. The processes of cultural evolution are fundamentally important to understanding human behavior but are comparatively little studied, especially with sophisticated quantitative methods.

The coevolutionary tribal instincts hypothesis, if it or anything in its genre are correct, means that coevolution with culture has driven the evolution of genes in directions genes would never have gone, left to their own devices. Cultural institutions achieved the tribal (and now larger) scale of organization by partly domesticating genes. The human achievement of ultrasociality seems to be one of those rare evolutionary transitions where a new level of organization emerges because some form of group selection, no doubt always tenuously in the beginning, unites previously fiercely competing entities into a larger scale cooperative system (Maynard Smith/Szathmáry 1995). This hypothesis is also perfectly consistent with natural origins. Large scale human societies are (so far) extraordinarily successful because they, on average, increase the fitness of both genes and culture, quite like other successful coevolved mutualisms.

The principle of natural origins is the fundamental building block of Darwinian metatheory. We have no competing metatheory that has much promise of giving us a truly deep and synthetic theory of human behavior. The trouble is not with the principle but its misapplication in the human case. It especially does not imply what cultural scientists have come to fear, a trivialization of the role of culture in human behavior. Culture, its evolutionary processes and coevolutionary effects are all straightforward topics for Darwinian investigation. A mass of evidence argues that we cannot understand human behavior without doing culture right. This same evidence argues that using concepts like the superorganic to separate the study of culture from the rest of human biology is equally flawed. The superorganic concept was a tribal ploy used by twentieth-century social scientists to create and maintain disciplinary boundaries with biology (cf. Campbell 1978 on the functions and dysfunctions of disciplinary boundaries). If we are correct, it never served a truly useful analytical role. Whatever useful function the concept and its boundaries served in the twentieth century, they are now utterly senescent. The task for twenty-first-century human science is to put culture back into human biology.

Culture operates through biological mechanisms – brains, hormones, hands – and the causal pathways by which it acts are certain to prove densely tangled with genetic causes. The difficulty we have in following the threads of genetic and cultural influences on human behavior is the best evidence we have on this point. If the relationship between genes and culture were simple, the case would have been cracked long ago. Scientists should not be faint-hearted in the face of complexity if that is where the real problem lies. Darwinism is rich in techniques for making progress in the face of intimidating complexity. The last 'tangled bank' paragraph of the *Origin of Species* is a lyrical passage that combines a downright mystical appreciation for the complexity of nature with a scientist's optimism that useful understanding is possible nonetheless. The extremes superorganicism and innatism are useless simplifications that lead human scientists to avoid the hard but central problem of the human species, the natural origin of the cultural system of inheritance and all the things that people can create because their biology includes the capacity for imitation.

Cultural scientists should not be timid about being reunited with biology. Culture is a brawny phenomenon in no danger of being 'reduced' to genes. Evolutionary biologists should not be timid about welcoming cultural scientists either, as biologists command the methods cultural scientists neglected because superorganicism especially stigmatized Darwinism. All sorts of borrowings and interchanges across the biology social science divide are likely to prove fruitful (Weingart et al. 1997). The only people with legitimate reason to fear a unified human biology with culture and genes playing their appropriate roles are those who want easy answers to hard questions.

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Climatology

Innovative Research Strategies in a Dynamic Field

For a very long time meteorology has been a rather inconspicuous scientific field. The development of theories, methods and findings did not show a different pattern from most of the other fields of 'normal science.' Within the last two decades, however, this situation changed very much. The discovery of the ozone hole and of the global warming process were the two main topics which caused an enormous public awareness on climate research. Top scientific journals like *Nature* and *Science* published several relevant articles in the field with strong impact, far beyond the community of meteorology. An Intergovernmental Panel on Climate Change (IPCC) has been set up under the auspices of the World Meteorological Organization (WMO) and the United Nations Environment Program (UNEP). Since 1990, the panel publishes every five years a report (IPCC 1990) to access the most up-to-date research on global warming. The production of the report is organized as a combined effort of some 45 scientists, reviewed by hundreds of other researchers and 150 governments. The panel is "considered the most authoritative voice on global warming" (NYT Editor 2000). The global warming forecasts of climate scientists have been heavily debated, among scientists as well as politicians and the general public. The topic arrived at the front pages of news magazines. Especially the discussion about the anthropogenic factor in global warming has become one of the major scientific controversies in the 1990s. Thus, it is not astonishing that sociologists of science discovered climate change as an interesting 'object of study' (cf. Weingart, Engels, Pansegrau 2000). Climate research, in a way, combines various aspects that are of particular interest for science studies, in general:

- it is a highly interdisciplinary field, sharing knowledge of meteorology, (geo-) physics, atmospheric chemistry, biology, oceanography, environmental science, computer simulation, geoscience, etc.
- it is a highly dynamic, rapidly developing field, attracting much public funding

– it is basic science with an evident potential of application to the needs of mankind
– it stimulates a controversy in the arena between science, politics and the mass media

The notes of Aant Elzinga as a participant observer on climate research in Antartica offer insights into the complex dynamics of the research process in this field. With his article – partly analytic, partly impressionist – he takes the reader directly to the research front, right into the ice.

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Making Ice Talk: Notes from a Participant Observer on Climate Research in Antarctica

Aant Elzinga

Champagne on Ice

In the dark of the ice-cellar we opened the champagne bottles. The faces of our party of ten light up as cameras flash, and the ceiling becomes visible. The bottom-side of the plain plywood slabs above us are laid out on a series of wooden ribs stretched across a two-and-a-half meter deep chasm dug out in the snow. It is our ice laboratory; makes a hardly noticeable local dent, an anthropogenic singularity in the topography of the vast pristine expanse of a polar snowfield extending in all directions to the blue canopy of the polar horizon. Directly over our heads, over the plywood roof lies a plastic sheet. On top of that a decimetre of hand-shovelled loose snow provides insulation from the radiant sun which constantly circles about in the sky above 24 hours a day.

It is in the middle of January 1998. Outside at minus ten, with the sun directly overhead, it is relatively comfortable, but here inside the ice lab, our natural cooler, the temperature has to be kept below minus 20 degrees centigrade. Otherwise the ice cores we have been collecting will 'forget' their past (if they melt) or distort their d 18-Oxygen information when analysts back home put them to the isotope test in the refrigerated laboratories of climatological research centres, be it in Stockholm, Copenhagen, Utrecht or Grenoble.

So begin some notes made as a participant observer during the Swedish Antarctic Research Programme (SWEDARP) in the austral summer polar research season of 1997/98. The expedition landed on the sea-ice off the west coast of East Antarctica by the Weddell Sea far below the southern tip of Africa in mid-December 1997 and left again from the ice shelf near the same spot towards the end of February 1998. By that time we had collected three-and-a-half tons of ice cores. Since the thermostat and motor on our sledge-borne freezer container had become unreliable this valuable cargo was moved and loaded before anything else, ferried by helicopter over to the ship for immediate storage in a sturdier freezer. Then seven days sailing over stormy seas northward back to Cape Town, whereafter we took a plane, while the ice cores were transferred to another ship headed for Bremen, and thence to Stockholm for re-distribution to a number of European research centers. Considering the accumulating cost incurred it is no exaggeration to say that upon reaching its destinations the ice cargo was in fact worth half its price per gram in gold.

In order to convey some of the problems and tensions arising in an Antarctic expedition the present essay takes extracts *(in written style)* of reports from the field sent to the homepage of the Swedish Polar Research Secretariat, and interlaces these into the present account. The purpose is to sketch a few local epistemological, historical and geopolitical co-ordinates pertinent to international research on global climate change.

In Search of a Site for EPICA

The occasion for champagne in our natural ice chamber at the drilling site on Amundsenisen's high polar cap plateau January 1998 was to celebrate the ice drill just having taken up a core from 100 meters below the surface. One hundred meters here touches strata of annually accumulated snowfalls (precipitation) from 600 to 700 years ago. This is the age of the ice with which we spiked our precious drink that day. Before we were finished we would be breaking into the previous millennium, in as far as we finally stopped at 135 metres (equivalent to ca. 1,100 A.D.). Our location, baptised 'Camp Victoria' (after the Swedish Crown Princess), was 76° 00' S, 8° 03' W, i.e., deep into the interior of the territory called Dronning Maud Land (DML).

Much of the rationale for the Swedish expedition 1997/98 is related to the problem of global warming. The ice coring operation in particular is a contribution to a European project funded by the EC and co-ordinated through the European Science Foundation, viz., the European Project for Ice Coring in Antarctica (EPICA). EPICA is one of the success stories when it comes to placing Europe on the world scientific map (paleoclimatology) in competition with the US and Japanese. Our own operation in DML was only one of several pre-site surveys (Holmlund 1998: 37–45; Näslund 1998). A multinational team of ten persons was despatched from the Swedish base Wasa near the coast, through a broad string of jagged nunataks (mountain peaks sticking up through the ice cap) to Amundsenisen. It included Swedes, a Dutchman and Norwegians. A separate Dutch project with two other researchers and a technician stayed partway, at the little Swedish summer station Svea in the Heimefrontfjella mountain range, to study meteorological factors pertinent to climate change (Bintanja 1995 and 1999).

> *24 Dec: One of the Hägglund tracked vehicles broke down on the way back from Svea to Wasa and had to be dragged / by the other / on a sledge. It was a broken axle that caused the mishap out in the field on Christmas Eve.*
>
> *26 Dec: The big question today is whether or not we can start the EPICA traverse earlier. The warm weather has put a stop to drilling / in the blue ice area / at Scharfenbergsbottnen.*
>
> *29 Dec: The work with organising the earlier departure for Svea and then onto the traverse on Amundsenisen is progressing smoothly. Today much of it has revolved around digging out and repairing a sleigh, fixing the windows and interior of an old living module that had filled with hard-packed snow, getting fuel needs sorted out for different legs of the trip, etc. Different functions in an expedition, scientific, logistical, medical, and finally, minimising the immediate environmental impact of our own presence on this continent – tend to pull in different directions. A clear-cut differentiation and definition of tasks and responsibilities is essential in this respect … Per Holmlund is continuing with the debugging of the radio echo equipment … After the delays in ice coring due to warm temperatures in the air / at Svea / now new problems have cropped up; technical difficulties with the drill requiring contact with Robert Mulvaney who is with the Halley (British Antarctic Survey) group that will be doing coring slightly south of our own EPICA-effort. Mulvaney has been involved with the redesign of the drill that is being used, and hopefully he can provide some advice on the current situation.*

In addition our expedition included environmental impact studies carried out by the Environmental Officer, who also remained at Svea, together with a photographer. British and German-led expeditions had further teams simultaneously working, respectively, at angles one degree of latitude south and one degree north of us, carrying out similar pre-site surveys. The eventual outcome of all these efforts were later subjected to scientific and logistical evaluations in order to determine where to situate a second leg of EPICA with deep-drilling down to 2 km starting in the year 2001.

The first leg of EPICA, at Dome Concordia (Dome C) far into the interior above the French station Dumont d'Urville is already into its third year. The climatic conditions at the two sites, Dome C and the future DML differ from each other. Dome C has less precipitation while DML receives more snow and hopefully will reveal important information about the 'signal' from changing past conditions over the Southern Atlantic. The latter is significant in discussions on changing ocean circulation regimes, coupling between northern and southern hemispheres, and the recent re-constructions of rapid climate change events at tail end of glacial periods (for a presentation of EPICA cf. Elzinga / Krueck 1999).

Epistemology: The Life and Purpose of Ice Cores in Shaping Climate Scenarios

Ice coring in Antarctica has had a dramatic impact on discussions regarding an enhanced greenhouse effect and uncertainty in projections of human-caused climate warming (cf. Street-Perrot / Robert 1994: 47–68; Graedel / Crutzen 1993: 223–229). This was especially after Claude Lorius and his French and Russian colleagues at the former Soviet research station Vostok in a most inaccessible place deep in the heart of Antarctica drilled a couple of kilometres into the icecap to bring up part of a "natural archive" (Lorius et al. 1985). Using ice of different ages, temperature trends can be built up. The now famous Vostok core was used to reconstruct a 160,000-year history of temperature variation, including a complete interglacial-glacial cycle (Jouzel et al. 1987; Legrand et al. 1988).

In their analysis the scientists claimed to 'see' warm temperatures in the interglacial period, about 120–130,000 years ago, and in the present interglacial of the past 10,000 years. Between these two periods, it is claimed, temperatures were more than 6° C colder than those experienced today. The analysis also points to temperature fluctuations in tandem with changes in volumes of greenhouse gases, CO_2 and methane, in trapped air over a period of 160,000 years. CO_2 content was found to be higher in interglacial periods, averaging around 260–280 parts per million (ppm), and lower during glacial times, averaging 190–200 ppm, and as low as 180 ppm. Note that it is now over 350 ppm. The methane content was 0.35 ppm during the glacial maximum, and about 0.65 ppm during the last 10,000 years until the nineteenth century. Now it is 1.7 ppm. Lorius et al. also did a climate sensitivity analysis and argued that the responsiveness of the climate system to the heat-trapping character of the greenhouse gases of the atmosphere is high (Lorius et al. 1990). They attributed about 5° C temperature difference between the last ice age and the interglacial period to the heat-trapping of greenhouse gases, and the rest to changes due to other causes. Furthermore they believe that the switches between ice ages and interglacials may have been triggered by the weak orbital variations (of the Earth around the Sun) and amplified by changes in greenhouse-gas concentrations.

Ice cores were thus enrolled as witnesses to tell us why we humans should be concerned about the enhanced greenhouse effect. In the late 1980s this spurred further deep drilling by several countries, and gave other ice coring efforts in Antarctica, including EPICA, considerable impetus.

Important studies of the transition from firn to ice in Antarctica were already made in the early 1950s by members of the Norwegian-British-Swedish Expedition who were the pioneers in DML (cf. below). At relatively shallow levels down to 60–80 m. the snow is consolidated as 'firn,' which is still porous, so that younger air seeps in from above. Below this, when the firn is further compressed and turns to pure ice the pores close, finally trapping the air. Therefore the trapped air to be analysed for greenhouse-gas and other telltale traces is younger than the ice that encloses it. Traditionally it has been assumed that in lengthy records the difference in age between the air and the ice that encloses it is not significant in constructing trends.

For mid- and high-latitude precipitation and mean annual temperature in polar regions there has thus been a simple formula according to which d 18-O regularly decreases by 1 per mil every time the temperature drops by 1.5° C when going across the ice-field, e.g., from coastal to Central Greenland. This relation has been used as a paleothermometer to translate information on changing 18-O isotope ratios at different depths into variations of temperature over time. Now the formula has been found to collapse for short periods of apparent rapid climate change recorded in the 'natural archive' of the Greenland ice sheet. As yet this has not affected interpretations of the Antarctic ice archive which appears to be less affected by seasonality, but it does call for caution in conceptual reconstructions of key correlations.

Recently, then, interpretative flexibility in the calibration of paleothermometers used to probe the ice record has become the subject of some discussion amongst leading scientists. This is in connection with the new problems thrown up by the apparent rapid fluctuations (so-called Dansgaard-Oerschger events after their 'discoverers') in ancient climates at crucial times in transitions from ice age to warmer interglacial periods. The last twenty years' traditional assumptions of fairly simple linear correlations between deuterium and oxygen-18 isotope ratios and atmospheric temperatures near polar plateaus at times of past precipitation are in cases of abrupt climate change being revised. To make sense of thermal anomalies, isotopic analysis of other gases (in this case 15N / 14N and 40Ar / 36Ar ratios) are now brought into play in much more sophisticated calculations that take into account differences between the age of the trapped air bubbles and the age of the surrounding ice matrix containing them.

Jean Jouzel, who currently heads the overall organisation of EPICA, summarises:

> In using this / the aforementioned traditional / relation as a paleothermometer, researchers have assumed that the present-day spatial relation does not change with time; that is, spatial and temporal slopes are assumed to be similar. Simple models show that this assumption holds only if such factors as the evaporative origin and the seasonality of precipitation remain unchanged between different climates, which is not at all guaranteed. These limitations have long been recognised and examined through simple and complex isotopic models (Jouzel 1999: 910).

Surprisingly the more sophisticated analysis also seems to lend itself to what may be rhetorical overtones in a debate regarding the relative neglect of climatological studies that have a different geopolitical frame of reference. In concluding his review of the revised analysis of events punctuating the glacial period 14,650 years ago, and the rapid onset of warming, Jouzel says, "this finding constitutes a breakthrough which will be extremely useful for deciphering mechanisms of abrupt climate changes and already suggests a North Atlantic rather than a tropical trigger for the climate event" (Jouzel 1999: 911).

There is much here to warrant a closer study of the social epistemology of paleothermometry in European laboratories. However that is not the purpose of the present account. Instead I want to home in on the study of life in the (ice-)field, the stage before the laboratory and gas analysis. It is the stage in which ice goes over from having been a facet of nature in its own-right to becoming an object-for-us, to be interrogated by humans, i. e. as ice-'core.' In as far as it is forcefully pulled up in cylindrical lengths of a given diameter, the 'recovery' as it is called – of the ice core – is essentially a human effort, depending for its success on costly logistics. It is also constrained in practice in the field by attention to factors both of human safety and environmental protection.

Life in the (Ice-)Field

While our expedition was meant to facilitate a future probe of the time-scale by which ice ages are measured, time-wise we ourselves were also situated at the front end of a long chain of complicated scientific events and processes (Sigg et al. 1994). The task of the team was to bring up ice cores, to coax and transport the objects to be interrogated to the laboratories back home. In this context ice is far from being a passive or malleable entity; it is capricious and puts up plenty of resistance. Lots of things 'go wrong' in the field; trial and error is a prevalent factor in many different dimensions of an expedition.

Other programmes inserted in an expedition also vie for due time promised participants, sometimes in bilateral agreements with groups of scientists in other countries, which may cut into time or create haste for the coring operation. The constraints, both natural and human are many.

Work in the field precedes the complicated process whereby chemical analysis of ice samples and air trapped in bubbles in the Antarctic ice is used to reconstruct past climate trends on our planet. Glaciological field-work is only one facet of climate-related research that takes place in the world today. It is time consuming, sometimes adventurous, and less visible than the more dramatic statements made by the gas analysts or computer modelers. But the trend analysts for their work need the materials that the humbler field workers make available. It is the stuff from which ultimately are constructed proxy accounts of parallel changes in atmospheric temperature and greenhouse gases as represented in elegant graphs published by leading scientific periodicals. By itself ours was not the kind of laboratory work that results in agenda-setting papers in *Nature* or *Science*; it only linked into these via the more sophisticated analysis back home.

On the other hand an expedition is no longer what it used to be in the old days. High tech has also made its entry in the field, with the domination of logistics now by sophisticated artificial life support systems on which the Antarctic researcher must rely. For example, there are no longer any dogs around, they are prohibited as an environmental hazard factor on Antarctica because they may infect seals with a contagious disease. Transportation instead involves light snowmobiles that can draw sledges, but also much heavier track vehicles than the old wartime weasels. Base stations are modern, sometimes with solar panels to generate electricity. Helicopters are the rule, and regular contact may be maintained with neighbouring stations by scheduled radio transmissions, and with persons and agencies back home via both satellite telephone and e-mail. Some of the rugged edge nevertheless remains, making fieldwork quite different from laboratory life.

In the history of southern polar science the development of new technologies has always played a key role, as have economic and political interests. These contingencies carry institutional motives (Elzinga/Bohlin 1993). Today overt economic motives relating to resource exploitation have been pressed back by monitoring of the environment and efforts to tease out anthropogenic from natural factors of climate change. At the same time national prestige and politics as motive factors remain and receive considerable play in the formation of Antarctic research agendas. To appreciate this and other contextual aspects it becomes relevant to consider some historical background.

Science as Politics and Politics as Science in the AT-Regime

The Antarctic continent is roughly the size of the US and Mexico taken together. Extremely inhospitable for human habitation, the continent has not witnessed the regular kind of colonisation whereby Western nations gained a foothold elsewhere on the globe. As a rule a group of humans wanting to spend some time there have to bring with them their own artificial life support system. Except for certain coastal regions where some explorers have managed to survive in primitive stone huts and used seals and penguins for meat, fire and lighting the wicks of their oil lamps, everything needed to keep alive and move about has to be brought in from afar.

> *30 Dec: Packing is the big issue today, with departure on New Years day creeping ever closer. Per's scientific equipment in five or six boxes of varying sizes does not take up as much room compared to all the other stuff needed to support our presence and movement on and across the vast rigorous icy stretches. Antarctica is a craving continent. It levels out everything before it, reducing to a common denominator of cold and sluggishness. Water freezes, metal cracks, bodies and machines slow down and strain. Counteracting this requires lots and lots of energy, in various forms. Ninety eight percent of this has to be brought in from other continents: solar electricity from our solar panels on the house and sledge-based living modules is an important exception … Packing just now for the three-week EPICA-effort in the interior has involved loading and securing: heavy equipment, spare parts, 2 snowmobiles with small sledges, 390 kg liquified petroleum gas for various heating needs, 30 barrels of diesel fuel (200 kg/barrel) for the two Hägglund vehicles (to be replenished by 10 further barrels during stopover at the Svea station), and 10 gasoline drums for the snowmobiles. In addition to this go 8 barrels of diesel fuel for the Icelandic jeeps/i. e., oversized Toyota Cruisers with huge deflatable tires to increase traction in loose snow; this was the first time these were tested these as terrain-going vehicles in Antarctica.*
>
> *Quality also counts in producing heat and converting energy under extreme circumstances (jet A-1 fuel for -50 degrees C, Arctic diesel for down to -35 or -40, and environmental diesel fuel when the atmospheric temperature is above -20 degrees; all this has to be planned to fit changing conditions during different legs of the EPICA-effort).*

During the optimistic 1950s there were visions of setting up mining settlements inside the ice, lit by electricity coming from nuclear reactors. However such scenarios never got beyond the free fantasies of science fiction and popular mechanics magazines.

Under the circumstances science became a surrogate for colonisation. The Antarctic Treaty (AT) statutes stipulate that in order to join in and have a right of presence a country has to display substantial research in the region. This used to be interpreted as meaning that one had to establish and maintain a research station. In recent times however it has been enough to rent facilities (e.g., the Polish station Arctowsky, after the collapse of the Cold War when hard currency was needed) or conduct good quality marine research off ships in the coastal waters. The Swedish station established in the late 1980s was directly motivated by the desire to enter the Antarctic Club before 1991 when the Treaty was up for possible revision and – it was anticipated – the door might be shut, thereby excluding potential new members.

Science is thus the vehicle whereby nations manifest their presence and their right to participate in the management of Antarctic affairs at a supra-national level. The AT is a viable regime *outside* the UN-system.[1] Although it has been contested both by Third World countries and NGOs, the Treaty has hitherto stood the test of time and periodic turbulence especially in the wake of the oil crisis of the 1970s, when several Third World nations were taken on board. This helped break up the compact Western colonial configuration, while taking some of the sting out of the opposition. Within the framework of the AT science has a dual role; apart from importance in its own right in advancing knowledge, science has symbolic clout in a geopolitical arena, therewith serving a continuation of politics by other means (Elzinga 1993).

As already indicated, the accent during the past ten years has come to lie on environment and environmental protection. Since 1991 there is a special Environmental Protocol which is linked to the Antarctic Treaty. It placed a moratorium on all minerals exploitation for fifty years, and specific rules have been worked out to govern Environmental Impact Assessment procedures to which all activities – with science and tourism as most prominent – in Antarctica have to be subjected. Ultimate responsibility for living up to these rules remains with national authorities in the countries from which expeditions originate, and there is no supranational controlling body. Observance of the principles laid down is voluntary; peer pressures and a system of mutual inspection play an important role.

One of the key concepts is 'minor and transitory impact.' This draws a line of demarcation between projects that do not need *further ex ante* impact assessment (i.e., projects that are expected to give less than minor or transitory impacts), and those (anticipating more than minor or transitory impact) that do require more thorough or comprehensive evaluations before being approved. Here exists a lot of interpretative flexibility – what is minor, and what is transitory impact? Another question is, what about cumulative effects of human activities in Antarctica? This is another domain where conflict and tensions may emerge during the course of an expedition.

> *30 Dec continued: Finally, security of life and limb of all individuals plays in; therefore on top of it all we carry 8 barrels of helicopter fuel – which we hope we don't need to use – for possible emergency medical and rescue operations. Foresight, indeed, has many dimensions, all of which become amplified under Antarctic conditions.*
>
> *Our doctor, Krister Ekblad led the packing of food for people: roughly 600 portions of 1 kg food and drink per person per day for three weeks, plus an extra week thrown in 'just in case.' We have to plan for an average intake, he says, of 4,000–5,000 calories/person/day, which is about double the normal consumption even for a hard working person back home. His explanation is that we must make provisions to burn at least 2,000/person/day extra to beat the cold, which drains us of our heat.*
>
> *Some of what we are leaving in the glacier down the hill from the base station, stached away in a crevasse provided by mother Nature; the crevasse has now been recruited as an 'actant.'*
>
> *For the participant observer such conflicts and tensions can also be used to explore the dynamics of research in the field. Here four aspects stand out in the identification and assessment of environmental impacts, or risk calculation: (1) cultural variability due to researchers' affiliation with different scientific disciplines (variation over disciplines like geology, glaciology, biology, atmospheric physics, etc.); (2) task differentiation within, say, an expedition (functional differentiation: researcher, construction worker, mechanic, helicopter pilot, doctor, driver, logistics and environmental officers, etc.); (3) generational differences, with younger researchers on the average attributing relatively greater importance to environmental ambitions; (4) what country a researcher comes from, in as far as this may bring in differences in the entrenchment of environmental consciousness in the cultures of different countries (cultural variation between countries) (Elzinga 1999 and 1998). The last point*

opens up for new tensions, also at the policy level between countries that are party to the Antarctic Treaty.

2 Jan: Good news was that the faulty bearing in the ice coring drill was reparable. Tomas filed and polished it, and with Freyr's help got the expensive sophisticated machine back onto its legs. This was crucial since the success of the EPICA-traverse hinges largely on this piece of equipment which is Dutch-owned but shared for mutual scientific benefit in a European context. The second day of the year ended with a late (midnite chili con carne and pasta) dinner for 14 hungry persons … Since meat was solidly frozen it had to be hacked into small pieces. Here a large knife and a crow bar were useful instruments.

Historical: Seven Pie-Like Sectors

Seven countries historically claim the right to have a special position in that they at various times put forward and documented territorial claims. That is why some maps of Antarctica show a series of pie-like sectors, all but one of which meet at a common point at the geographic South Pole. (The Norwegian slice does not go all the way down to the Pole, since this would give precedence to a sectoral definition that other countries like the former USSR might have used to substantiate their claim to Spitsbergen in the northern hemisphere). The text of the AT neither recognises nor denies these national claims. Thus it provides a *modus vivendi* where the use of the continent foregrounds science and other peaceful activities.

The Norwegians for their part base their claim to DML on Antarctic exploration during their period of prowess as a whaling nation. In the 1920s when the price of oil was rising, Britain was fearful of the depletion of whales in her sector and refused to issue further permits in the Falklands Dependency regions where most whales were hunted 1904–1914. Norwegians developed new technology, which allowed them to process whale meat entirely offshore on ships at sea (so-called pelagic whaling).[2] Thus they neither needed to pay tax to the British, nor subject to controls. They also looked for and opened new hunting grounds. The southern ends of the Atlantic and Indian oceans, just off the Antarctic coastal ice, were found to be particularly rich regions. In the course of these activities the dominant Norwegian firm, led by Lars Christensen, also sponsored survey mapping of the coastline and inland (e.g., Hjalmar Riiser-Larsen 1929/30; Viggo Wideröe 1936/37; cf. Bogen 1957).

In all Christensen financed no less than nine Norwegian Antarctic Expeditions from 1927 to 1937; in his own report he points to three of these as yielding important scientific contributions (Christensen 1938). Using the newly available technique of airplanes and aerial photography a lot of territory was covered, revealing interesting new features. Christensen himself notes a feature that could fascinate anyone interested in the coming and going of ice ages: on Ingrid Christensen Land (named after his wife),

> A curious phenomenon in the mountains on this part of the coast was a great quantity of small fresh water lakes up in the mountains, quite open and without a sign of ice on the water or around the edges of them (Christensen 1938: 9).

Oceanographic, meteorological and biological studies were also promoted, mostly in the interest of the whaling industry, but also of interest to scientists more generally. The Norwegian Whaler's Insurance Association used the material to produce comprehensive maps of the coastline. As empirical benchmarks in historical time some of the results of these studies are still of value today, including for computer simulation modeling, when comparing changes in the annual extent of sea ice in connection climate research.

When it comes to the emphasis on climate research there are further historical events that deserve attention in the present account. It is interesting here to highlight an often forgotten historical line from the situation sixty and fifty years ago to the efforts we see today, at least in DML. Indeed, the question of climate change was already being addressed the first time the interior of Dronning Maud Land was mapped. This is something that has fallen into obscurity and is worth recalling today when the term 'global warming' is on so many lips.

From Neu-Schwabenland to Dronning Maud Land: Norway Beats Germany

On many Antarctic maps one will find the name Neu-Schwabenland. This is in honour of the catapult ship used by a German expedition to Antarctica 1938/39. To this day the names of the the expedition leader

Ritscher, the ship's captain Kottas and the two Lufthansa pilots, their flying boats, as well as a number of other older German names are still attached to some prominent features of the map of the larger territory called Dronning Maud Land.

Presently the German Neuymayer research station is also located on the coast in this area, while the former DDR station Georg Foster is now dismantled. During the past couple of decades German researchers have added a layer of new names to commemorate colleagues and modern sponsors, although East Germans came to reject the older names introduced by Ritscher's expedition, preferring the later Norwegian names. Naming in Antarctica can be a rather political business, even out in the field. For example, even today names attached to new sites are carefully chosen to signal honorific deference in scientific or political credibility cycles, or simply in the hope of enhancing future funding opportunities from research councils and enrolling private sponsors.

When German aviation ultimately returned to the area in the 1983/84 season with ski-equipped aircraft it was during the third post-IGY (International Geophysical Year) expedition. Even then the German pre-war expedition was a significant marker in geographical space and time.

The Schwabenland expedition in January/February 1939 carried out what at that time was to be the most systematic and extensive aerial photogrammetry yet undertaken anywhere. This expedition, led by Captain Alfred Ritscher, made use of a catapult ship Schwabenland which belonged to Deutsche Lufthansa. Previously it was stationed in the Azores as a landing and servicing platform for flying boats of a Dornier-Wal type on the German airmail route between Europe and South America.[3] Two of these planes, together with their veteran Lufthansa postal route pilots (who were used to landing in choppy waters) were taken along – the planes could be catapulted off the ship to reach flying speed in a matter of a few seconds, and after completing their mission they could be scooped up out of the sea with the help of a crane. This technology, never tried in the Antarctic before, was fairly successful, helped by the luck of good weather.

The German Antarctic Expedition 1938/39 was not only or primarily concerned with science. Its origins lay with Nazi-Germany's Big Power intent to participate in the continued division of Antarctica into spheres of interest and to secure German whaler's interests whose oil production was important for both industry and an expansive military machine on the brink of starting a second world war. Thus it was not only a matter of mapping the region, but also to lay the basis for claims to sovereignty, which was not at all unusual – several other countries, including the US, had been doing the same thing. In this particular area however it was only Norway that was the contender.

In order to substantiate possible sovereignty claims and annexation the German planes on their reconnaissance missions were also given the task of throwing out small javelins which stuck into the ice sheet below – this trick had been tested in the Alps. The metal arrows had small zwastikas in their tails, insignia to mark the nationality of the claim. These were supposed to be hurled down every 30 km along lines criss-crossing in a broad grid to mark out the territory.[4] There was no time to waste because it was well known that Norway also intended to lay claim to the same territory.

As it turned out the Norwegians, through their own intelligence sources in Berlin were already alerted to the secret German expedition that left Hamburg late 1938. Consequently their monarch proclaimed sovereignty over a still larger portion of this previously unclaimed territory. It is eight times the size of Norway itself, stretched on both sides of the zero meridian from 20° W to 45° E, and is named Dronning Maud Land in honour of the Norwegian queen at that time. The date was 14 January 1939, just five days before the Schwabenland reached the ice-edge of the Antarctic coast.

Glaciology from the Air: Oases in a Desert of Ice?

The scientific or technical content of the German effort using photo mapping as a basis to lay a claim to a large slice of Antarctica was more successful than the underlying political objective. In the span of fifteen days seven sorties were made over the Antarctic ice sheet and coastal mountain ranges to cover a large geometric grid. A vast number of aerial photographs were taken, useful for stereoscopic analysis and subsequent mapping, reaching into the interior as far as 800 km south of the coast (to ca 74° S lat.) (Ritscher 1942).[5]

Scientists who were aware of this work during or shortly after the Second World War were impressed. For example, the Swedish geologist J.G. Andersson who had been in Antarctica at the turn of the century wrote in 1945:

The cartographic result of these seven inland flights was simply unique. An until then completely unknown sector of the Antarctic coastline between 11.5° W lat. and 20° E lat., as well as the interior terrain to a breadth of 300 km had been mapped in a way that signified a great advance for knowledge of the South Pole continent (Andersson 1945: 416).[6]

The only trouble was that this airborne mapping was unconnected to ground surveys that could have provided astronomical positioning as triangular points, so-called 'ground truth.' Thus the maps hung in the air as it were. Estimates of elevation above sea-level of nunatak peaks had been grossly exaggerated, usually being out by 1,000 m., and the elevation (above sea level) ascribed to the polar plateau was 1,500 m. too high. Excepting a few features, later ground expeditions found it impossible to connect landmarks they saw to the photogrammetric maps.[7] Consequently a lot of the German names were later rejected by the Norwegians when they produced new maps over the region in the 1950s. It was not until 1986 that some understanding was gained of the German maps. This was in a Ph.D. thesis whose author used maps from the Maudheim expedition and recent satellite images to reset the aircraft position from which each of the German photographs was taken (Brunk 1986; cf. Swithenbank 1999: 233, note 4).

> *3–4 Jan: After a late awakening to recoup our energies, the wagon train took off at 1 PM, preceded by Lars and Mart on snowmobiles to reconnaissance the leads; they were followed by the two Icelandic jeeps. The SANAE people / from the South African station / indicated they would come by, so Pelle stayed behind waiting to be picked up by helicopter to guide our guests to the field – this would also give him a chance to check the tricky Kibergdalen pass from the air … The German Schwabenland aerial surveys in the late thirties apparently did not reach south of Kirwangweggen (which had been called Neumayer Steilwand). Heimefrontfjella became an important discovery for the Norwegians, who attached the names of their freedom fighters and leaders of the anti-nazi resistance and clandestine guerilla operations ('Heimefront') to mountain peaks, nunataks and valleys here. Geography and cartography can be very political. Now even the maps compiled by the Institut für Angewandte Geodäsie in Frankfurt / Main uses names like XU-fjella,*

KK-dalen (wartime code names of the resistance), Pionerflaket, or Mathiesenskaget just to our left in Sivorgsfjella.

… Tomorrow comes the struggle up to it, up the slope and past the crevasses (50–100 metres deep) which close in from both sides leaving only a small safe passage (ca 100 metres broad) between Sivorgsfjella and Tottanfjella. Getting stuck in a crevasse is not only dangerous, it would also probably delay us a whole day with unloading, salvaging, reloading and rerouting through a new lead.

The Ritscher-report nevertheless had repercussions that were to be significant for post-war climatologists.[8]

One who perused the Ritscher-report in detail was Hans W:son Ahlman, the Swedish glaciologist, professor at Stockholm University until 1950 and thereafter Swedish ambassador to Norway. Ahlman was particularly interested in the small snow and ice free areas he saw, like islands in an alpine type mountain landscape 300–400 km from the coast. Much of his life he had studied glacier retreat in the Arctic. The question now was whether these 'oases', as he called them, represented a similar trend in the southern hemisphere, in which case one had to do with a 'global' climate change, past or present. He wrote:

As far as I am aware, these conditions are the first more certain indications in interior Antarctica of a relatively late warm period, even if one does not know the chronology. However there is nothing that tells against the assumption that it constitutes something similar to the post-glacial warm period on other parts of the globe (Ahlman 1944: 651).

In a later paper on the subject he wrote:

As far as I know, there is no other area on the earth where one can find, as one does here / in the Schwabenland photographic data / local glaciers in the domain of inland ice, sensitive and reacting to the climatic factors that determine their destiny (Ahlman 1948: 247).

[And,] it is possible here to get an answer to the question whether or not the recent history of glaciers and climate, and therewith climate fluctuations, also include the Antarctic (Ahlman 1948: 249);

in other words, whether or not the 'fluctuations' are global.

NBSX and the Issue of Global Climate Change as Perceived 1948

The pictures taken by the Schwabenland expedition and its accompanying report inspired Ahlman to work towards organising an Antarctic expedition in hopes of gaining first hand knowledge of the area and to determine whether or not the glaciers there were waning. He was interested in making the ice talk at close range.

The ultimate outcome of his efforts was the Norwegian-British-Swedish Expedition of 1949–1951, also called the Maudheim expedition, and sometimes NBSX. He worked intensively and lobbied leading scientists and politicians for four years to get it launched. It was his last great project, predicated on the belief that there might be global characteristics of the climate warming problem (at the time it was referred to as *climate improvement*) (Kirwan et al. 1949: 11–13; Ahlman 1948). It was largely Norwegian financed but included fourteen men from five nations in the overwintering party. At the time it was recognized as "representing an entirely new venture in international scientific co-operation" (*Cambridge News* 14 Feb 1949; Illingworth 1949). Later it was referred to as a precursor and exemplar for the International Geophysical Year 1957/58 (Crary 1962, cited in Swithenbank 1999: 227), which in turn lay the groundwork for a viable vehicle for a supra-national regime, the AT, in which science makes up much of the glue (cf. above).

The speculation regarding snow and ice-free oases with lakes in a desert of ice was used to interest a wider public and in fund raising for the NBSX. This led to amplification in the daily press, where there was talk of a long lost civilisation in the Antarctic (*Stockholms Tidning* 6 Dec 1948). Some papers referred to an 'Antarctic Shangri La', and the 'oasis mystery'. A Minnesota newspaper is reported to have picked up a *Times* of London interview with a member of the organizing committee L.P. Kirwan and credited him with the discovery of Beduin encampments, date palms and camels at the South Pole.[9]

> *6 Jan: The interesting thing with the landscape under the ice in this region is that it tells us something about the origins of the ice many millions of years ago … What we see on the radar screen is a landscape that has lain there deep-frozen and undisturbed for more than 10 million years … When we turn back to where the wagon-train is we find that it has come to halt – one of the Hägglund*

track-vehicles has a gearbox ceased up … After much discussion two of party of ten were sent back to Wasa to fetch spare parts, 600 km altogether, there and back again by snowmobile … An advance party of five was sent on with the jeeps to the drill site at 76° S, 8° W to start getting up smaller cores of firn, so as not to lose too much time.

7 Jan: Work with replacing the gearbox in the axle done and the train got on its way again by 7 PM.

15 Jan: Different tasks proceeded largely according to plan. An attempt to obtain a 20 metre firn core however almost cost us the smaller drill when it got stuck at the infamous risk zone at 16 metres. Antifreeze sloshed in the hole and a strong crane on the track vehicle saved the day … The larger drill reached down to 111 metres, but the big event was passing the 100 m. mark on the 14th, which was celebrated with champagne.

The theory of a retreating polar ice cap due to global warming was picked up by many writers at the time, and seems to have influenced others with an Antarctic science ambition. Finn Ronne, for example, the leader of a privately sponsored expedition to Graham Land 1947–1948, in his book *Antarctic Conquest* (1949) states boldly:

The retreat of the ice that they / Darlington and Latady flying over George VI Sound 1947 /, and which I already observed in our local glacier, we found characteristic wherever we went. A similar shrinkage of glaciers and ice barriers has been noticed in the Arctic, which points to a gradual warming of the earth's climate. If this process ever gets to the point of melting the Greenland and Antarctic icecaps completely, the water thus released will so raise the sea level that all the world's seaports will have to move miles inland. However, since such changes would take hundreds of thousands of years, it's nothing for us to worry about (Ronne 1949: 67).

It should be added here that Graham Land is the British name for part of the Antarctic Peninsula facing South America. It is a region in the close vicinity of oceans and therefore more sensitive to recent climatic fluctuations than are regions in the continental interior. Retreating glaciers and collapse of parts of ice-shelves around the Peninsula are an issue of considerable concern today.

Meteorologists and oceanographers were also taking an interest in theories of climatic fluctuations (Wallén 1950). At a scientific meeting in Copenhagen in Oct 1948, the following resolution was taken:

Having considered a number of lectures on climatic fluctuations the International Council for the Exploration of the Sea recommends that these important and far-reaching problems ought to be more closely investigated and that these investigations might be adequately supported by Governments in different countries (Ahlman 1948: 241).

Not everyone accepted Ahlman's speculations. One's disciplinary affiliation might make a difference. The geologist J.G. Andersson, already cited, vented his scepticism early on regarding the 'oasis'-theory of a polar icecap thaw, or fossil lakes from a past climatic period. Having studied the same pictures from the Schwabenland expedition he wrote, in 1945, how the alpine lakes around certain nunataks like the dramatic Wolhthat-massif can also be interpreted in a different way.

... the situation tallies well with the notion developed by some southpole explorers, that in the vicinity of dark cliffs local snow melts and therewith follows the creation of melt water lakes, which quite naturally often appear to be ice-covered, perhaps with some holes where the water finds its way under the ice. Thus I do not think one needs to regard these lakes as fossil lakes as professor Ahlman has sought to interpret them (Andersson 1945: 436).

Some Results of the Maudheim Expedition 1949/52

Ahlman's original plan had been to have the Maudheim expedition land near the zero meridian to set up a station on the coast from which glaciologists and geologists could make their way inland to the dramatic areas photographed by the Germans, especially the Wolhthat-massif. Sea-ice conditions and currents however made this impossible, and the party ended up quite a bit westerly, at Kap Norvegia on a bay known to Norwegian whalers. This was early February 1950: The wintering party were quickly unloaded with all their gear; they were to spend two full years a ways up on the ice-shelf which was about 200 metres thick, floating on the water, and moving up and down with the tide like on a hinge.

The expedition members included meteorologists, glaciologists, a dog-handler and a cook, a medical officer, plus the expedition leader John Giaever.[10] Inland transportation over the ice sheet was by Weasels (war-surplus tracked vehicles), and two teams of dogs to pull sledges, the latter being the mode used for travel furthest into the interior. It was a challenging time, a multinational group doing research where none had set a foot before them. It was also adventurous, with crevasses, terrible winds and whiteouts. Three members in a group of four drowned when they got disoriented during a whiteout near the base-camp Maudheim and inadvertently drove one of the weasels over the ice-shelf into the cold sea; the fourth was rescued after spending 11 hours alone on an ice-floe. Another person got a stone splinter in his eye during geological field work and eventually had to have his eye removed by amateur surgery in an improvised operating theatre set up in the base-camp hut, instructed from afar over the wireless by a medical authority back in Sweden.

Carbon monoxide poisoning from the exhaust fumes of the engine on the Canadian Longyear 'Straitline' rock drill used for ice coring in a separate shanty, as well as poor ventilation in the main house almost got to some of the others. Still at the end of the two years the group were able to bring out some important results. It was the first time anyone had drilled and recovered a 100-metre ice core in Antarctica (it was from the ice-shelf by the wintering base, where most of the time was spent in core drilling and in examining the cores obtained). As the senior glaciologist of the expedition, Valter Schytt wrote: "Here was an opportunity to study for the first time the processes by which snow is altered to ice in a climatic region where melt water plays no part in the metamorphosis" (Schytt 1953). Snow density studies, together with the surveyor's leveling work and some other parameters were also used to calculate fairly accurately the depth of the ice in the ice shelf. The result agreed with the outcome of seismic soundings, which were used to verify it (the value found was 180–190 metres – cf. also Robin 1953).

A profile of the bedrock underneath the ice sheet was made on the basis of seismic soundings along a 600 km traverse from the coast into the interior, showing a maximum depth of the ice field in some places 2,600 metres; this was another first (Robin 1956). Finally, Valter Schytt lay to rest his mentor's (Ahlman's) notion of possible glacier retreat in Antarctica. Charles Swithenbank (1999) discusses what they saw in the field just after New Year 1951. This was after they had reached the nunatak they recognised from the German maps, named after one of the flying boats.

Passat was made of diorite sill material and it too had a big wind-scoop, but here the moat held water. There was one lake 20 by 150 metres and it appeared to be about half a metre deep … clinging to the bedrock surfaces … luxuriant mats of green, long-haired moss in sheltered spots. Here, we realised was an Antarctic oasis (Swithenbank 1999: 148).

The lichens clung to the rock right down to the snow-level. Schytt knew that even in warmer climates lichens might take many decades to migrate. If the ice surface were retreating then a band of lichen-free rock would be found an appreciable distance above the snow-level. Since this was not the case one could infer that there was no glacial retreat in the region.

16 Jan: Jon, Aant and Freyr continued to drill small firn cores and Lars Karlöf began analysing them. Lars is measuring the snow's electrical conductivity which varies with the snow's density and the origins of the airmass. The seasonal variation in these parameters can be used to distinguish different years and also the yearly precipitation. The results he is getting are very interesting. They can be interpreted in a way that makes 'our' site an excellent one for future deep core drilling since the precipitation is relatively high and therefore it gives the high time resolution needed to be able to make good comparative studies with the ice core results from the inland ice of Greenland.

The next day they went to explore Boreas, named after the other German flying boat. There it was the same story: lichens and moss in abundance.

On that morning, in other words, we established the fact that the general recession of the glaciers in the north had no direct counterpart in the inland ice of Dronning Maud Land … Not until much later could we also state that the inland ice itself was not increasing (Swithenbank 1999: 150).

The ice had told its story, and it was a falsification of a hypothesis that had already started to get entrenched in some circles and – through the media – in popular imagination.

Before the wintering party was taken home by the *Norsel* in February 1952, some Swedish planes the ship had brought along were used to do extensive reconnaissance flights into the interior. Thanks to clear weather they were able to photograph almost half the area previously covered by the German expedition 12 years earlier. This new aerial photo-survey, together with ground information and maps made by the wintering party during their two years of field-work laid the basis for a revision of maps of the region. The later Norwegian maps threw out a number of German names since the features of the landmarks to which they attached could not be identified. This was also the case for the range of nunataks where the Swedish station Wasa was established in 1989. The German name Kraul Berge (after the name of the Schwabenland's ice navigator, Otto Kraul) still appears on certain countries' maps (e. g., a French one), but on the Norwegian map it is called Vestfjella (Westerly Range), and the Swedes call the nunatak on which their station is placed, Basen in Vestfjella.

Coming to DML almost fifty years later in search of a site for EPICA, our expedition found the same rugged landscape the members of the Maudheim expedition had reported. Physical conditions and landmarks reminded us of their stories. On the other hand we came with an entirely different artificial life support system, which meant that some of our logistical and other problems also were different.

> *16 Jan ctd.: The radar programme has not gone as well so we have had to make some changes in three respects in hopes of solving our dilemma – one of these is a huge excavation shaft 2.5 metres deep near the drill site in order to study the annual snow layers in more detail.*
>
> *18 Jan: (Temperatures by this time were reaching -39° C at night). Packing and ready to get back … Temperature measurements in the drill holes appear to be more complicated than we had expected … Maybe they reflect irregularities in the snow's accumulation pattern upstream from the drilling site. This would disqualify the region for a future deep drilling operation and complicate interpretation of our own 136 metre long core.*

Concluding Remarks: Constraints and Negotiations Around Research in the Field

From the field notes one can see that some things have changed and others remain the same; 'the ice' is still a capricious and demanding partner in the act of research in Antarctica. One has to make constant improvisations to overcome difficulties put in the way by Nature and the wear and tear of an expedition.

Our major setback was the failure of the radar. This triggered a controversy over responsibility: Was it those in charge of logistics who should have seen to it that a power generator unit did not produce disruptive 'peaks' in the electrical current serving the radar unit, or lay the fault with the scientists for not testing the equipment sufficiently back home, or for failing to define more specific parameters for operating conditions? The problem was never resolved. It was compensated in part by falling back on more traditional methods of snow stratification analysis in the field.

We see here how research in Antarctica is not constrained by Nature alone. Certainly, if it is too warm during a given period, and ice core drilling fails because of this, or if instruments break down, one may have to reschedule/re-negotiate scientific work plans. This brings into play at least three sides of a *triangle of action and reaction* where research activities are constrained by what may be called the human dimension, played out in *logistics*, concern for protecting the *environment*, as well as *health and safety* (Elzinga 1998 and 1999). Within this framework for science in action, strong components of negotiation reduce degrees of freedom in research work.

Apart from logistics and human safety, in recent years more and more emphasis has also been placed on EIA and monitoring. Primary considerations in the triangle constraining the free play of the researcher in field-work thus pertain to personal safety, the material and technical conditions of logistical support systems, and today also the obligation to take into account the impact of what one does to the environment. Social studies of science here can contribute to deeper insights into the complex dynamics of the research process. This is particularly significant when it comes to environmental impact analysis.

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Notes

1 To date 44 nations have agreed to the AT, but only 27 are Consultative Parties (full members), i. e., the ones who actually control the decision-making process.
2 It has been estimated that from 1905 to 1937 Norway caught 430,935 whales in Antarctic waters (Burke 1994; 30, note 7 to Ch 7).
3 The airline advertised a 4-day Berlin-to-Rio airmail service; pictures of the catapult system can be found in Burke (1994: 150–156; also see Grieson 1964: 493–498).
4 On the map that was produced, spots with arrows are specially marked. American, British, South African and German scientists on later expeditions have used metal detectors to try and find the arrows, but without success. One story has it that the navigator or photographer in the plane simply threw out all the arrows in one spot to get it done with (Robert Headland, Scott Polar Research Institute, Cambridge – private communication).
5 Ritscher's entire report consists of two parts, a report of the expedition and its results, and a selection of illustrations with commentary in a separate folder; some of the pictures are colour tainted and paper spectacles with blue and rose tinted mica are included in the inside cover of the second report, providing the reader with a stereoscopic gaze. In all 11,600 photographs were taken, and the ones developed were 18x18 cm. For an early review see Andersson (1945: 403–420); Fagerholm (1944) also reprints some of the photographs. Fagerholm notes how aerial photogrammetry was spurred by military interests, and very advanced in

the reconnaisance by Italy prior to the bombing of Abbysinia in the mid-1930s.

6 Andersson was so impressed by the German effort that he gave a detailed account of the Schwabenland expedition, both in the first edition of his book (1945), and again in the second edition (*Sydpolens Hjältar* 1954), which is expanded to include a brief account of the Maudheim expedition. A Norwegian in the early 1940s recognizes the German contributions, but points out that Norwegian aerial photography had already mapped quite a bit of territory earlier, and that Norway occupied Dronning Maud Land since 1939 (Aagaard 1944).

7 Moreover they only had the published photographs at their disposal. Ahlman apparently met with Ritscher after the war, only to learn that the bulk of the photographic material had been lost, buried during the war in the ruins of the Deutsche Seewartes House (National Hydrological Office) in Hamburg (cf. Ahlman 1948: 216).

8 A second volume (Ritscher 1958) was published during the International Geophysical Year (IGY) 1957/58 with hydrographical, oceanographic and biological papers, and a substantial introduction by H.P. Kosack who used material from the BNSX expedition 1949/52 to correct several errors and reinterpret some of the German maps. Kosack later wrote a comprehensive encyclopaedic work (Kosack 1967). The Norwegian Bogen criticizes Kosack from a politico-ideological perspective (Bogen 1957).

9 Reported in *New York Herald Tribune* 21 April 1949; *Christian Science Monitor* in its earlier account 4 April 1949 notes that Kirwan also envisioned prospects of industry and possible extraction of mineral resources, including uranium deposits. A later report by Kirwan appears in *Times of London* 19 Nov 1949.

10 Giaever's expedition report is in Norwegian, in book form (Giaever 1952). It was translated immediately translated into Swedish (Giaever/Schytt 1952), and then English (Giaever 1954). Note that the Swedish edition carries the glaciologist Valter Schytt's name as co-author. Charles Swithenbank's recent book (1999) gives a detailed personal account of the NBSX. He was the youngest member of the expedition, going as Schytt's assistant. Swithenbank's book, published fifty years after the expedition's commencement, has a full list of the scientific publications, and it

is a mine of much other information concerning the state of the art research at the time. A broader historical perspective, all too brief though, may be found in Fogg (1992). It covers early history of glaciology (pp. 269–274), seismic survey (pp. 264–266), with a picture of the bottom profile made over Dronning Maud Land, and it discusses the ice coring to derive climate historic records (pp. 274–279).

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Metaphors

Moving Targets in the (Social) Sciences

Ever since scholarly discourse has concerned itself with metaphors the latter have been said to toy around with correct meanings and conventionalized usages. At best, one holds that they have no relation to true knowledge at all and lets them pass because of their merely decorous role. More often, however, suspecting the worst, one is afraid of their outright deceptive effects. Thus, it comes as no surprise that metaphors have been regarded with suspicion and can be found in the midst of various dualistically structured debates: To mention but a few, they have been seen as ornamental, yet inessential; educational, yet lacking genuine insight; as economical carriers of complex meaning, yet easily misleading. Their structure, their usage, and their function have been subject to ongoing criticism. Only Max Black's seminal paper on "Metaphors" in 1962 endowed the topic with new attraction for both philosophers (of language) and linguists. Although scholars such as Richards (1936) and Burke (1941) had liberated metaphors from being a deviant unit of speech and thought three decades before, it occurred only within a decidedly antipositivist climate that one took an unbiased stance toward metaphors and investigated their semantic and pragmatic particularities. From the sixties onward scholars increasingly were of the opinion that metaphors indeed served important discursive ends: While explanations and evaluations still vary enormously, ever few scholars doubt the considerable, if not constitutive power of metaphors.

Today, the scholarly discourse scrutinizes an impressive amount of structural and functional issues (cf. the bibliographies by Noppen 1985, Noppen and Holst 1990), engaging a huge array of disciplines, instigating a bewildering amount of new questions and theories. This multidimensional discourse on metaphors does not stop short of science studies. In particular, metaphors have become interesting for studies in the area of ideological critique; see, for example, Harrington's study of holistic science in the Third Reich (Harrington 1995) or Lakoff's study of conceptual metaphors guiding political attitudes (Lakoff 1995). The guiding notion is that metaphors, being powerful conceptual tools of producing knowledge and world-views, shape (social) scientific notions as well. This is exactly why most authors are still suspicious of metaphors: Ideological effects of metaphors and cognitive operations based on them just cannot be fully controlled. Only a few authors, however, regard metaphors as ordinary, yet unfamiliar terms or phrases to which various discourses connect, and, by using them, shape and reshape meanings of both the metaphors and themselves. Therefore, a metaphor-view on knowledge dynamics may highlight the fate of invidual terms and phrases as they meander through heteregenous discourses, producing locally specific meanings, yet at times converging on overarching issues, paradigms, or 'cultural matrices' (see Maasen / Weingart 2000). Hence, on this view, they are tools for understanding interdisplinary communication as well as communication between different parts of society, such as science, politics, media, economy. Jim Bono will emphasize their performative function: Metaphors are instruments of thought and action.

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WHY METAPHOR? TOWARD A METAPHORICS OF SCIENTIFIC PRACTICE

JAMES J. BONO

Why metaphor? Despite vigorous attempts in recent years to recuperate metaphor as a subject of or tool for science studies, the question why one should pay serious attention to metaphor remains a live issue for many students of science as cultural and social practice. Reasons for this resistance to metaphor are undoubtedly complex and numerous. Among them, two especially stand out for us. First, metaphor as a linguistic category seems to pose problems for revisionist historians and sociologists of science. Wishing to move beyond traditional positivistic accounts of science, many Anglo-American historians and sociologists of the 1970s, 1980s, and 1990s – invoking Kuhn, Feyerabend, and Rorty – turned away from preoccupation with scientific theories as formal propositions, rejecting language-centered models of science in favor of local analyses of the production of scientific objects and knowledge. Often they rejected as well views of science as a 'mirror of nature,' of language as mere representation, and hence of words as 'mimetic' and 'corresponding' to things. Ignoring the degree to which serious attention to metaphor engages the crisis of representation and complicates such theories of correspondence, some revisionist practitioners simply brand metaphor as too tainted by the 'literary' and 'abstract' to touch what really matters about science as a social and cultural activity. In this view, metaphor is dismissed from serious deployment as an analytic tool by virtue of its presumed associations with a rejected past and outmoded methodology (for background on science studies since Kuhn, cf. Golinski 1998; Hess 1997; Nersessian 1998).

Second, the turn toward 'practice' in science studies has often meant the exclusion of the metaphoric and most linguistic dimensions of science from serious consideration as part of the dynamics of knowledge production and change in science (on the turn toward 'practice,' cf. Golinski 1990, 1998; Lenoir 1988; Pickering 1992; Rouse 1996). Despite Foucault's insistence on language and discourse as practices, and on the materiality of such practices[1], much of Anglo-American science studies resists any significant role for the metaphorical in science. Rather, metaphor may, at most, find a role as part of strategically motivated 'literary technologies' aimed at the dissemination of socially produced scientific knowledge and recruitment of networks of loyal disciples.[2]

What follows is an attempt to outline an emerging metaphorics of science, one that, I would suggest, confronts the sources of resistance to metaphor noted above and finds them wanting. Contributing to this new insistence on the value of metaphor to the social and cultural analysis of science, is the work of Peter Weingart and Sabine Maasen. Together, they have much to teach their Anglo-American counterparts about overcoming 'fear' of metaphor (cf. Maasen 1995). One response to the resistance to metaphor is disarmingly simple: metaphor is everywhere, and cannot be dissociated from the activities constituting science. As Weingart puts it:

> Debates over the permissability of the use of metaphors in science are futile, since the flow of concepts from everyday language to scientific language, or generally between different contexts is inevitable. The problem is primarily which functions and dysfunctions certain metaphors have in a concrete case (Weingart 1995: 128).

Much as postpositivist history and sociology of science would want to draw attention away from science as a linguistic activity, Weingart's perspective suggests an abundance of empirical example for other, routine, and indeed 'everyday' uses of language in science that do not threaten to resurrect the positivist ghost of the disembodied scientist and a purely abstract, intellectual science. Quite to the contrary, Weingart and Maasen find in metaphors an analytical tool for a robust sociological account of science as a situated social activity. Central to their analysis is the function of metaphors as "messengers of meaning" (Maasen/Weingart 1995), which they have amply and effectively shown to account for the dissemination of key concepts, and the reorientation of research programs, across disciplinary and discursive boundaries (Weingart/Maasen 1997).

In certain respects, Weingart and Maasen's insistence on metaphor as an essential part of science studies' analytic toolkit complements other recent academic developments. Nowhere has the sea-change affecting metaphor been more dramatic than in the field of cognitive science, especially cognitive linguistics. Moving beyond philosophically freighted, abstract accounts of knowledge and knowing, cognitive studies have unearthed a rich and homely lode of empirical examples drawn from everyday life suggesting the ubiquity of analogy, schemas, models, and, most centrally, metaphors to basic cognitive processes such as categorization, pattern recognition, invention, 'mental leaps,' and various aspects of reasoning. For cognitive sciences, metaphor and language are not abstract symbolic systems laid over experientially derived knowledge like a thin decorative veneer. Quite to the contrary, language – and especially metaphor – are themselves rooted in experience and in turn provide fundamental schemas – basic metaphorical structures – for organizing, comprehending, and navigating our experience and then translating it into the cognitive rudiments of knowledge and action.

Most visible, in this regard, is the work of George Lakoff and Mark Johnson. Indeed, the very first paragraph of their first book, *Metaphors We Live By*, suggests the changes that have swept over cognitive approaches to metaphor in the last twenty years:

> Metaphor is for most people a device of the poetic imagination and the rhetorical flourish – a matter of extraordinary rather than ordinary language. Moreover, metaphor is typically viewed as characteristic of language alone, a matter of words rather than thought or action. For this reason, most people think they can get along perfectly well without metaphor. We have found, on the contrary, that metaphor is pervasive in everyday life, not just in language but in thought and action. Our ordinary conceptual system, in terms of which we both think and act, is fundamentally metaphorical in nature (Lakoff/Johnson 1980: 3).

More than a linguistic trope, metaphor turns out to be a fundamental cognitive operation and, as such, central to thinking and to acting in the world. Indeed, we make contact with our world, we engage – and hence 'experience' – it through the very metaphoric operations that inform our 'conceptual system.' This movement away from a view of metaphor as 'literary' and as sharply distinguished from, while subordinate to, the 'literal' has opened up vast domains of both ordinary and specialized practices to careful scrutiny as cognitive systems through examination of their metaphoric structures and operations.[3]

Citing a wealth of empirical detail, Lakoff and Johnson and other advocates of a cognitive approach to metaphor – for example, Mark Turner and Gilles Fauconnier (cf. Turner 1987, 1991; Lakoff/Turner

1989; Fauconnier 1997; Turner/Fauconnier 1999) – have sought no less than a systematic reformulation of our understanding of cognition itself. For Lakoff and Johnson, this has meant pushing a very specific argument about the sources of metaphor and of metaphorically based cognitive processes. While using as evidence the empirical data of linguistics and analysis of metaphorical expressions, Lakoff and Johnson argue that the latter are but linguistic instances of larger, overarching conceptual structures that they call 'conceptual metaphors.' The conceptual metaphor, 'Argument is War,' for example, structures the way in which we think about a whole range of experience. It defines, in other words, a fundamental conceptual structure that gives rise to particular linguistic forms within everyday speech as captured in actual metaphoric expressions such as claims about 'winning an argument' or 'successfully defending one's position.' Metaphors, in this view, are common everyday expressions that depend upon, and reveal, the existence of 'deeper' conceptual structures that are part of our basic cognitive apparatus, and which are themselves metaphorical in nature. Similarly, Lakoff and Johnson frequently point to the conceptual metaphor, 'Love is a Journey,' as structuring a whole range of experiences and discourse concerning human relationships: "I don't think this relationship is *going anywhere*"; "This relationship is a *dead-end street*" (Lakoff/Johnson 1980: 44–45). Conceptual metaphors, then, are fundamental constituents of our underlying 'conceptual system' providing us with categories and schemas to organize our world.[4] Conceptual metaphors are meaning-generating products of our cognitive apparatus that, in turn, produce and authorize a vast array of detailed metaphorical expressions that link together the tissue of our experience.

Where do such conceptual metaphors come from? According to Lakoff and Johnson, the more fundamental and basic of our conceptual metaphors have their roots in our experience of the physical world. This is especially clear in the case of what they term 'orientational' and 'ontological' metaphors, which, they argue, provide us with powerful conceptual tools for organizing "a whole system of concepts" (Lakoff/Johnson 1980: 14) and for distinguishing the relationships (such as spatial boundaries, or actions/agencies) that define and distinguish one thing from another with which it nonetheless stands in some relation. Like orientational metaphors ("Happy is up; sad is down," Lakoff/Johnson 1980: 15), ontological metaphors depend upon the primacy of bodily experience of the physical world. Three of the most powerful such metaphors, for Lakoff and Johnson, are to be found in the schemas of 'containment,' 'force,' and 'balance.' Each of these metaphors represent fundamental conceptual structures that pervade virtually the entirety of our experience, structure our rational, or logical, thinking about the world, and ultimately derive from our *embodied* experience of the world. Even though Lakoff and Johnson open the door to the role of cultural experience in shaping our metaphorical conceptual system, the clear priority placed on the body, and embodiment, as source of basic cognitive operations, especially metaphoric processes, is indisputable in their work. Theirs is a *Philosophy in the Flesh* (Lakoff/Johnson 1999)!

Redefining metaphor as an important, if not fundamental, cognitive process, rather than simply as a rhetorical category and linguistic phenomenon, and insisting upon an embodied dimension to metaphor, are two crucial moves that I wish to affirm as genuine contributions to the kinds of account of metaphor that are needed, I believe, in science studies. Moreover, both these dimensions of the new cognitive model of metaphor can be adapted to complement and support a performative model of scientific metaphors that insists that the metaphorics of science operates on the level of both scientific discourse *and* practice. Valuable as cognitive linguistics has proven, certain missteps and suspect assumptions – at least, in Lakoff and Johnson's views – must not go unremarked. Two difficulties are especially worth noting: the universalizing tendencies of Lakoff and Johnson's account of metaphor; and the peculiarly abstract, foundationalist, and unsituated view of embodiment they embrace.

Universalizing tendencies may be the shibboleth of much theorizing in the humanities, and rightly suspect in analyses of social and historical phenomena, but nonetheless retain a certain legitimacy in the scientific study of natural phenomena. Few would deny the power and utility of well-supported scientific explanations that, in fact, do extend their reach to all phenomena of a given kind. Hence, in questioning the universalizing tendencies of Lakoff and Johnson's account of metaphor, it is not my purpose simply to give voice to a fashionable mantra. Seen as a contribution to cognitive *sciences*, Lakoff and Johnson inevitably seek those aspects of language and cognitive processes that indeed are as close to being 'universal' phenomena in human cognition and communication as possible. My criticism of their work should not be construed as a rejection of this aspiration. On the contrary, the question of what linguistic and cognitive processes and phenomena may rightly be considered 'universal' is well worth asking, and the answers inherently significant.

The ubiquity of metaphor and its central role in higher order cognitive processes may well constitute such empirically grounded universal phenomena. Beyond such very general conclusions, Lakoff and Johnson's tendency to impute universalistic status to specific metaphorically configured cognitive schemas, and then to regard such schemas as the basis for producing their cognitive-linguistic maps of all kinds of social and cultural interactions and artifacts, is suspect. Indeed, Nickles suggests that "schemas" do their work "in a local, instance-by-instance manner – something closer to a Baconian, pragmatic-experimental demonstration than to projection in the form of a universally valid law or rule" (Nickles 1998: 80). Take the metaphorical schemas of containment, force, and balance that are so fundamental to their argument and putatively universal. There is no question that these schemas are widespread and the basis for further metaphoric extensions. But are they universal in any meaningful sense? For Lakoff and Johnson the implications seems to be that they are. Containment, force, and balance are "image schemas" and, as such, "are relatively simple structures that constantly recur in our everyday bodily experience" (Lakoff 1987: 267). Further, "these structures are directly meaningful, first, because they are directly and repeatedly experienced because of the nature of the body and its mode of functioning in our environment" (Lakoff 1987: 268). As if this were not enough to make the point, Lakoff goes on to suggest that "since image schemas are common to all human beings, as are the principles that determine basic-level concepts, total relativism is ruled out, though limited relativism is permitted" (Lakoff 1987: 268).

Lakoff's 'limited relativism' undoubtedly opens the door wide enough to permit a role for 'cultural influences and differences.' If so, it nonetheless leaves intact the universalism and foundational nature of schemas like containment, force, and balance as such. The persistent claim is that such schemas are shared by all human beings and subject only to minor subsequent variations or, put differently, different subsequent metaphorical extensions. Sufficient evidence exists, I would claim, to suggest that what is 'universal' about such schemas is of such a general nature as to be devoid of any meaningful content. Is there, for instance, anything in common between, let us say, Western and ancient Chinese 'containment' schemas other than the *unspecified* duality, 'in-out'? Once we examine in any detail the specificity of this in-out duality, we find precious little in common, precisely because 'containment' as a schema implies different sets of relationships in the conceptual and experiential worlds of the two cultures. This is not to say that there are not 'insides' and 'outsides' in both cultures, nor that they are not both linguistically marked as such; it is to claim, however, that the boundaries between what is inside and what is outside are differently drawn and, at its most extreme, that the very notion of a 'boundary' itself is differently constituted in the two cultures. These differences are fundamental, for example, to acupuncture, to the very idea of 'organs' and their anatomical and physiological relationships to one another, and to the understanding of the body in relation to its 'external' environment in classical Chinese as opposed to both ancient and modern Western medicine. In all of these instances, attempts to understand Chinese medical schemas in terms of Western containment schemas in which there is an expectation of sharp boundaries between 'things' and in which agency is solely granted to such sharply distinguished entities leads to misunderstanding and outright confusion. Such energetic principles as *ch'i* and *yin* and *yang* operate exclusively in a world in which cosmos and body, concrete organs and their 'surrounding' environments, do not stand in any simple sense in relations of exclusive interiority versus exteriority to one another. Similar arguments and distinctions can easily be made regarding the schemas of 'force' and 'balance,' again using classical Chinese culture, and such examples as medicine, Buddhism, and definitions of 'personhood' as a foil for Western schemas of force and balance.[5]

As the above discussion suggests, Lakoff and Johnson's questionable assertions of universalistic claims for their metaphoric schemas direct attention to their assertions about embodiment and the bodily basis of metaphors and schemas. For these authors, cognition is a relentlessly 'bottom-up' process, with the fundamental schemas, metaphors, and categories driving cognition arising from our condition as embodied creatures. Again, let me be clear in stating that my objection is not to the notion of cognition as an embodied process, nor to attempts to understand 'mind' as necessarily embodied. On the contrary, I would insist that any account of the embodied nature of cognition must pay exquisite attention to the variegated ways in which embodiment is produced, achieved, and experienced. This, it seems to me, is precisely the step that Lakoff and Johnson elide. Instead of attending to concrete, situated, forms of embodiment and embodied cognition, they offer us a generalized, abstract, homogeneous body: one that gives rise to 'image schemas' and foundational metaphors that are universal. One might go so far as to say that, in avoiding the Cartesian conundrum of mind-body dualism, Lakoff and Johnson reenact an intransigent nature-culture dichotomy. They do this, it seems to me, by insisting on the origins of schemas and metaphors in the body's *physical* experience of itself and its environment.

Instead of giving priority to some pristine physical experience and consequently drawing the lines of 'influence' or 'causality' from the body / physical *to* the metaphoric / cultural, we should look instead to the body and embodiment as itself a hybrid, mixed 'thing,' as a site where the natural and the cultural are produced, or, better yet, as the place where the 'physical' and the 'discursive' become inseparably entwined in complex feedback loops. Put more concretely, how we come as bodies to experience the physical constraints of our world has much to do with how we, as embodied cognitive subjects, are situated in our world. Rather than a single, universal form of embodiment in the world attributable to a single, universal physical body that, for all practical purposes, does not vary from individual to individual, or from culture to culture, gender to gender (and so forth), embodiment takes multiple forms. While all bodies share certain physical characteristics, embodiment – and embodied experience of self and the world – varies in all kinds of ways. Some differences in embodied experience arise from patent differences among physical bodies themselves; bodies, for example, that have been marked as 'abnormal' by the standards of a medicalized bureaucracy. Others, while marked as 'normal,' nonetheless experience embodiment in multiple and varying ways due to subtle physical variations attributable to a range of factors from anatomical and developmental variations, to hormonal and biochemical variation, to variations produced by disease and immunological factors, to – in the world of postmodern medicine – the emergence of prosthetically transformed bodies. Such differences, and their relations to lived, embodied experience of self and the world, have only begun to be articulated in the burgeoning literatures of patient autobiographies, 'pathographies,' medical humanities, disability studies (including literature and disability studies), intersex and transsexual studies, and a range of related literatures and scholarly studies (Davis 1997; Dreger 1998; Hawkins 1993; Hunter 1991; Lykke / Braidotti 1996; Mitchell / Snyder 1997; Price / Shildrick 1999; Price 1995; Thompson 1996; among his many patients and cases cf. that of Virgil in Sacks 1995: 108–152).

Moreover, such variations in physical bodies are enormously difficult to separate from other factors – whether we label them 'cultural,' 'discursive,' or otherwise – that contribute to the way in which individuals regard their physical bodies and hence experience themselves as embodied. Indeed, it is precisely because of this difficulty that it makes little sense to speak of a dichotomy between nature and culture or the physical and the discursive / metaphorical. We experience our bodies – we experience ourselves *as* embodied – as simultaneously physical and redolent with meaning. This is why changes in our physical bodies have such unpredictable consequences: whether the changes are major or minor, abrupt or gradual, traumatic or 'natural,' the effects of such changes vary tremendously from person to person, and, among other factors, may be affected by cultural and gender differences. What seems to matter is the meaning such changes come to have for individuals and for the groups to which they belong: how physical changes come to 'fit' into the stories we inherit and subsequent (re-)tell of ourselves. While embodiment depends upon the existence of a living, physical body, embodiment itself is the product of the particularities and specificities of inhabiting a body in a certain way. Put differently, embodiment is neither 'physical' nor 'discursive,' neither 'natural' nor 'cultural,' but rather the primary (if learned) and concrete way in which we *relate*. Indeed, we are, in a sense, *not* embodied *until* we relate – to others and, through the 'other,' to self and the world. Embodiment attaches us to our world, to our 'self, ' and to 'others.' How we inhabit our bodies – whether 'able' or 'disabled', however 'enhanced,' 'altered,' 'prostheticized,' 'gendered,' or otherwise marked – produces an embodied, cognitive self that orients itself within, relates to, and operates in the world in specific ways. Just as such an embodied, cognitive self shares a physical world with others, it also shares a cultural and discursive world. Yet, precisely because of the particularities and specificities of its embodied relationship to self and world, its experience of the physical world is both shared and multiple and heterogeneous with respect to others.[6] Similarly, it both shares schemas and metaphoric structures with others, and also inhabits the world differently, investing its schemas and metaphors with particular variations of meaning, leading to subtle variations in relating to self, other, and the world.

Recognizing the complexities and rich variations of embodiment leads us away from an account of metaphor that stresses its universal features and foundations. Instead, it leads us to acknowledge that schemas, metaphors, and metaphoric systems of meaning are themselves subject to and situated in the particularities and specificities of history, culture, discourse, and all sorts of webs of relations. Returning to Lakoff and Johnson's schemas of containment, force, and balance and their cognitive analysis of metaphor, one is struck by their desire to find a single Archimedean point in a unmarked and abstract physical body as not so much empirical as, following Richard Coyne, metaphysical (Coyne 1995: 249–301; for discussion of Lakoff and Johnson 1987: 264–276). In particular Coyne invokes a Heideggerean perspective to critique Lakoff and Johnson:

For Heidegger, the spatial 'in' of containment is subservient to a primordial notion of 'in' as involvement. There is the nonspatial 'in' of being-*in*-the-world, being *in* a good mood, being *in* love. Seen in this light, Lakoff and Johnson's notion of containment is subservient to the more primordial notion of involvement. Prior to our bodily experience of containment is our being-in-the-world, an altogether more primary and important concept. Similarly, Heidegger offers a revision of notions of causality, which for Lakoff and Johnson is related to the bodily experience of force. For Heidegger, causality is subservient to care. From our being-in-the-world, we direct our attention within a region of concern. Notions that we may cause something to happen and that we may exercise control over a situation are derivative of this more-basic understanding of our place as exhibiting care … These arguments are obviously counter to those proffered by Lakoff and Johnson. Heidegger argues that there is a more basic experience than embodiment … Whereas Heidegger's identification of preembodied experience could be construed as yet another instance of discovering a foundation (not in the body, as for Lakoff and Johnson, but experience *prior* to the body), the preembodied has the appearance at every turn of being undecided. It is a fluxional involvement that defies pinning down. Heidegger's primordial concepts are not foundations but excursions into Pre-Socratic concepts of contradiction, flux, and play. How else could we characterize being-in-the-world? (Coyne 1995: 274–275).

Rather than grounding metaphoricity in such a metaphysical conception of a singular and stable form of physical embodiment, the position I have suggested is precisely to regard metaphor as a contingent, historical 'tool' which we use (and which 'uses' us) to approach, ultimately to inhabit, the unstable flux of things from which our world must emerge (Bono 1999).[7] Through metaphor we 'turn' toward the world and establish complex webs of relations with it. Take, for example, the metaphors of 'balance' and of 'warfare' that have characterized different epochs of medical thought in the West. The Hippocratic and Galenic ideals of health as a balance of humors, or active bodily fluids, authorized a particular set of relationships between individual bodies, and their external environment, and led to the cultivation of certain regimes of bodily care and control. By contrast, the 'embattled' body of modern germ theory adopts a quite different set of relations to its hostile external environment and enforces on itself – and on society more generally – a stringent medicalized, socio-political regime. Through metaphors we thus define ourselves as embodied cognitive selves in relation to what 'involves' us, or not (containment schemas), or what 'concerns' or moves us, or not (force schemas).

Schemas, metaphors, and metaphoric systems of meaning are not stable and universal, but respond to contingencies of history and environment. Whether arising from the body and embodied experience, or from other domains, metaphors do and must vary, as Sabine Maasen has insisted, "culturally, historically, situationally, individually" (Maasen 2000). Like Maasen, I reject the "analytical priority" Lakoff and Johnson give "to bodily experience" (as they define it) and insist upon the important tasks "performed" by metaphors at the discursive level (Maasen 2000: 210). At the level of discourses, metaphors serve as "messengers of meaning" as Weingart and Maasen have argued, and also as "mediums of exchange" among different disciplinary discourses, among different disciplines and cultural domains, and within different discursive ecologies (Bono 1990; Rosenberg 1999). Yet, if metaphor is to provide a significant analytical tool for the new science studies, I believe that we must also insist upon an embodied dimension to metaphor.

The relation between metaphor and embodiment is, I think, crucial to overcoming the resistance often expressed by proponents of science as practice to metaphor and literary dimensions of science. As I suggested at the beginning of this essay, one source of resistance to metaphor has been the persistent tendency to oppose practice to discourse. The rejoinder, of course, is to claim, following Foucault and others, that discourses are practices and, indeed, to insist upon the materiality of discourses and practices.[8] Within science studies, the drift has been strongly toward understanding scientific practices as intimately engaged with the materialities of experimental protocols, of instruments and the machinic dimension, and of natural and scientific objects.[9] Despite this convergence of science studies, and the discursive analysis of science, upon 'practice' and materiality, a gap remains. This gap can perhaps be characterized as one forcibly separating 'textual' practices and laboratory or instrumental practices. My claim is that an understanding of metaphor as embodied and performative can help us bridge this gap, indeed, can help us reimagine the gap as a kind of optical illusion. In so doing, the textual comes to be recuperated, not as a site of mere transcription – an archive for dead knowledge and information, but as a site of action and invention.

Here I must gesture toward a much longer argument. That argument begins by tracing the shift from a synchronic analysis of practices in the science studies literature to a fertile notion of 'practice' as *temporally emergent* in the recent work of Andy Pickering (Pickering 1995). Pickering wishes to see science as concerned with encountering (and then acting with and upon) agencies in nature. "The world is filled," for Pickering, "with *agency*": it "is continually *doing things*" (Pickering 1995: 6). He goes so far as to contrast science as practice – seeking to uncover the "dance of agency" (Pickering 1995: 22) – with traditional conceptions of scientists as "disembodied intellects" (Pickering 1995: 6) seeking mirror-like representations of things. Pickering's move doesn't just reproduce the concerns with material practices, objects, and instruments found in the turn toward practice in science studies. By emphasizing the temporal emergence of agencies *and* practice through the scientist's 'accommodations' of machines, instruments, experimental protocols, and models to the resistances of material agencies, he underscores the central importance of the scientist as a situated, embodied actor and, with it, the embodied and temporally emergent nature of scientific practice and knowledge. Interestingly, though Pickering exhibits an appreciation for the role of "models" in this emergent process, he explicitly demurs from granting any role to metaphors, precisely because, in the accepted view, metaphors are simply 'textual' (Pickering 1995: 19).

But metaphor, as Lakoff and Johnson insist, is a cognitive operation. More to the point, within the new cognitive regime metaphorical processes are basic to cognition itself, and therefore to extending human thought and action to new – or, as we might recast it, emergent – terrain. The cognitive model of metaphor, despite the shortcomings noted earlier, provides valuable empirical support for a shift that I have insisted upon elsewhere: from metaphor as representational to metaphor as *performative*. The work of metaphor, I argue, is not so much to represent features of the world, as to invite us to *act upon* the world *as if* it were configured in a specific way *like* that of some already known entity or process. It is this capacity of metaphor to, for example, make us act upon Nature as if it were a Book (as in early modern natural philosophy and natural history), or to act upon biological organisms as though they were the product of complex informational codes, that makes it so central to science and scientific practice. Without the metaphoric construction of heredity – especially DNA – as an informatic code, the mobilization of molecular biology and affiliated disciplines in the late twentieth century to produce an entire array of instruments, recording devices, and protocols to 'read' the molecular alphabet in which the book of life is written could not be imagined.[10]

With this notion of metaphor in mind, we can reimagine Pickering's temporally emergent 'practice' as a process whereby the 'models' embedded in the material practices, machines, and instruments of science and projected onto the material objects and agencies in nature are themselves instantiations of metaphors. They are, in effect, metaphors put into – or translated into – material actions and things. Put differently, we can say that the materialities of scientific practice – machines, instruments, experimental designs and protocols, and objects – are discursively configured and deployed through the metaphors embedded in and operating through them. A good example of scientific instruments and protocols embedding metaphoric models is the now ubiquitous Flourescent Activated Cell Sorter (FACS), which embeds in its design the informational metaphorics of molecular biology, thus tending to favor the skill-set and interpretive modalities of the molecular over the morphological approach to immunology.[11]

In effect, what I am suggesting here is a way to think about the limits, indeed the liabilities, of the discourse vs. practice, or the text vs.

action, dichotomies.[12] As Elizabeth Grosz powerfully suggests, "A text is not the repository of knowledges or truths, but also passage or point of transition from one (social) stratum or space to another. A text is not the repository of knowledges or truths, the site for the storage of information (and thus in danger of imminent obsolescence from the 'revolution' in storage and retrieval that information technology has provided as its provocation to the late twentieth century) so much as a process of scattering thought, scrambling terms, concepts, and practices, forging linkages, becoming a form of action. A text is not simply a tool or instrument; this makes it too utilitarian, too amenable to intention, too much designed for a subject. Rather, it is explosive, dangerous, labile, with unpredictable consequences … Texts, like concepts, do things, make things, perform actions, create connections, bring about new alignments. They are events – situated in social, institutional, and conceptual space" (Grosz 1995: 125–126). The world as we know it and operate upon it is one in which we continually conjoin discourse and practice, text and action: where we simultaneously learn *and* act by embodying intentions and projecting our metaphorically constructed models onto matter which we shape and use to our ends as *instruments* of thought and action. The world as we know it and study it is filled with material-textual, or material-discursive, hybrids – instruments; machines; illustrations; diagrams; maps; charts; physical models; computer simulations – that are simultaneously part of the material world and *instruments* for our knowing and manipulating it.[13] They are all, in their own way, what I like to call *material metaphors*: embodied metaphors-in-action![14]

Notes

1 For example, "Discursive practices are not purely and simply ways of producing discourse. They are embodied in technical processes, in institutions, in patterns for general behavior, in forms for transmission and diffusion, and in pedagogical forms which, at once, impose and maintain them" (Foucault 1977: 200).

2 On the notion of literary technologies in science studies, cf. the seminal work by Shapin / Schaffer 1985.

3 For an indication of the expansive reach of metaphorical analysis and of metaphor theory generated by the cognitive paradigm, cf. the recent Special Issue by Fludernik / Freeman / Freeman 1999.

For an example of the application of cognitive analysis of metaphor to a specialized field, cf. van Rijn-van Tongeren 1997.

4 The notion of schemas has received much attention in psychology and cognitive sciences. More recently, it has been applied to the sciences by philosophers inspired by the cognitive revolution. Thomas Nickles, e.g., regards schemas as "cognitive mechanisms" that can help us understand how "a complex situation or set of inputs" can be structured "into an organized whole" (Nickles 1998: 78–79). For Nickles, quoting Ulric Neisser (Neisser 1976: 22), "schemata are anticipations" in which specific exemplars or frameworks are transferred or projected onto new situations, thus illustrating how "the past affects the future" (Nickles 1998: 80, quoting Neisser 1996: 22). I would suggest that the cognitive mechanism of metaphor is closely related to the generation of such schemas.

5 For a rich source of examples, and for careful analyses of contrasting Western and Chinese schemas, cf. the brilliant book by Kuriyama 1999.

6 On issues of nature vs. culture, the body, and cognition, cf. Grosz 1994; Kirby 1997; Wilson 1998.

7 I argue for this perspective as well in an unpublished paper, Bono 1997; in expanded form the latter constitutes a chapter of Bono, in progress.

8 On the relations between discourse and practice, cf. also Certeau 1984; Bono 1995.

9 For the turn to practice, cf. Lenoir 1988; Golinski 1990; Pickering 1992; Rouse 1996.

10 Cf. the essential new book by Kay 2000.

11 Thus, a very detailed example of the metaphoric configuration of the materialities of scientific practice can be read into the very careful study by Cambrosio / Keating 2000. I plan to provide such a reading in my book, *Figuring Science*.

12 For views of texts as action, cf. the fundamental work of Paul Ricœur 1991; for example, "the models *of* actions elaborated by narrative fiction are models *for* redescribing the practical field in accordance with the narrative typology resulting from the work of the productive imagination. Because it is a world, the world of the text necessarily collides with the real world in order to 'remake' it, either by confirming it or by denying it. However, even the most

ironic relation between art and reality would be incomprehensible if art did not both disturb and rearrange our relation to reality. If the world of the text were without any assignable relation to the real world, language would not be 'dangerous'" (p. 6).

13 For a stimulating discussion of diagrams and mathematics that complements this view, cf. Knoespel 2000 and Châtelet 2000.

14 Cf. my unpublished essay, Bono 2000; an expanded version will be included in my book *Figuring Science*.

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Science and the Public
Pushing PUS with Science Studies

"Do you feel that science and technology will eventually solve most problems such as pollution, disease, drug abuse, and crime, some of these problems, or few, if any of these problems?" In 1972, the answer for 30 percent of the total adult US population was "most problems" (National Science Board 1973, 98). Some 25 years later, public attitudes towards science are still positive: "Science can eventually explain everything" – 53 percent of the general public agreed with this statement in a 1998 survey (National Science Board 2000: 8–14).

Surveys with questions like these have been done since the early seventies. From its first volume onward, the biannual US National Science Indicators have included a regular chapter on *Public Attitudes Toward Science and Technology*. In recent years, the title of the chapter has been changed to *Science and Technology: Public Attitudes and Public Understanding.* In fact, this change reflects a shift of interest: Although public attitudes are still a major issue, they are framed differently, namely as a complex dynamic between science and society. Notably, the nuclear accidents of Three Mile Island near Harrisburg, PA (1979) and Chernobyl (1986), and the controversial debate on nuclear energy stimulated a new public discussion on various aspects of science and technology. The institutionalization of technology assessment (TA) as a scientific discipline has been one of the results of this discussion. Moreover, the role of scientific experts in society has become heavily debated in the 1970s and 1980s, and, at the same time, scientists "discovered the media" (Weingart 1998). The "visible scientists" (Goodell 1977) appeared and "selling science" (Nelkin 1987) became a major issue. At the end of the 1980s, climate change and the ozone hole stimulated a new discourse in the arena between science, policy and the mass media (Weingart et al. 2000).

However, only with the beginning of the 1990s, the new field 'Public Communication of Science & Technology' began to intitutionalize. The PCST network was formed as a "loose international organization of individuals interested in all aspects of the relationship between science and the public" (*http://www.people.cornell.edu/pages/bvl1/pcst-net.html*). In 1992, the journal *Public Understanding of Science* was launched in cooperation with the Science Museum, London, and international conferences of the community are now held every second year.

The editor of *Public Understanding of Science*, Bruce Lewenstein, will introduce into the complex interaction of science, politics and the media. He argues that there is a fundamental contradiction between democratic ideas of equal participation and the meritocratic ideal that produces scientific elites. PUS programs produced by elite scientists who do not understand the public's perception and use of science will not serve the public well. In his view, PUS should not be about more information, but about a better understanding of the scientific process. Hence, one more reason to consider the insights of science studies.

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What Kind of 'Public Understanding of Science' Programs Best Serve a Democracy?

Bruce V. Lewenstein

Most justifications for government support of 'public understanding of science' (PUS) programs rely on the argument that responsible citizens in democratic societies need to make social decisions that involve science and technology. Yet there is a fundamental contradiction between democratic ideas of equal participation and the meritocratic ideal that produces scientific elites. One of the outcomes of this contradiction is a series of PUS programs that do not serve the public well, because they are produced by elite scientists who do not understand the public's perception and use of science. PUS programs are usually based on providing more and better information to appropriate publics. Data from various studies shows, however, that what people need is not more information, but better understanding of the scientific process. Not the mythological, 'hypothetico-deductive scientific method,' but the real, socially-mediated, patronage influenced, experimentally-underdetermined, theoretically-guided – in short, MESSY – scientific method. Natural scientists who attack the historians and sociologists who have described this method as 'anti-science' are shooting the messenger. The enemy are those members of society who deny the power of rational inquiry, not those who promote a more nuanced, contextualized understanding of how scientific knowledge is produced. PUS programs should rely more, not less, on the findings of historians and sociologists of science.

Twenty-five years ago, the astronomer Benjamin Shen offered three definitions for science literacy (Shen 1975). The first, he called *practical science literacy.* By that, he meant the knowledge of science that we need for living in modern society: that antibiotics are useful if you have bacterial disease, that automobiles work by converting fossilized potential energy into kinetic energy, that computers can do only what their programmers have instructed them to do.

The second type, he called *civic science literacy.* This kind of knowledge is what we need as citizens in Western democracies: the power of public health initiatives, the relative risks and benefits of coal and nuclear and solar power plants, the economic and political value of environmental regulations, and so on. Civic science literacy is not something you need on an everyday basis, but it is what you need to judge the decisions that your representatives in government and industry must make in *their* everyday activities.

Finally, Shen identified *cultural science literacy*. This is knowledge of science as the product of the human mind, something akin to music or art. We care about the Higgs boson, not because it helps us everyday or because it matters to our civic life; we care about it because to understand the Higgs boson is to understand something about Nature. To understand the helical structure of DNA and the amazing symmetry of As and Ts and Gs and Cs (adenine, thymine, guanine, and cytosine) is not needed to help yourself heal, or even to pass judgment on genetically-engineered foods; the DNA helix is simply an amazingly beautiful object. The ability to grasp the inherent beauty of DNA is what makes us human – both our own understanding of it, and our appreciation of the intellectual effort that went into elucidating its structure.

While other definitions of science literacy have been proposed or elaborated in the years since Shen's paper, none of them has the value of simple distinctions that Shen's definitions offer. All can be subsumed under Shen's definitions without doing terrible injustice to their meaning (Thomas / Durant 1987; Laetsch 1987).

When people talk about improving public understanding of science and technology, or supplying resources to the public communication of science and technology, they sometimes justify their remarks by calling on the need for better practical science literacy. This is especially true in developing countries, where problems of health, nutrition, water supplies, agriculture, and so on clearly can be addressed with specific scientific and technological knowledge (Schiele 1994). Sometimes, calls for more public understanding of science draw on the cultural science literacy justification; this happens most often, of course, in the developed countries, and most often in the writings of intellectuals concerned about human nature and the value of rational inquiry (cf., e.g., Holton 1965; Holton 1974; Snow 1959).

But by far the greatest support for public understanding of science activities relies on the civic science literacy argument. "Better public understanding of science can be a major element in promoting national prosperity [and] in raising the quality of public and private decision-making …," said a British Royal Society report in 1985 (Royal Society 1985: 9). "There are few, if any, public issues … that do not have a scientific or technical component. Conversely, issues that appear to be largely scientific or technical in nature mostly have major social and political implications." As one result, the Royal Society's report argued, "there is clearly a strong case for Parliamentarians, in particular, to have a much better understanding of science and its relevance to their responsibilities than they now have."

Similarly, when various provincial and federal branches of the Canadian government sponsored a major symposium on "When Science Becomes Culture" in 1994, the president of the *Conseil de la science et de la technologie du Québec* asked "Is the public able, or does it even desire to influence the political powers with regard to problems involving technology or science? Regrettably, science and technology belong all too exclusively to those who work in these fields." The general public must understand science, the minister argued, in order to guide the politicians (Berlinguet 1994).

In the United States, when the American Association for the Advancement of Science sponsored a major science education reform program, its definition of science literacy also focused on the importance of science for *citizens*, not individuals with immediate practical concerns or deep intellectual interests; the program (called "Project 2061")

> promotes literacy in science, mathematics, and technology in order to help people live interesting, responsible, and productive lives. In a culture increasingly pervaded by science, mathematics, and technology, science literacy requires understandings and habits of mind that enable citizens [*n.b.*] to grasp what those enterprises are up to, to make some sense of how the natural and designed worlds work, to think critically and independently, to recognize and weigh alternative explanations of events and design trade-offs, and to deal sensibly with problems that involve evidence, numbers, patterns, logical arguments, and uncertainties (AAAS 1993: xi).

Traditionally, promoters of public understanding of science – who almost always come from or have strong ties to the scientific community – have argued that improving the "quantity and quality" of scientific information available to the public would be the best way to help meet the civic needs of citizens (Lewenstein 1992).

The Fundamental Contradiction

Yet there is a fundamental contradiction between this pronouncement of the scientific elite and the simultaneous commitment to democracy, which specifies that individual citizens acting together should determine what best suits their needs and interests. Taken to an extreme, some attempts to resolve the contradiction lead to the various 'democratic science' movements that advocate citizen control over science and question the authority of science to govern itself (cf., e.g., Fayard 1988; Sclove 1995). Scientists recognize the contradiction, but deny that citizen control is the answer. Instead, they argue, better 'public understanding of science' will lead to better public support of scientific independence.

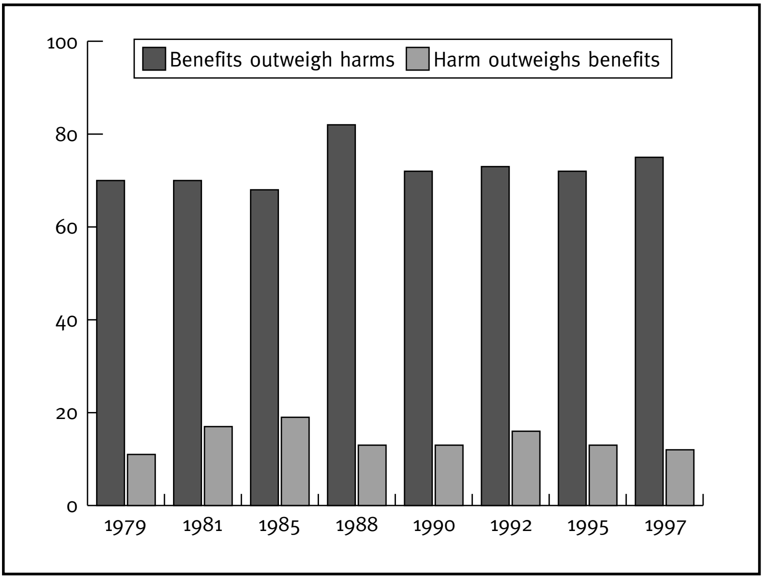
Why 'better'? While the broad public does, in general, have a good attitude towards science (though perhaps not towards technology), it also recognizes that scientists do not always have the answers – *even though* (and this may be the critical point for understanding complex public attitudes) scientists are often unwilling to acknowledge when they *do not* know the answer. That tension is the crucial issue. Given a commitment to public understanding of science that depends largely on the 'civic science literacy' idea, and given a supposed mismatch between the need for public support of science and the public's actual support of science, what kind of public understanding of science programs are needed in a democracy?

To answer that question, I want to call attention to the word 'supposed' in the previous paragraph. In the rest of this paper, I will argue (1) that public support of science (as shown by attitudes and images) *is* good; (2) that when we take seriously the idea that we must listen to citizens in a democracy, we learn something about science from them; and (3) that, therefore, our public understanding of science programs must address the issues of uncertainty and context that worry the public at large.

Public Attidudes Toward Science and Public Images of Science

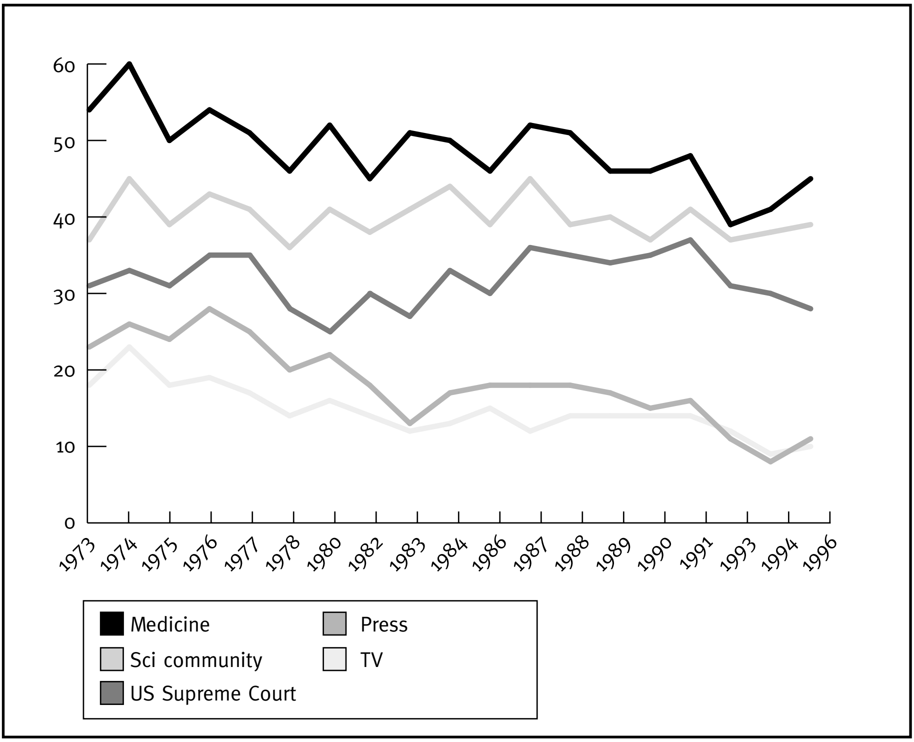
Consider first the evidence on attitudes (Figure 1). Americans overwhelmingly believe that science and technology make their lives better (NSB 1993; NSB 1998). More than 80 percent say that science and technology make our lives "healthier, easier, and more comfortable." (A comparable number of Europeans say the same thing.) Looked at from the other direction, less than 40 percent of Americans say that science and technology "make our way of life change too fast." (Here, some international differences do appear: 55 percent of Europeans and 57 percent of Japanese think science and technology are changing life too fast.) When asked about specific issues in the quality of life (such as public health, working conditions, and standard of living), generally less than 10 percent of Americans think science and technology have a negative impact. (The one exception is "world peace," where the positive impact outweighs the negative impact by only about 10 percent.)

Figure 1: Public assessments of scientific research. Data from NSB 1998, Appendix table 7–20 (Percentage of respondents)



Equally important, science comes off well when compared with other institutions in American society. In a series of questions asked since the early 1970s, Americans consistently rank science near the top of institutions they trust, putting lower such options as the US Supreme Court, organized religion, and the media (Figure 2).

Figure 2: Public confidence in leadership of selected institutions. Data from NSB 1998, Appendis table 7–19. (Percentage of respondents expressing confidence in leadership of selected institutions)



Another way to look at the public's attitude toward science is to consider the public *image* of science. Images cannot be quantified in the same way that social scientists measure formal attitudes, but we can get some suggestive information from both impressionistic and formal content analyses of media images. A number of studies have shown that the public image of science splits into two groups – heroes and horrors. The heroes are the scientists who provide benefits – cures for disease, new computer technologies, answers to the energy crisis (Lewenstein 1989). The horrors, of course, are the demons and mad scientists who would destroy life with science and technology. Frankenstein's monster – or is it Frankenstein himself? – represent the horror image of science.

Another way to think about the dichotomy between hero and horror is to describe the image of scientists as an image of wizards. Wizards and the power they wield can be either good or bad – or both. Any fan of Tolkien or viewer of the "Sorcerer's Apprentice" in Disney's *Fantasia* knows this. But in a recent survey of children's science shows on American television, Marilee Long and Jocelyn Steinke showed that the evil images of wizards are rare, whereas the good images, the images of scientists as truth-seekers, are prevalent (Nelkin 1995; Long / Steinke 1996).

Missing from most of these images is the everyday, *humdrum*, ordinary image of the scientist and engineer as a human problem solver, doing the best he or she can to solve the problems that face the world. (These images *are* appearing in the children's TV shows, probably as a direct result of the producers' realizing that the images do not appear anywhere else. But they're absent in most other places in popular culture.) Nonetheless, despite the presence of horror images and the lack of humdrum images, the hero image remains a powerful icon in our society. Science has provided us with prosperity.

But, as Marcel LaFollette (1990) suggested in an analysis of media images in American magazines throughout this century, prosperity can be oversold. Science has claimed credit for nuclear power, improved plastics and pesticides, and outer space. Science cannot suddenly disclaim its contributions to modern life after Three Mile Island, Bhopal, Chernobyl, and the Challenger explosion. The public is not stupid. When it sees the scientific community saying that 'public understanding of science' is equivalent to 'public appreciation of the benefits that science provides to society' – but also sees that science brings us things that are not necessarily good – there's a clear disjunction between the image and the reality. That disjunction, LaFollette argued, rather than the problems themselves, makes the public distrust science. (The same problem affects politicians who do not keep their promises.)

How people respond to the images of science in their minds is perhaps the crucial point. In the United States, many physicists and chemists in the mid-1990s complained about an exhibition at the Smithsonian Institution called "Science in American Life." They said it presents a negative, anti-science message, though the curators responded that their goal was simply to show the changing attitudes of Americans toward science. After visiting the exhibit hall, however, one of the sharpest critics of the exhibition had to moderate his remarks. “The kids thought it was terrific, they did not pick up any of the negative stuff,” he said (MacIlwaine 1995). Moreover, a study of visitors showed overwhelmingly positive reactions to the exhibit (Pekarik et al. 1995).

Those of us who care about science can easily highlight the images we do not like and complain about them – but we do so at our peril, ignoring our colleagues doing careful studies of how people actually respond. We know, for example, that individuals make risk decisions for very complex reasons that often go far beyond simple calculations of hazard probabilities (Kasperson et al. 1988; Slovic 1987; Hornig 1992; Hornig 1993; Priest 1995). Instead, they factor in the degree to which they dread the risk and the ‘signal’ effect of recent news about the risk. Perhaps most important, individuals make many risk decisions based on how much they can control the risk.

Understanding the Tensions: Control and Uncertainty

The issue of control – which is, of course, often central to political issues – is crucial if we are to understand public attitudes toward science and science literacy. Three case studies provide the data.

In 1986, after the Chernobyl nuclear accident, the Cumbrian hills of England were contaminated by fallout (Wynne 1989). Scientists at the British Ministry of Agriculture, Fisheries, and Food (MAFF) ordered sheepfarmers not to bring their sheep to market, except under certain conditions. The restrictions, they said, would be lifted shortly, after the fallout had been safely absorbed by the soil. The MAFF scientists, however, made a series of mistakes. First, they did not recognize the limits of their own knowledge. Their estimates of how long the restrictions would apply were based on soil models from a different part of England, models that had little relevance in the fells of Cumbria. The restrictions, originally imposed for three *weeks*, were still in place a *decade* later. Second, the scientists refused to acknowledge their own uncertainties. They continually asked the farmers just ‘to trust’ the scientists, even after the scientists had lost the trust by demonstrating how wrong their science could be. Contributing to this loss of credibility was that the scientists imposed their restrictions without considering the expert knowledge that sheepfarmers had about grazing habits, water runoff, and other issues relevant to the restrictions.

Also in England, indeed also in Cumbria, researchers were puzzled to find that workers at the nuclear reprocessing plant in Sellafield actively resisted acquiring knowledge about nuclear contamination (Wynne 1991). Surely if anyone needed to be 'scientifically literate,' it was people who work with extremely dangerous, very high technology on an everyday basis. Exploring the issue, however, researchers discovered that the workers did not want knowledge that they could not use. They functioned best when they stayed ignorant. Then, and only then, could they work efficiently by simply trusting their supervisors to design safe procedures. If the workers had developed independent knowledge – especially knowledge about the *uncertainties* of the scientific knowledge of the dangers they faced – they would have found themselves paralyzed. Without the authority to control or change their actions, but with the knowledge of the uncertain dangers they faced, their workplace would have become far more 'risky' than when they trusted their supervisors without the burden of knowledge.

Finally, across the Atlantic, a case in Canada shows what happens when you put these questions of control and scientific uncertainty into direct practice. While designing exhibits and community programs about mining to be used throughout the province of Alberta, program developers realized that members of the communities they visited were extremely aware of the uncertainties of scientific knowledge – and of the impacts these uncertainties would have on their communities (Bradburne and Wake 1993; Wake and Bradburne 1995). What the citizens wanted was not more 'knowledge,' but rather to learn ways of combining the knowledge they either had or could acquire with the uncertainties in that knowledge. They wanted guidance in action, not simple facts.

All three of these cases show us that public attitudes toward science, even if good in the aggregate, ultimately depend on the ways in which the public perceives that it can control science. In particular, when the public believes that scientists are making claims of certainty and authority that the public recognizes as untenable – then the scientific community loses its credibility. When the public sees science and technology that neither it *nor* the scientific community seems to have completely under control (including intellectual control), then it begins to fear science and give it the horror image, not the hero one.

Knowledge is Needed, But What Kind?

I want to make clear that I am not denying the problem of lack of knowledge. My late colleague Carl Sagan, the well-known astronomer and fantastically successful popularizer, once told a story about meeting a young man who was obviously enthusiastic about science. The man bubbled over with his excitement about the curiosity and cleverness of scientists. Then he began asking Sagan more specific questions, inquiring about the power of pyramids, UFO sightings, extrasensory perception, and a host of other pseudo-scientific 'findings.' Sagan was appalled. Here was someone clearly enamored of science – and absolutely missing all understanding of what constituted reliable scientific evidence, useful scientific method, and well-established knowledge about the natural world.

Like Sagan, I desperately want members of the public to learn to use their rational faculties more effectively. We must help more people learn to be skeptical, to question statements that are unsupported by facts. We need to improve education throughout our system, not just in science – in history, in language, in geography, in literature. Even in politics!

But when we consider public understanding in the context of democracy, we must recognize the conflict between the elite visions of science as a crucial component of progress in addressing national and international problems, and the democratic or popular visions of science that are much more nuanced and – sometimes – critical. When we accept that the democratic side of the contradiction as a deeply rooted, *reasonable* response to the what the public experiences, we are ready to see the conflict, not as an irrational uninformed barrier to understanding, but as a fundamental aspect of the organization of our society. Then we are ready to ask, how can we best serve the citizens of our democracies? What kinds of public understanding of science programs will help us move forward?

To some scientists, including some prominent ones, we improve science literacy by focusing on specific scientific knowledge. For example, ten years ago Robert Hazen and James Trefil, two physicists with very successful records as popularizers, put together 20 principles of science which they believed that everyone should know (things like "one set of laws describes all motion" and "everything is made of atoms") (Pool 1991). But many *scientists* disagreed with them (Culotta 1991). The absence of math and biology from their list drew widespread criticism. So did the attempt to create any such simple list. "I would object to the absolute and simple-minded terms in which [the ideas] are expressed ...," wrote Elwyh Loh, a medical professor at the University of Pennsylvania. The compilation "is baby-talk that reduces Science with a capital 'S' into Saturday morning cartoons."

Perhaps even more telling is the conclusion of the AAAS's Project 2061. Based on extensive work with psychologists and curriculum evaluators, Project 2061 has rejected the model of cramming more and more facts into students.

> If we want students to learn science, mathematics, and technology well, [the project's staff wrote,] we must radically reduce the sheer amount of material now being covered. The overstuffed curriculum places a premium on ... short-term memory and impedes the acquisition of understanding (AAAS 1993: xi–xii).

What, then, is the alternative?

Clearly, if we move away from science as bits of knowledge, we must look at science as a process. But – and here is where I part company with many scientists – we need to focus on the *real* process, not the mythical one of developing hypotheses, gathering data, testing the hypotheses, revising and repeating the process. Many "well-intentioned calls to combat scientific or technological illiteracy" fall into the trap of advocating facts rather than context, according to LaFollette (1995). Trying to maintain the cultural authority of science, scientists use the myth of the single, clear, all-powerful scientific method to defend themselves against charges that science is a tool of corporate capitalism, or a hegemonic opinion produced by cultural elites, or other attacks from postmodern critics (Gross and Levitt 1994; Levitt 1999). As LaFollette (1995) said,

> Describing scientific knowledge as if it emanated fully realized from a 'black box' does preserve scientists' cultural sanctity ... It also neatly circumvents explanations of research values and goals. Effective modern citizenship [*n.b.*] demands a higher level of 'knowing about' science, however. It is enhanced by fuller explanation of why scientists recommend one thing or another, and of what underlies their standing as experts.

Like LaFollette, I believe that we need to teach something about the context and process of science. But what do I mean by process? Not the so-called 'scientific method.' Historians, philosophers, and sociologists of science over the last generation have convincingly demonstrated that while scientists often call upon a standardized method (especially the 'hypothesis, test, conclusion' model) for rhetorical purposes, the actual processes by which scientists acquire knowledge are much messier and more complex. In *Scientific Literacy and the Myth of the Scientific Method*, chemist Henry H. Bauer (1992) argued that we should be focusing on the social processes of communication, collaboration, and communal judgment to understand how random hunches, observations, and ideas about nature become transformed into reliable understanding of the world around us. The physicist John Ziman (2000) has recently made a similar argument.

The problem is that the messy reality of scientific life, including especially the degree of social interaction among scientists, government agencies, industrial sponsors, audiences, and publics that leads to reliable knowledge, is anathema to scientists who believe that science is fundamentally a search for Truth and Nature. It is very difficult to accept that scientific consensus is shaped by power relations, political contingencies, interpretive flexibility, rhetorical constructions, and other elements of social behavior that together go by the label 'social construction.' But a careful reading of historical and sociological records clearly shows that scientists use social activities to achieve their understanding (Jasanoff et al. 1995).

Robert Smith's prize-winning history of the Hubble Space Telescope (Smith 1989), for example, follows in exquisite detail the process of committees, reports, personal persuasion, political manipulation, and other fundamentally social processes by which astronomers reached consensus on what was worth studying and how – in the very technical sense of which instruments, built with which capabilities, to which tolerances, with what specifications – the astronomical community should go about studying space. The decisions made by this complex process directly affect what we know about the natural world.

Or consider a much earlier time: in 1610, as Galileo learned to use his new telescopes for observing the planets, his decisions about what he had found were shaped by his campaign to get, and then keep, a patronage position at the court of Grand Duke Cosimo de' Medici (Westfall 1985). He first observed three moons of Jupiter; but the

Grand Duke was one of four brothers. Not until he found a fourth moon could he announce his discovery of the "Medicean Stars." Similarly, his decision to engage in a systematic survey of the planets was shaped by his promise to the Grand Duke to provide "many discoveries and such as perhaps no other prince can match, for of these I have a great many and am certain I can find more as occasion presents itself." If he failed to meet his promise, his salary and support would disappear.

Implications of the Social Model of Science

What are the implications of this belief in the fundamentally social nature of the scientific process for the general effort to create greater public understanding of science? Most immediately, when we decide to focus on the 'process' of science, we must mean the *real* process by which science achieves its powerful status in society, not an idealized and abstract 'scientific method.' We cannot present scientific authority as somehow beyond the criticism we make of arguments based in religion or myths.[1] For, like each of these 'nonscientific' fields, science achieves its power only through the socially-constructed consensus among its practitioners that is then used as a rhetorical tool to fashion a broad social consensus that it provides answers unobtainable through other means. Religion and myth play continuing roles in modern life not because people are ignorant, but because the insights and satisfactions that come from these fields satisfy deep human needs. We need to understand that science achieves authority because *we* have *agreed* to give it authority – agreed based on the evidence supplied and defended through a complex social process.

To conclude, consider again the notion of civic science literacy. We do need citizens who know something about science. But we need to go beyond simple declarations of that need. For the kinds of decisions we want people to make *as citizens*, we want them to know something more than simple facts about Nature. We want citizens to know how science produces reliable knowledge about Nature – and especially how social forces at both the individual and societal levels help shape the production of that reliable knowledge. Only then will citizens be in a position to tell their democratically-elected representatives how to proceed on political issues that involve science and democracy.

Notice that I am *not* advocating the kind of 'democratic science' in which citizens *make* the decisions themselves, a sort of 'science by majority vote.' People who advocate this kind of citizen participation are deeply skeptical of scientific and technological expertise. I do not share that skepticism. The natural world imposes powerful constraints on what we can do, as individuals and as citizens. We need scientific and technological experts who use their professional skills, including their professional judgment, to tell us about those constraints. But I recognize, as many scientists who defend the so-called "scientific method" in knee-jerk fashion are apparently unwilling to recognize, that our *knowledge* of the natural world is deeply shaped by social factors. As citizens, we must understand the contexts in which knowledge of Nature is produced, and how different social forces might produce different sets of knowledge – which, in turn, might lead to different social decisions about how to move ahead on difficult public policy issues that have scientific and technological components.

Some of the scientists who I am criticizing believe there is a war between critics of science and science itself (cf., e.g., Holton 1993; Park 1994; Wolpert 1992; Gross and Levitt 1994; for more nuanced views, cf. Labinger 1995; Labinger 1997; and Labinger/Collins 2001). Most of these scientists have focused on the historians and sociologists who have produced what I believe are honest and faithful portraits of how scientific knowledge is produced by a social process. The scientists are, I think, shooting the messenger. There is an enemy, and it is those who deny the power of rational discourse (fed by evidence evaluated by a social process of testing and consensus-building and trust) to teach us something about the natural world. But the way to deal with the enemy is not to insist on the primacy of technical expertise before we even begin the discussion. That leads to war. The answer, as in any diplomatic negotiation, is to begin by talking, by listening, by *hearing* the other side. What is it about science that feeds and promotes the horror image? Why do people actively choose to be ignorant about science? What kind of information do people – nonscientist *citizens* of our democracies – want to know?

Once we have begun talking, we can build the trust and respect on which mutual understanding can build. That understanding probably will not be a commitment to cultural science literacy – because, for most of the public, science as culture will never have the appeal of rap music and earrings and the political sex scandals. Nor will mutual understanding end up focusing on practical science literacy, because the public will come to understand – I hope and believe – that developing scientific knowledge requires a level and breadth of curiosity that cannot be tied to practical concerns.

In the end, mutual understanding will focus on civic science literacy – because that is the place where the social context of science brings together the real process of science (the one I've described above) with the real interests and needs of the public.

In this paper, I have not tied my comments to theories of political systems or democracy. We still need people to do that. But, like a good scientist, I think I've mucked about in the data of public attitudes towards science, public images of science, and the nature of science itself to show that prevailing approaches that treat the public as ignorant, passive couch potatoes cannot be justified. Instead, an alternative interpretation treats the public as active members of a democracy and respects their perspective on science as one produced by realistic encounters with the products of scientific and technological inquiries.

And also, like a good scientist, I'm left with many more questions about whether the approach I'm suggesting will work. To answer those questions, we need more data.

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Note

1 I do not mean "myth" as in false, but as in "story" – a story based in reality, but abstracted for its moral lesson rather than for its faithfulness to what scientists actually do. I thank Gerald Holton for challenging my use of the term.

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Knowledge Politics

The Paradox of Regulating Knowledge Dynamics

Science has not only led to the mass production of knowledge but also has it invaded society with multifarious effects: Consequently, today one talks about knowledge in the plural, for whereever knowlege is produced counter-knowledges occur. Therefore science studies has put a novel issue called knowledge society on the agenda: Scholars inquire into its texture (Böhme, Stehr 1986; Stehr 1994) as well as into its type of knowledge production (Gibbons et al. 1994; Willke 1998, 1999). While it is not as yet decided what a society based on knowledge will eventually look like it seems to be certain that we face up to some fundamental dilemmata of knowledge: Implementing knowledge inevitably means to adapt it to local conditions, thereby changing it. What is more, knowledge may prove not only useful and profitable but also risky. While societies promote systematic production of knowledge so as to improve individual well-being and collective standards of prosperity, health, and freedom, neither the quality of knowledge thus produced nor its effects once it has become implemented can be adequately foreseen. At issue is nothing less but the control of the unforeseeable.

Since technology and science, far more than economy, have become the real motor of societal change, institutions became established that debate and assess their potential or real effects before or while implementing them. So-called technology assessments, mediations, hearings or round-tables are designed to control or police knowledge, the main strategies being to minimize dangerous effects and to maximize public acceptance. Whereas two decades before nuclear energy or military research has been the primary concern of such interventions, today the attention has shifted to the biological and environmental research: Medicine, food, and nature are conceived as key issues deeply affecting individual lifes and societies at large. In the light of 'genetic engineering,' for instance, the issue is about making individual choices, privatize knowledge, and legislate its accessability. Thus, what is at stake today is the intricate relationship between the individual, economy, and the state: If anything, they share a common interest in regulating knowledge so as to keep the ideological, cultural, and moral effect of science and technology under control. While Merton's norms are still part of the game named quality-control of knowledge, its regulation from within science does no longer seem sufficient. External regulation are sought to highten the efficacy of policing it: Drug regulation, intellectual property, and copyright protection are examples of the ways in which the distribution and implementation of knowledge becomes a domain of explicit legislation and a target of political and economic decisions. To be sure, regulating knowledge is not about 'reducing' it (though shortage of availability and accessability are forms of policing knowledge). On the contrary: Regulating knowledge will enforce the significance of knowledge, thereby disseminating the places where knowledge becomes implemented, disputed and adapted. Policing knowledge, thus Stehr as well as Weingart (2001), inevitably increases the dynamics of a knowledge-based society.

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Policing Knowledge

Nico Stehr

Early one morning in late July of 1999, Lord Melchett, the head of Greenpeace in Britain, was detained for questioning by the police after he and about 30 Greenpeace members raided a field of genetically modified maize near Norfolk. The protest came to an abrupt end after the farmer called the police and they arrested the protesters. According to *The Times* (July 27, 1999) the raid left government trials of seed crops that had been genetically modified in disarray. The farm on which the protest took place was one of seven test sites damaged or destroyed within months. The protest by Greenpeace followed a recommendation by the Association of Local Governments to its 170 members in England and Wales to phase out genetically manipulated foods (or GM food) until they are proven safe. A number of councils followed the recommendation. Major food manufacturers and supermarket chains as well as fast-food chains in Britain had already announced that they will not carry any products that contain genetically modified ingredients. A poll in the summer of 1999 found that 79 percent of the British public agrees that GM crop testing should be stopped. In Canada and the United States, genetic modification of foodstuff has hardly been questioned by the public. A major political battle on this front between North America and Europe is likely.

In January of 1999, the *Daily Telegraph* (January 22, 1999: 9) reported that the British Medical Association warns, in a report entitled *Biotechnology Weapons and Humanity*, that rapid advances in genetics will "soon transform biological weapons into potent tools of ethnic cleansing and terrorism." The British Medical Association urged that the regulations of the 1972 International Biological and Toxic Weapons Convention should be tightened and improved, anticipating the possibility of genetic warfare which is a practical possibility today.

The so-called 'genetic protection initiative' (the petition for a referendum 'for the protection of life and environment from genetic manipulation') in Switzerland was clearly rejected in June 1998 in a plebiscite in all the cantons, to the great 'relief of the pharmaceutical industry' (*Neue Zürcher Zeitung*, June 8, 1998). With a voter turnout of 40.6 percent, 66.6 percent of voters opposed the petition, which according to its advocates would have declared Switzerland to be a great, unified 'genetic protection area'. The petition demanded, among other things, changes to the Swiss constitution forbidding the production, purchase and sale of genetically modified animals, the release of genetically modified organisms into the environment and the granting of patents for genetically modified animals and plants.

The fact that all of my examples of recent attempts to regulate the application of knowledge deal with genetic research – and the list could easily be extended – is, of course, a result of the fears and/or nightmares which have lately been prompted by just this area of research.

Knowledge Politics

In this contribution I plan to discuss what may well become one of the most significant and contentious issues for intellectual, legal, public, scientific and political discourse in the coming century: the growing pressure to police *novel* knowledge – or in other words, the emergence of a new field of political activity, namely knowledge politics.[1] In democratically organized societies, it is a legitimate role of political discourse and action to contribute to and take part in decisions that effect the ways in which scientific knowledge and possible technological artifacts are deployed in society or not.

During the early post-war decades of rapid economic growth, the application of scientific and technical knowledge in developed societies was not necessarily unanimously and uncritically advocated, to be sure, but there was a considerable degree of silent assent.

Such headlines of recent times as 'We know too much' and 'How much genetic self-knowledge is good for us?', or keywords from ever more vehement disputes, such as 'We dare not make use of everything we know', are part of the background and the environment of the current increasingly urgent demands for the regulation of knowledge in modern societies.[2] These science and technology controversies open a window on modern struggles over meaning and morality, economic benefits and damages, as well as the emerging and shifting locations of social power and control in knowledge societies.[3]

More specifically, it is the shift from regulating and policing normality or identity (Foucault) to the growing concern in knowledge societies with efforts to police novelty and differences. As I have indicated, the examples that come to mind, and that have captured the attention of the media and the public recently, are numerous and growing.[4] For example, the United Nations, provoked by advances in ocean exploration, is drafting a treaty that attempts to regulate marine archeology and commercial efforts to hunt for and reclaim lost cultural treasures – and therefore the knowledge about ancient civilizations, such as the empire of the Phoenicians, that may come with their discovery (cf. *New York Times*, October 12, 1998).

It is perhaps self-evident and comes as no surprise to anticipate that 'knowing' will be seen in knowledge societies as a domain in urgent need of policing and as a site to study the functioning of power in modern society.[5] Inasmuch as the widespread dissemination of knowledge increases the fragility of modern societies (cf. Stehr 2000) efforts designed to control knowledge may be interpreted as strategic attempts to reduce or manage their fragility. Whether such attempts are likely to be successful is therefore an important issue.

But the issue of the control of knowledge becomes significant for another reason as well. Insofar as knowledge, especially 'additional' knowledge, assumes growing importance within the economic system and becomes subject to economic interests, efforts to control, restrict or privatize its use will grow as well. A prominent example comes from genetic research and the Human Genome Project in particular. In light of the intensive competition among hundreds of researchers worldwide in the Human Genome Project, the concern intensifies that findings that might 'alter the world economy' will be monopolized, at last temporarily, if they can be protected by patents or other forms of intervention by the state. And since it is not only knowledge about genes that may turn into a valuable raw material, the fear of a progressive privatization of science grows.

Finally, demands to cope with the growth of knowledge refer to the attendant extension in capacities to act. Actors increasingly find themselves in situations in which the need for novel decisions emerges; and with it, of course, new apprehended dangers and risks. The potential openness, and not the self-evident traditional closure, of situations calls for, it seems, regulation and policing of knowledge now that knowledge is seen as the motor of new possibilities to 'manipulate' elements of a situation that in the past had been apprehended as beyond the control of all participants. The role and the prominence of references to fate, nature or the design of some higher being that symbolized the closure of conditions of action lose their relevance. What was seen as forever beyond the control of everyone now becomes – initially in the thought experiments of a few individuals, at least – subject to control and manipulation. And what was in the past seen as an exceptional moral dilemma, or the need to arrive at a decision in an extreme situation or under rare circumstances, now becomes increasingly common.

Regulating Knowledge

Efforts to police knowledge are not new. The notorious and ongoing struggle in some parts of the United States, for example, to ban the teaching of evolution in schools is therefore a relevant case in point. The vote of the Kansas Board of Education to delete virtually any mention of evolution from the state's science curriculum[6] is one of the more recent examples of successful efforts of creationists to ban mention not only of biological evolution but also of the big bang theory from the curricular guidelines of schools in the United States. But most of the efforts to regulate and police the possible ideological and cultural effects of science that have been and continue to be undertaken from time to time in different societies have not been overly successful.[7] In addition, there is a distinctive shift in the kinds of concerns and consequences that may prompt efforts directed toward the regulation of knowledge. In the last couple of decades, there is a noticeable shift from concerns that revolve around security, to concerns with risk and now more and more to questions of uncertainty (cf. Bechmann / Stehr 2000).[8]

A transformation in public sentiment in favor of policing knowledge signals a basic change in the legitimacy of science,[9] in particular a shift away from a preoccupation with the 'ideological' or cultural implications of basic knowledge claims generated by science and possible conflicts with established world views, and toward an increasing preoccupation with its practical application and consequences. What I have in mind is perhaps best described as an attempt to directly control or regulate the immediate use or anticipated consequences of incremental knowledge but not the '*secondary*' implications of knowledge.[10] Attempts to police the secondary consequences of knowledge claims could refer, for instance, to action in the form of regulations prompted by the claim that passive smoking increases blood pressure. Efforts to curtail smoking in certain spaces or by certain individuals may be based on and justified by this claim. But in such a context the claim itself is not the subject of any regulation.

The now widely discussed public demystification of experts may be seen not only as a prime example of a fundamental change in the nature of the relations between knowledge-based occupations and clients, consumers, patients, students, trainees, customers, etc., but also as a profound transformation in the public image of scientific knowledge. This change considerably enlarges the number and range of individuals who relinquish their traditional subordinate role in such expert/client relations as recipients of advice that rests on an *a priori* suspension of doubt. Helen Lopata has described the process I have in mind as the 'sophistication and the rebelliousness of the client' in contexts in which expert knowledge is dispensed (Lopata 1976: 437). Lopata notes that several social changes are responsible for the difficulty in monopolizing knowledge, by the professions for instance, and for the refusal of consumers and clients to remain passive and conforming recipients of expert advice. There is, first of all, the very increase in the volume of knowledge-based occupations, which reduces the ability to strictly enforce and control the boundaries and the nature of discourse and increases the fragmentation of fields of expertise. The fragmentation of expertise becomes public knowledge. Secondly, the astuteness and cognitive skills of the public increases. New organizations and pressure groups emerge, reinforcing the decline in the authority of experts.

Efforts to regulate and police knowledge are typically undertaken and/or initiated as well as legitimated outside the boundaries of the scientific community (naturally with repercussions for the production of knowledge within the science system). For the purposes at hand, 'regulating' refers, in the most general sense, to the conscious, strategic use of political and legal power, as well as economic resources and cultural authority, to shape – whatever the specific objective – the utilization of scientific-technical knowledge.[11] It involves a complex set of mainly formal ventures designed to encourage, restrict, shape, or banish knowledge claims and set standards for their use through pressure, the creation of institutions, and the deployment of norms and beliefs to make certain that knowledge evolves along a desired path and has only sanctioned consequences.

The source of the standards chosen to police knowledge, the regulatory procedures put in place, and the intellectual systems legitimizing the cultural dismissal of certain uses of knowledge typically also do not originate in science and technology itself. For example, in the face of demands to preserve and defend the nature of human nature in response to developments in scientific and technical capacities to alter the *status quo* of human reproduction, scientific 'notions of nature do not provide us with unambiguous standards of naturalness to which we can appeal for normative orientation' (van den Daele 1992: 549). Since scientific notions of naturalness allow for the construction of a range of possible natures, regulation efforts advancing the cause of abstaining from practical steps intervening into human nature have to appeal to moral claims and political action that may or may not succeed in arresting human nature. The anchoring of standards and justifications outside of science does not mean that individuals who are scientists may not be found among those who vigorously support attempts to regulate knowledge.

My list of the available measures to control knowledge may at first leave the impression that I include science and technology policies as primary examples of such efforts. Strategies designed to regulate knowledge are mostly responses to changed and novel knowledge, not vice versa. Science and technology policies aim to encourage the development of knowledge, but they generally do so in highly ambivalent and open-ended fashion. Many decades of experience demonstrate, furthermore, that it is difficult or even impossible to steer and control the dynamics of developments in science and technology by way of political standards (cf. van den Daele 1992: 553–555). In contrast to strategic efforts designed to plan and encourage future knowledge, attempts to 'police' knowledge cover a much wider social field than science and technology policies, including more informal control processes. The controls knowledge politics may impose could extend to the ways in which knowledge is disseminated and travels, is dispensed, made accessible, employed and interpreted.

The ideal-typical concepts of research and knowledge policies and their separate strategic functions for the development of knowledge and its societal deployment may increasingly be blurred in knowledge societies as the boundaries of science and society become more fluid and porous. Efforts to regulate knowledge will influence science policies and sciences policies will have an impact on attempts to police knowledge.

Shifting boundaries between science and politics for example may be manifest with respect to the process of the fabrication of knowledge; in particular, the emergence of cognitive closure, consensus formation or the evolution of uncontested facts in scientific fields increasingly may incorporate non-scientific actors and non-systemic groups. The more or less direct intervention into cognitive processes in science perhaps is most evident in the case of problem-oriented research such as environmental research, risk and technology assessment. Some fields of medical research may serve as another example. In France, the involvement and support of patient groups for the treatment of muscular dystrophy has lead to considerable investments by their organisation into molecular biology and the human genome (cf. Latour 1998: 208).

The Social Control of Knowledge Claims in Science

In yet another sense, the social control of knowledge claims *in* knowledge-rich and knowledge-based social systems is not a novel phenomenon. What makes science unique among social systems, for example, is the way in which and the extent to which the social task of maintaining the 'quality of the products' of science is accomplished 'with so little difficulty that the problem of quality control has received no more than passing mention in any systematic discussion of science' (Ravetz 1971: 273). Assessment of 'quality' is constitutive of much of the work done in science.

For Karl Popper, as is well known, the willingness to submit ideas to critical scrutiny and commitment, and not to accept knowledge claims at face value, constitutes the demarcation criterion between science and other social systems, including systems driven by ideas. Whether or not such a demarcation criterion linked to the motives of individual scientists and the institutional norms allows us to distinguish in an unambivalent manner between science and other increasingly knowledge-based social institutions is not at issue in this context. Nor do I intend to inquire into the functions of quality control, how the standards of the quality control may be elaborated, the precise mechanisms and enforcement of the social control of knowledge in science, whether these processes are effective in weeding out 'shoddy science', and how science may be stratified with respect to the policing of knowledge. Much has been written about these matters in recent years. Quality control in present-day science is clearly no longer as invisible and taken-for-granted as in the past. However, a more extensive discussion of the internal control mechanisms of science is accompanied by skepticism about the efficacy of self-policing, and therefore by demands that control within the scientific community must become a strictly formalized undertaking. In a society that is itself knowledge-based, the problem of the social control of knowledge both within and outside of science inevitably becomes a central social and political problem

The social regulation *of* science-in-progress is a highly difficult and perhaps impossible undertaking that, furthermore, has the unintended consequence of reducing the authority of science as an asset to politics. Perhaps the most significant barrier in the way of extensive external social control mechanisms on science-in-progress is the size and organization of the scientific enterprise today, as well as its competitive and its international texture.[12] The politics of science must not be conflated with the politics of society. The politics of knowledge cannot simply be reduced to political power, and science generates many kinds of knowledge, not only knowledge that is essentially political and therefore of immediate practical use.

The Societal Regulation of Knowledge

It seems highly likely that not only the state and major social institutions, but also social movements and groups of affected 'laypersons', will demand and organize to implement measures to increasingly regulate knowledge. In the past two decades, for example, AIDS research in the United States has been marked 'by a sustained lay invasion of the domain of scientific fact-making' (Epstein 1996: 330) breaking down some of the entrenched barriers between science and society.[13] The experience of AIDS research signals that efforts to control the application of knowledge – in this case prominently the aspects of who is to benefit, when and for what 'price' – has repercussions for the development of knowledge in academic science and for research and development in corporations.

It is perhaps self-evident that the growing efforts to police knowledge signal that claims about the inevitability of a self-propelled domination of society by science are simply unsupportable. The specific issue I will therefore discuss is not what I consider almost beyond dispute, namely that the deployment of control and regulation measures will increasingly be aimed at knowledge, but rather the entirely unresolved issue of the likely efficacy of all efforts to police knowledge. There is a yawning gulf between approaches that stress the ease with which knowledge is monopolized and controlled by an elite and the very different perspective advanced here, which emphasizes the extent to which the expanded role of knowledge significantly diminishes the ability of either major societal institutions or small groups to harness without serious challenge the gains that result from the growth of knowledge.

During the evolution of industrial society, liberal democracies successively instituted increasingly elaborate legal frames pertaining to the social status and use of property and labor. Thus the freedom of economic actors to exercise power and authority by virtue of their individual or collective ownership over labor power or the means of production is increasingly constrained and circumscribed by a host of legal norms, as well as organizations and political programs that emerge around these factors. Ownership is restrained not only spontaneously by the market, for example, but also by the state. Deliberate and anticipatory legal constraints on the use of property and labor are not neutral. Legal norms convey, from the point of view of certain actors, especially those who feel impotent in acquiring ownership and in affecting the legal rules pertaining to their disposition, privileges; while they signal (natural) rights to those who control property and labor. Unequal access to ownership, and therefore any stratification of effective influence on the construction of the legal restraints and rights, is in turn typically – but not always exclusively – based on an unequal distribution of labor and property in industrial society, elements that are constitutive for its social and economic existence.

It is almost self-evident that *legal* efforts and legislation in knowledge societies will be increasingly directed toward ways of controlling the employment, and indirectly the development, of knowledge. I emphasize political and legislative efforts to control the implementation of scientific knowledge rather than more tenuous forms of informal or spontaneous social control because the latter are simply part and parcel of the conventional state of affairs of science and its relation to society, namely the standard selectivity with which knowledge develops and is utilized. Vigorous opposition to political ventures to limit the considerable autonomy of the modern scientific community and to control knowledge will be as common as was opposition to efforts to control the use of property or the ways in which labor power might be utilized by the owners of the means of production.

One question that must be examined in the face of demands for the regulation of scientific findings has to do with the problem (which is not merely a new problem) of the extent of the social independence of science, its origins, its foundation and development; as well as the demand, which under certain circumstances opposes such independence, for some kind of control over scientific development, the communication of scientific findings and/or the consequences of scientific knowledge, whether through a kind of voluntary self-control by scientists or by means of externally implemented measures.

The type of control over science that is chiefly of interest here is therefore not related to the (primary) social control of scientific findings, that is to say, to forms of control that arise from the existence of such social constructs as the 'scientific community' itself. The system-specific regulation of knowledge has already been mentioned. Robert K. Merton, in one of the most influential treatments of this topic, has attempted to describe the peculiar form of primary or system-immanent social control in the modern scientific community by drawing attention to the existence of a number of special social norms that regulate the social relations among scientists. The presence of a particular social convention, such as for example the demand for unimpeded access of all scientists to all research findings, which also simultaneously means a ban on any form of secrecy or selective communication of scientific results, represents, no matter what attitude one takes to the concrete rules of conduct, a form of social control that influences or regulates, for example, the possible content, extent, goals and methods of communication. In summary, only a limited palette of possibilities from a multitude of other possibilities in the relevant context can be realized. In terms of primary social control, it is therefore a matter of a control taken for granted by scientists, and of a form of constraint on their social and intellectual life that is largely regarded as legitimate and necessary. Whenever the control and/or the freedom of science are under discussion, this taken-for-granted social control cannot be at issue. This control, which certainly must vary in its extent and manner and in the degree to which it is accepted, is, if you like, one of the indispensable resources of the social cohesion or solidarity of any institution, and thus of the scientific community as well.

Against the background of system specific social control within science, therefore, those discussions that lead to a revision or extension of the already existing forms of control in the scientific community are of interest. With mounting efforts outside of science to regulate new knowledge produced by science, the nature of social control within science is bound to be effected and changed. I do not merely mean to refer to what constitutes a kind of anticipatory regulation of research efforts and the informal or formal acceptance of zones that constitute investigatory matters and methods that are off limits, for instance, in the form of ethical certification requirements. In fact, what can and likely may increasingly occur is a convergence or mixture of regulatory practices.

Appended to the United States Human Genome Project is an NIH/DOE Committee to Evaluate the Ethical, Legal, and Social Implications Program of the Human Genome Project (ELSI). The committee has a short but contested history. The National Institute of Health (NIH) has proposed to attach ELSI units to its other institutes and research endeavors (cf. Murray 2000). Such a program, though peer-review based but not in the usual sense since assessments of research proposals are interdisciplinary, represents at least an enlargement of traditional system specific mechanisms of social control in science if not, in this instance, an intrusion of the state and the public concerns into the regulation of the development of knowledge and obviously difficult anticipatory judgments about its social implications. Such committees also raise the general question of the role of democratic order and the influence civic society ought to have on the ways in which the results of scientific research are deployed if at all.

The Public and Science

And in this context, the 'loss of contact' (Holton 1986: 92) between science and the larger public is today emerging as a salient attribute of the interrelation between knowledge and society. Large segments of the public have become disenfranchised, at least in the view of the scientific community. This loss of contact is not only the result of a growing cognitive distance between science and everyday knowledge; it is also affected by the ever increasing speed of knowledge expansion and by the deployment of knowledge as a productive capacity. The decreasing cognitive proximity increases the political distance from science, for example by restricting public reflection on both anticipated and unanticipated transformations of knowledge resulting from the application of knowledge.[14] The scientific community shares responsibility for this diminishing intellectual proximity, since the preferred self-image of science as a consensual, even monolithic and monologic, enterprise is increasingly in conflict with both its public role and its own internal struggles about research priorities, as well as the generation of data and their interpretation.

However, on political and moral grounds many groups, constituencies and institutions must be consulted before decisions are made about issues that affect the regulation of knowledge and indirectly the development of science and technology. It would be misleading to think that the distance from and the loss of contact with science, or the considerable scientific illiteracy in modern societies, is somehow a 'potentially fatal flaw in the self-conception of the people today' (Holton 1992: 105) and/or signals the possibility of a dramatic collapse in public support for science. It is more accurate, perhaps, to speak of a state of precarious balance affecting the autonomy and dependence of science in modern society. A loss of close intellectual contact between science and the public is perfectly compatible with both a diffuse support for science in modern society and an assent to legal and political efforts to control the impact of science and technology. In another sense, however, the loss of cognitive contact is almost irrelevant, and highly controversial; for example, when 'contact' is meant to refer to close cognitive proximity as a prerequisite of public participation in decisions affecting scientific and technological knowledge. Such a claim is practically meaningless because it almost requires public engagement in science-in-progress (cf. Collins 1987: 691).

From the point of view of the scientific community, the lack of cognitive proximity to the general public has advantages and disadvantages. The loss of contact between science and the public can perhaps explain, at least in part, why the scientific community, in view of its attractiveness and usefulness for corporations, the military and the state, has been able to preserve a considerable degree of intellectual autonomy (cf. Gilbert/Mulkay 1984). Such autonomy, however, is contingent on a host of factors within and without the scientific community. The loss of contact is a resource for the scientific community. It signals a symbolic detachment and independence that can be translated into an asset vis-à-vis the state and other societal agencies. Science becomes an authoritative voice in policy matters; or it represents, in ideological and material struggles with other political systems, the openness of society (cf. Mukerji 1989: 190–203). But the cognitive distance also limits the immediate effectiveness of the 'voice of science' in policy matters,[15] and extensive autonomy and independence of science may result in an excessive celebration of 'normal' scientific activity and lead to a lack of innovativeness.

From the point of view of the non-scientific institutions, the lack of intellectual proximity of the public to scientific knowledge in general and research fronts in particular also has both advantages and drawbacks. Selected disaffection with science and technology has always accompanied its development; strong demands and efforts to legislate selectivity in the ways in which knowledge is implemented and deployed can lead to even stronger disaffections with science, although such a response may be dismissed as part of an anti-science crusade or movement. But the term 'anti-science' is vague and brings together a broad range of things that typically 'have in common only that they tend to annoy or threaten those who regard themselves as more enlightened' (Holton 1992: 104).

The Developments of Social Controls

The social control and regulation of scientific knowledge that has moved from the stage of being-in-progress to some form of completion and desires to be implemented outside of the scientific community is already quite extensive. In all modern societies, we now find elaborate drug regulations and corresponding agencies that register, test, control or permit pharmaceutical substances to enter the market as legalized drugs. Until a few decades ago, decisions about the production and marketing of chemicals as drugs were typically made by corporations, by individual pharmacists or by physicians (cf. Bodewitz et al. 1987). As scientific knowledge is 'applied', it becomes embedded in social contexts external to science. As a part of such embeddedness, knowledge is subject to the kinds of control mechanisms and social constraints found in these contexts. It simply cannot escape the selectivity that issues from such external contexts, even if only in efforts designed to generate trust toward a certain artifact or solution offered by novel knowledge.

The whole area of national and international intellectual property and copyright protection is another arena in which legislation to control the deployment of scientific and technical knowledge is already extensive. In many ways, such controls date back at least to the 1883 Paris Convention for patents and related industrial matters and to the 1886 Berne Convention for copyrights. The acceleration in the speed with which inventions reach the market, their shortened economic life-span and the extent to which recent inventions, for example in the field of microelectronics, the organization of production, medical treatments and biotechnology, are difficult to protect from copying efforts will increase pressures to enact further protective legislation (cf. Vaitsos 1989).

In social theory, the institution generating knowledge and the institution contemplating and executing political action were once regarded as entirely unrelated domains. At the beginning of the twentieth century, the dilemma of the indispensable separation of science and politics found perhaps its most influential expression in Max Weber's ([1921] 1948: 77–128; [1922] 1948: 129–156) essays on science and politics as a vocation. Today, the intellectual foundations that allowed Weber to legitimize the fundamental division between the practices of knowledge and politics have fallen into disrepute. Confidence in the neutrality, instrumentality and political neutrality of science has been thoroughly eroded. Reference to the politics of knowledge therefore no longer constitutes a profound break or a violation of the norms of scientific action and the essentially means-like attributes of scientific knowledge. Science is deeply implicated in social action and political agendas hold sway over science. Precisely how dependent or interdependent science and politics are is a matter of ongoing debate and empirical analysis. But the widespread disenchantment with science and the extensive material dependence of the scientific community on the state do not justify the equally unrealistic proposition that the boundaries between politics and science have altogether vanished. Science remains embedded in particular political realities, and as long as it is situated in a form of civil and political society free of totalitarian strains, scientific activity tends to benefit. By the same token, as long as traffic across the boundaries of science remains widely unimpeded and subject to negotiation, both science and society gain.

In as much as knowledge becomes the constitutive principle of modern society, the production, distribution and especially the application of knowledge can avoid political struggles and conflicts less than ever. The distribution and implementation (and with it the fabrication) of knowledge increasingly becomes a domain of explicit legislation and a target of political and economic decisions. Such a development is inevitable, because 'as the institutions of knowledge lay claim to public resources, some public claim on these institutions' (Bell 1968: 238) and their results are unavoidable. Even more significant is that, as the importance of knowledge as a central societal resource increases, its social, economic and political consequences for social relations grow rapidly, together with demands to regulate the specific utilization and access to knowledge.

The dissemination and application of knowledge does not occur in the imaginary world of perfect competition and equality of opportunities. As a result, a politics of knowledge must confront the consequences of the social distribution of knowledge, especially the stratified access to and utilization of knowledge. It remains an open question, for example, to what extent dispossession of knowledge generates social conflicts and in what specific ways such struggles manifest themselves. Daniel Bell (1964: 49) warned several decades ago that right-wing extremism may 'benefit' from any exclusion of social groups from access to and acquisition of technical expertise.

However, such predictions about the intellectual, social and economic gaps sustained by knowledge overestimate the extent to which knowledge and its use can in fact be controlled. It will be increasingly difficult to control knowledge, in spite of the many efforts that will undoubtedly be made. Efforts to control knowledge encounter contradictions. Sustaining economic growth, for example, requires an expansion of knowledge. And knowledge that expands rapidly is difficult to control. The expansion of knowledge enlarges the segment of knowledge-based occupations. Knowledge expansion and knowledge dissemination rely on conditions that are themselves inimical to control. Nonetheless, as I have observed, the typically expressed fear that an inevitable outcome of such developments is the greater ease with which knowledge (and information) can be monopolized and effectively employed for repressive (even totalitarian) purposes, or even as a tool of maintaining the benign status quo, had been a widely accepted premise of discussion of the social control of knowledge even before Orwell's classic book on the subject. What exactly nourishes this point of view? What is the basis for the widespread conviction that knowledge and technical artifacts are relatively easy to control and that access to knowledge can be easily denied?

Knowledge Hierarchies and Monopolies

One of the ways to understand the various means by which knowledge is seen to be controlled, perhaps even monopolized, and its gains – following the Matthew principle – primarily allocated to the rich and powerful, is to examine the literature that has incessantly informed us that precisely such outcomes are built into the very logic of scientific and technological development. What exactly is it, in the view of these critics, that gives technology and scientific knowledge such potency and discriminatory power? And what kinds of *mundane encounters* with modern science and technology may have prompted or at least reinforced the critics' theoretical conceptions of science and technology? Typical encounters with science and technology in everyday life must have left their mark and strengthened otherwise rather abstract assessments of the technical artifacts and scientific knowledge. I will suggest that these essential and affirming encounters are experiences with 'frozen' or arrested technical artifacts and knowledge forms.

My aim is not an exegesis of the epistemological or theoretical ancestry of such views. I presuppose that the critique of modernity, insofar as it touches upon the rationality of science and technology, represents a form of civilizational critique that has accompanied the emergence of modern societies from the beginning. The critics of modern civilization flatly reject the claim that science and technology, as celebrated by its proponents, are socially and politically neutral. As Marcuse pointedly asserts: "Science, *by virtue of its own method* and concepts, has projected and promoted a universe in which the domination of nature has remained linked to the domination of man" (Marcuse [1964] 1989: 166). For illustrative purposes, I refer in some detail to two representative philosophical and sociological critiques of the interrelations between the social and intellectual fabric of society, knowledge and technology; namely, the analysis of modern science and technology by Herbert Marcuse and Helmut Schelsky.[16]

Marcuse's views of the role of modern science and technology gained considerable public resonance with the publication in 1964 of his *One-Dimensional Man*, subtitled 'Studies in the Ideology of Advanced Industrial Society'; but they can be traced back to his writings and those of both Adorno and Horkheimer in the early 1940s. Critical theory, in effect, abandons Marx for Weber on the issue of the emancipatory potential of modern reason. Marcuse observes at the time, 'National Socialism is a striking example of the ways in which a highly rationalized and mechanized economy with the utmost efficiency in production can operate in the interest of a totalitarian oppression and continued scarcity. The Third Reich is indeed a form of "technocracy" (Marcuse 1941: 414). In the case of National Socialism, politics is still a decisive force; yet technical knowledge is already seen as an indispensable instrument of political control.

A quarter of a century later, Marcuse assails the scientific mind and the transformation of knowledge into a form of scientific-technical rationality that has perverted the project of emancipation and has led to the human domination of nature. Marcuse (1964: 146) argues that such outcomes are inherent in science, that 'scientific-technical rationality and manipulation are welded together into new forms of social control' resulting in a kind of epistemic enslavement of modern individuals. Modern individuals become incapable of seeing and dealing with the world in any other manner, hence their entrapment.

The technical presumption of science becomes a political presumption and has consequences for human social organization because the transformation of nature, according to the logic of technology, also involves changes in the social relations of individuals. Whatever claims may be made on behalf of the essential political neutrality and potential of technology, Marcuse stresses emphatically, even against Marx, that a technology that has become the *universal* form of material production, "circumscribes an entire culture; it projects a historical totality – a 'world'" (Marcuse 1964: 154). In other words, the relation and respective implication of science and its technical application, and of the nature of the society that is thereby created, can in the final analysis only be viewed as an intimate connection that operates under the same logic. Technological reason and its universals, namely the discipline and control of production resulting in regimentation, the pursuit of narrow goals or specialization and the absolute uniformity of regimented and specialized labor or standardization, are bound to predominate throughout society.[17]

The same inherent force, the rationality of domination, soon propels the universes of scientific and ordinary discourse. All sectors of society, all social activities and all subjectivities are brought under the control of technical forms of discourse. The domination of nature and society go hand in hand. Science and society become reflections of the logic of technical rationality. Marcuse therefore concludes that the "scientific method which led to the ever-more-effective domination of nature thus came to provide the pure concepts as well as the instrumentalities for the ever-more-effective domination of man by man *through* the domination of nature … Today domination perpetuates and extends itself not only through technology but *as* technology, and the latter provides the great legitimation of the expanding political power, which absorbs all spheres of culture" (Marcuse 1964: 158).[18] The resulting lack of freedom and autonomy appears neither as irrational nor as the result of political forces but as a 'rational' submission to the technical necessities of existence. In the final instance, therefore, instrumental reason becomes ubiquitous and turns life in society into a 'totalitarian' existence. The sphere of the political becomes, as in Schelsky's scientific civilization, the sphere ('the incessant dynamic of technical progress has become permeated with political content' [Marcuse 1964: 159]) and rationality becomes irrationality. The state becomes merely an expression of the technical base and is depoliticized. Social change will be arrested for the most part, especially by virtue of the power and the primacy of the society's administrative apparatus, and this containment of social transformations is perhaps the most singular achievement of advanced industrial society.

Marcuse's analysis of scientific rationality is highly abstract and lacks congruence with social reality, especially with the ways in which and the extent to which many modern individuals experience spheres of autonomy and responsibility. He provides no examples of how technological means are turned into mere means of social control and domination; for example, how the telephone or television invariably become instruments of domination. The reluctance of dictators to promote a modern telephone system in the early part of this century would indicate that they feared its subversive possibilities. To this very day, differences in economic and demographic factors do not satisfactorily account for the large disparities in the dissemination of the telephone in state socialist and capitalist societies after the Second World War (cf. Buchner 1988). But even more to the point is Alain Touraine's observation that Marcuse's theory of modern society lacks reality congruence: "The image of a totally unified society, in which there is a perfect correspondence between technology, firms, the State, and the behavior of consumers and even citizens could not be further removed from observable reality" (Touraine [1992] 1995: 159).

Helmut Schelsky's thesis that advanced industrial society is evolving into 'scientific civilization' was first expounded by him in a lecture in 1961 entitled 'Humans in scientific civilization'. For Schelsky, *modern* technology represents not merely an adaptive capacity to the constraints of nature, but a reconstruction of nature by society, and therefore of society. In the context of modern technology, humans no longer confront nature with the assistance of organs aided, improved and developed in their capacity by technology, but on the basis of a 'detour' via the brain, or the application of theoretical knowledge in practical contexts. The outcome is that, using the language Schelsky employs, an 'artificial' nature as well as an 'artificial' change of humankind itself. The result therefore is a "re-construction and re-creation of man … in his corporal, psychological and social existence" (Schelsky [1961] 1965: 16). We produce, as Schelsky observes, "the scientific civilization not only as technology but necessarily also in a much broader sense continually as 'society' and as 'soul'" (Schelsky [1961] 1965: 17).

Modern technology changes the relations of humans to nature, to themselves and to others. The result of this dual transformation is the 'circulation of self-determined production' (Schelsky [1961] 1965: 16) representing the real foundation of scientific civilization. The self-regulated and self-propelled nature of this process, the constant production and reproduction, evolves into a self-steering process which does not appear to allow for any escape:

> Every technical problem and every technical solution invariably becomes also a social, a psychological issue because the self-propelled nature of this process, created by man, confronts humans as a social and psychological dictate which in turn requires nothing but a technical solution, a solution planned and executed by man since this is the nature of the condition to be tackled (Schelsky [1961] 1965: 16–17).

Modern technology constitutes a particular logic, and this logic necessarily becomes the dominant logic of human life. One of the significant consequences of such a conception of technology is that the traditional 'logic' of technology reverses itself. That is, technology as a producer of mere means of human action becomes a producer of ends or meaning, or in other words, 'means' of action determine its ends and prefigure the direction of social change. Schelsky describes technology as an intellectual process which dissects varied natural objects into their elementary parts in order to re-assemble them according to the principle of the least effort or maximum efficiency. The result of modern technological construction, therefore, is a novel product or process with *artificial* features and, in analogy, an *artificial* human being.

Schelsky's and Marcuse's theories evidently converge. They share the thesis that there is the distinct danger that technology in modern society will increasingly displace spontaneous social and political action and significantly reduce individual spheres of responsibility and autonomy, resulting, in the end, in the 'death of the self'.

Marcuse and Schelsky are by no means alone in their assessment of the trajectory of the social, political and economic development of advanced industrialized societies. Nor are they alone in attributing the societal changes they describe to intrinsic and enslaving 'laws' of science and technology. On the contrary, their observations and warnings resonate with a broad intellectual trend that actually began to take on its peculiar characteristic in the 1950s, when social theorists first noted distinctive and presumably irreversible trends in industry and production.[19] Social scientists asserted a tendency in industry toward increasing technological progress, manifesting itself in the rapid mechanization or *automation* of production. While the increased automation of production that is, as Marcuse (1964: 35) observes, inherent in technological progress itself enormously enlarges the output of commodities, it does not, as many observers then noted, make work more meaningful, demanding and challenging. The result is summed up by David Riesman and his collaborators in *The Lonely Crowd* (1950): Industry is now producing bored workers through simplified work routines, and the central meaning of life is increasingly shifting away from work toward a search for creative expressions in leisure activities.

Schelsky's and Marcuse's observations resonate with Bell's (1960) thesis about the end of ideology, as well as with the prognosis by Robert Lane (1966) that we are about to enter an age in which scientific knowledge increasingly dislodges the political element from politics. By the same token, the futurists Herman Kahn and B. Bruce-Briggs (1972: 8–29) in the early 1970s discern multi-trends within modern society that have been widely noticed by 'macro-historians', including the 'centralization and concentration of economic and political power' as well as 'innovative and manipulative social engineering'. The growing rationality that comes with the rapid accumulation of scientific and technical knowledge, according to Kahn and Bruce-Biggs, is increasingly applied to "social, political, cultural, and economic worlds" (Kahn/Bruce-Biggs 1972: 9). Although this trend may not accelerate, the desirability of social engineering is widely supported and an "almost universal belief among the educated" (Kahn/Bruce-Biggs 1972: 29).

The influence of ideological and, more generally, of political factors on scientific and technical developments remains unanalyzed, however. This suggests that the conventional central theoretical categories employed in the analysis of modern society, partly inherited by present-day social science from the past century, such as class or economy but also such notions as capitalist or socialist, have lost their crucial role in social theory. Observers were increasingly convinced that the distinction between capitalist and state socialist economic orders was becoming obsolete. At the same time, however, confidence in the power and the uniqueness of scientific knowledge remained strong. Raymond Aron ([1962] 1967: 42) embraced and highlighted these assumptions in his theory of 'progressive' industrial society. At the same time, questions about the motor of 'social change' or the centrality of the economic system for societal transformations were raised anew. It is at this time that theorists began to advance the thesis that technology and science, rather than the economy, are the real motor of societal change in modern social systems (cf. Parsons 1970: 619).

More generally, however, Schelsky's and Marcuse's accounts of the social and political force of modern science and technology suffer from an unintended but nonetheless misplaced confidence in the practical efficacy of scientific reasoning and quantification. Knowledge and technology are for the most part treated as a black box. The concern with technical artifacts is primarily functionalist. The major question posed concerns the psychological, social and political consequences of objects in the sphere of social relations. What exactly confers such power on objects is never examined. Marcuse and Schelsky presuppose an image of science and technology that then gives them reason to despair. One perceived consequence of technology and science, the extent to which the world of objects begins to dominate the world of subjects, paradoxically rests on an acceptance by both Marcuse and Schelsky of the positivist image of science as a most efficient, rational enterprise that produces highly useful devices and knowledge claims. As a result, we must return to our initial question: what nourishes such a view of science and technology, in spite of Marcuse's and Schelsky's otherwise deep misgivings about such a science and such efficient technical objects?

At this point, we must take cognizance of some kind of phenomenological analysis of everyday experience and common sense understanding of science, especially regarding technical matters, that are not further investigated by Marcuse and Schelsky, even though they serve as starting point and as affirmation of their observations. The primary experience in everyday contact with technology is the *finished* product. The everyday experience of technology is not rooted in an understanding of the conception and fabrication, in short: The decisions that constitute the nearly always invisible 'technical code' of a matter and that co-determine the ways of using such technologies in everyday contexts are not manifest to the user.

Feenberg has provided us with a fruitful explication of the concept of the technical code: The technical code refers to those attributes of an object that

> reflect the hegemonic values and beliefs that prevail in the design process. Such codes are usually invisible because, like culture itself, they appear self-evident. For example, tools and workplaces are designed for adult hands and heights not because workers are necessarily adults, but because our society expelled children from the work process at a certain point in history with design consequences we now take for granted. Technical codes also include the basic definition of many technical objects insofar as they become universal, culturally accepted features of daily life. The telephone, the automobile, the refrigerator, and a hundred other everyday devices have clear and unambiguous definitions in the dominant culture (Feenberg 1995: 4).

While the technical code of an object originates or is provided in the context of its production, it is thus not yet necessarily decided how ultimately to handle an object – in the context of its use – as if it were natural. For this, the 'cultural code' is a further requirement, since it contributes to the decision of which possibilities for use are connected with an object. Technical and cultural codes may overlap, but they can also diverge. Ultimately, the cultural code can also change. In any case, technical and cultural codes more or less definitively limit the imaginative possibilities for use, and have as a consequence the fact that everyday experiences with objects are primarily 'successful' experiences. And this counters the disappointments that naturally also continually occur, nonetheless probably basically confirming the confidence in the predetermined technical and cultural process of the object. The technical and cultural code endow the object with a specific process or even a purpose which will be fulfilled by it. The codes stabilize usage. Objects confer certainty. The degree of security that allows these coded processes to be reproduced again and again is then associated primarily with an image of reliability – although the goals that can be realized with this reliability can be of various different kinds. In any case, in the process an emotional connection with the object takes form. This certainty, security and reliability in principle in everyday dealings with technical objects at the same time induces, according to my thesis, a high degree of confidence in the efficiency of objects. The fact that connected with this efficiency there might at the same time be a feeling of helplessness or of the 'power of objects over us' is understandable. The limited technical and cultural code of an object, even if 'the radical constraints on possible integration of objects are in the interest of those integrations that serve to satisfy the needs of powerful individuals or groups' (Joerges [1979] 1996: 25) obstructs alternative possibilities for use and confirms one's helplessness in handling objects. This is, to be sure, nothing other than a reification of the dominant code.

A phenomenology of technology underlines some general observations by Alain Touraine about the actual role of technology in a society that is increasingly based on technology:

> We live in a society in which means were completely divorced from ends. Far from determining or absorbing ends, the same means could therefore be used for both good and evil ends, for both reducing inequality and exterminating minorities. The increasingly dense networks of technologies and signs in which we now live, and which orient and govern the ways in which we behave, by no means imprison us in a technological world and by no means destroy social actors. They impose neither a logic of efficacy and production nor a logic of control and reproduction. The image of technocracy triumphant is pathetically inadequate if we contrast it with the increase in consumption, the rise of nationalisms and the might of transnational companies (Touraine [1992] 1995: 148–149).

Prospects

In my view, efforts to police knowledge and to defend society against some of the anticipated but also uncertain effects of the utilization of recent gains in knowledge ultimately will do little to seriously limit its application, in one way or the other. But this will not keep various societal agents from trying.

One of the most immediate and controversial questions that awaits regulation and resolution as the result of evolving knowledge about the susceptibility to certain health risks in relation to specific genes is the question of how insurance companies (and other organizations and institutions), in particular health insurance companies, will use such information.

Private *health* insurance companies in Germany have announced (*Frankfurter Allgemeine Zeitung*, July 21, 2000: 17) that they plan to continue to use established procedures when it comes to a determination of calculating the risks individual applicants represent (also Murray 2000: 242–245; *Task Force on Genetic Information and Health Insurance*, 1993). That is, full disclosure of all relevant information is required. The applicant is under no obligation to disclose information she/he does not happen to have. A genome analysis will not, the insurer's indicate, become a prerequisite in issuing a policy. However, individuals who happen to such information, for example, as the result of taking part in a research study, are expected to divulge the genetic information.

But how is one to insure that insurance companies limit their usage of such information voluntarily? What exactly is genetic information? How broad or narrow can or should one define genetic information? And, how does one treat the interaction between genetic and non-genetic 'causes'? How does one attribute responsibility? Can an insurer acquire genetic information indirectly, for example, on the basis of a family history? Are special legal norms required? Genetic tests are bound to become more common, more accessible, and less and less expensive. Policing knowledge looks like work that Sisyphus might know.

Notes

1 By the same token, a report issued by the Rand Corporation (Fukuyama / Wagner 2000: 1) anticipates that in the early part "of the 21st century, the technologies emerging from the information and biotechnology revolutions will present unprecedented governance challenges to national and international political systems." The report deals with the governance of both research and knowledge policies.

2 The discussion and formulation of the novel moral principle for a "right to ignorance" by Hans Jonas (1974: 161–163) is germane in the context of this discussion.

3 The new political field I identify as 'knowledge politics' is, certainly, not immediately connected with the often-described ambivalent sense of crisis in modern societies, based on the over- and / or mass production of knowledge. The tension between the extent of knowledge production in advanced societies and the limited capability of the individual person to assimilate the huge amount of knowledge available, was already described by Georg Simmel ([1907] 1978) a hundred years ago in a theory of the current age in the final chapter of his *Philosophy of Money*. The tragedy of culture manifests itself in the cleavage between objective culture made independent and the obstinacy of subjective culture. The problem of the policing of knowledge is not related to the production of knowledge in total – even if it is related to overproduction, however that may be defined – but rather to the range of incremental knowledge, which is conceived as being capable of changing reality.

4 Dorothy Nelkin (1995: 447–456) has published an informative typological summary of the public controversies in which science has found itself embroiled in the United States in the past.

5 Steve Fuller (1993: 377) advances a similar assertion, as far as I can see. He indicates that 'in the world of tomorrow, breakthroughs in the natural sciences are regarded as triumphs of applied sociology and political economy, rather than of, say theoretical physics, chemistry, or biology'. It is better understood and presumed that the implementation of a specific knowledge claim can alter the social fabric of society and the anticipated transformation is no longer seen as mainly beneficial.

6 Cf. 'Kansas Votes to Delete Evolution from State's Science Curriculum', *New York Times*, National, August 12, 1999.

7 The regulation or the stratification of access to knowledge is nonetheless a constitutive component of everyday life. The world of adults, for example, is differentiated from that of children.These stratified worlds go hand in hand with the ability to impede or even to obstruct children's access to certain forms of knowledge. The quotidian forms of regulating access to knowledge are not under discussion here.

8 I am grateful to Günther Küppers for this observation.

9 Whether the public willingness to support the field of knowledge politics will intensify in connection with what some scientists have defined as a 'comprehension gap' among the population, or whether this willingness will have any significance at all, remains to be seen. In a lead article, the English Sunday paper *The Observer* (21 February 1999, p. 28) describes the perceived wide comprehension gap as follows: 'Between the scientific upper class, the latter-day Leonardos trekking into the brain or sketching the universe, and the majority of voters and politicians in all Western democracies, there is now a deep comprehension gap'. This deficit in comprehension, however, should not be underestimated in the sciences themselves either, given the growing division of labour among the disciplines.

10 A shift toward concerns with the externalities of science does not mean that contested efforts to regulate the conduct of 'scientific inquiry' (cf. Wulff 1979) and, for that matter, attempts to manage or plan scientific research (e.g., van den Daele/Krohn/Weingart 1979) will disappear. On the contrary, issues of ethics, accountability, and conflict, as they relate to the genesis and execution of inquiry, will of course remain highly significant. At the same time, discussions about the conduct of inquiry will be affected by anticipated outcomes of research.

11 My use of the concept of 'regulation' resonates with the way in which Steinmetz (1993) deploys the term to analyze the regulation of the emergence of the welfare state in Imperial Germany. This concept takes its distance from the economic literature on regulating the practices of capital accumulation (e.g., Jessop 1990) because that approach tends to rely on an overdetermined image of the ultimate efficacy of regulation practices.

12 The enlargement of the scientific community into an international or even global community is becoming a focus of reflection and research in science studies (e. g., Schott 1988; 1993).

13 Assessing the impact of the interventions by uncredentialed participants in biomedical research and in AIDS care, Epstein (1996: 346) concludes that 'the impact of the AIDS movement on biomedical institutions in the United States has been impressive and conspicuous [and] it has rapidly become something of a cliché to say that the doctor-patient relationship will never be the same in the wake of AIDS'.

14 As late as in the 1970s, confidence in the capacity of 'disinterested' scientists to resolve public issues in the area of space exploration, nuclear power or food additive regulation, etc., was still considerable and significantly exceeded confidence in other groups or agencies (cf. Miller 1983: 90–93; Jasanoff 1990: 12). The general decline in the last two or three decades among the public of developed societies of the trust in science and technology as a problem-solver, a trust that had hitherto been a core element of modernity, has been documented by Inglehart (1995: 391).

15 Chandra Mukerji (1989: 197) describes the trade-off: 'What reassures scientists the most when they face the power of the voice of science and their powerlessness to use the voice in the public arena is the idea of their autonomy. Scientists are not, in the end, politicians, and they suffer political defeats better than the loss of face among their peers. As long as they can conduct research with which they can advance science [both science itself and their positions in it], they can feel potent. But the cost is that scientists cultivate an expertise that empowers someone else'.

16 A more extensive description and analysis of both Schelsky's and Marcuse's critiques of the excessive power of modern science and technology in society may be found in Stehr 1994: 203–221.

17 The decisive outcome of these developments is that the workers are incapable of acquiring a critical view of the repressive social order. The 'masterly enslavement' is pervasive throughout society, affecting all individuals at all levels of production.

18 Theodor W. Adorno's ([1966] 1973: 320) image of the extension of the rule of nature to a rule over man by man is similar. Adorno warns that the "unity of the control over nature, progressing to

over man and finally to that over men's inner nature" is one of the enormous dangers of the present age.

19 The genealogy of Schelsky's and Marcuse's fears about the impact of modern science and technology is of course much longer. I will refer to Max Weber but could list many more observers who have expressed concerns about the fateful consequences of science and technology in the age of modernity. Marcuse's and Schelsky's diagnoses resonate closely with Max Weber's analysis of the modern age as a demystification of the world resulting from the growing rationalization of social relations through science and technology. Weber emphasizes the painful tension between rational, empirical knowledge and meaning systems found in the life-world. Moreover, Weber's intellectual 'grandchildren' often share an 'Exodus impulse', namely the attempt 'to explode the fatalistically closed "steel-hard casing" of the demystified world' (Bolz 1989: 7). Schelsky and Marcuse therefore also make use, although for the most part implicitly, of a long established radical as well as conservative (romantic) intellectual tradition that launched a highly critical and skeptical analysis of the impact of technology and science on culture and social relations.

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Indices

Subjects

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