Science Studies
Probing the Dynamics of Scientific Knowledge

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"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 rethink 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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