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JOY IN REPETITION MAKES THE FUTURE DISAPPEAR

A Critical Assessment of the Present State of STS

THE CURRENT JOYS OF STS

Every scientific field has to set up its own notions of progress, even though these may turn out to be time-dependent.¹ Although it might be argued that STS as a field is still too young for a comparison of its initial goals and visions with its actual achievements, such an argument ignores the pressure on any new field to build up an intellectual, disciplinary and institutional identity.² There are various ways of constructing such an identity: breaking with a tradition, and hence opposing it; inventing a past that serves to provide normative reference points for present directions; or continuously readjusting history to legitimize present and future courses of action. It is our contention that STS as a field has largely failed to set up criteria for its progress. Its development has proceeded for the most part in an unplanned manner and without a real center. The intellectual and social organization of STS was built around single books (and their authors) rather than with an attempt to establish a tradition in textbooks.³ The vagaries of the institutionalization of STS, although different in Europe and in the United States, have undoubtedly contributed to a process whose outcome resembles what Richard Whitley has called fragmented adhocracies (which occasionally shade into polycentric oligarchy). The dominant feature of these sciences is their intellectual variety and fluidity. They lack a stable configuration of specialized tasks or problem areas, nor do they have strong co-coordinating mechanisms which systematically interrelate results and strategies. The underlying matrix is a combination of high task uncertainty with a low degree of mutual dependence (Whitley 2000).

As long as STS was producing new literature clearly distinct from the old in sufficient quantities, this alone sufficed to mark progress, and no additional criteria had to be supplied. Due to circumstances mentioned also by Richard Whitley in his foreword (contained in this volume), the field began to flourish with a multitude of strands developing side by side. Efforts to build a shared identity with recourse to a few founding fathers (and no mothers, as usual) remained either isolated or were simply ignored. There was a widespread but illusory assumption that STS could imitate what was believed to be one of the ingredients of success in the sciences, namely the inherent opportunism attributed to natural scientists. What was overlooked was that for opportunism to work, the right opportunities have to be present. STS could also — and, un-

fortunately, rightly so — claim that questions and criticisms raised from outside the field were either based on blissful ignorance or willful misrepresentation, as evidenced in the recent science wars. Collective and individual responses which arose in defense of the correct core of STS literature and its theoretical and empirical foundations hardly went beyond the *status quo* to question the direction in which the field was moving. Under these circumstances, no vision of the future is needed, since the present delivers all that is seemingly wanted: more of the same.

The history of STS enabled it to conceive itself as a modern Sisyphus. One of the implicit assumptions of its early programme was that the prevailing understanding of what science was and how it worked was deeply flawed, and that it was the calling of STS to correct these distortions. Deconstruction and de-mystification were means of achieving this. But this task did not come to an end, since additional misunderstandings and distorted views arose inevitably from different quarters. No longer were the old philosophers of science the main culprits. They simply ceased to be interlocutors. The epistemological questions they had raised no longer were intellectual issues over which academic battles could be fought. Once the philosophers were no longer the main contenders, another spate of misunderstandings emerged, many now internal to the field itself. In the general eagerness to eradicate heresies a number of vociferous campaigns in journals and in conferences were fought in the name of STS. Reflexivists were pitted against empiricists, the social became opposed to the cultural, SSK confronted ethnomethodology and ANT had to stand up to social constructivism.⁴ While these lively debates were initially conducive to an atmosphere of intellectual dynamism, over time their argumentation either tended to exhaust themselves or led to a narrowing of focus. The debates essentially became a self-serving matter for insiders. Meanwhile, in numerous empirical case studies, most of them situated on the micro-level, a fascination, if not an infatuation, with the artifacts under study set in. The style of presentation began to resemble in its performativity the practice of show and tell, a characteristic of the U.S. school system, but also of certain media styles (Wagner 1996).

There remained, however, two powerful sources that provided a constant flow of distorted or misunderstood notions about science for STS to correct. One was latent and difficult to detect, the other manifest and seemingly easy to deal with. The latent but immensely powerful source of false images, self-interpretations and public representations was science itself. This source remained hidden for a long time, since its only public manifestation was found in the work of philosophers acting as the self-appointed spokespersons of the scientists. The moment of revelation came when the different versions of science exploded onto the public scene with Sokal's hoax. The virulence, utter hostility and aggression that followed took STS by surprise, even if it can be explained in retrospect.⁵ What was at stake now became a political issue, namely who is allowed to speak in the name of science: STS or the scientists?⁶ The other, more manifest source of public images, representations and the proper understanding of science in need of continuous adjustment and correction is society itself. As shown, for instance, by Pestre in a programmatic article in *Annales*, society has a false understanding of science, and STS, together with the historians of science, is here to correct it (Pestre 1995). This is the reason why STS has carved out for itself an honourable role as authentic representative and spokesperson for a society whose images and understanding of science were either wrong or too powerless to make its voice heard.

Hence also the celebration of laypersons, whose knowledge and understanding are alleged at times to be superior to those of the scientific experts.

The convenience of having chosen the role of Sisyphus is not to be underestimated. His fate is that of muted rebellion, of having assumed the literally unending and unfinished task of responding in a repetitive and unchanging way to circumstances not of his making which continue to re-generate themselves. Sisyphus does not have to ask many questions about himself and his destiny, since all is given. *Joy in repetition*⁷ makes the future disappear and therefore renders it harmless. An unwanted side-effect not publicized by the gods, who wanted to punish him, is the intellectual blindness that befell Sisyphus. It never occurred to him that he himself might question what he did and how he got there, questions that could lead him to alter his agenda.

STS likewise has never established its own research agenda of unresolved problems. Yet, if it does not want to become trapped in an unending present of joyful repetition, albeit in the guise of rebellion, it will have to create a future that is neither Sisyphus-like, and therefore closed, nor utopian or dystopian, as so many future expectations of other actors in society are when assessing the benefits and burdens of science and technology. In a caricature of its own theoretical stance that scientific problem choice is not the outcome of an inherently rational selection process within science, nor that Nature whispers into the ears of scientists which problems to choose, STS hardly ever showed interest in the functioning of its own processes of problem selection or which issues, originating in wider societal developments, it did take up or not. Nor did it delve into the processes through which contrasting theoretical positions within STS are negotiated or eliminated. What, if anything, did Society whisper into STS ears? If problem choice within STS is at least partly seen to be the outcome of larger societal processes, then a vision of the future would be needed which takes into account the processes of its own professionalisation and its likely effects. This implies the capability to select problems for their own — scientific and intellectual — sake and to transform them into research priorities which are seen as a collective, and not simply as an individual task. It also implies the willingness and capability to transmit the relevance of scientific activities to a wider audience and perhaps even to one's clients.

We perceive STS to have reached a critical point in its development. The change is one from exploring new intellectual ground and joyfully inventing stimulating concepts and novel theoretical directions in blissful ignorance of where it might lead, to having to confront its own possible futures. But even if the road towards professionalisation and consolidation is chosen, it is still full of pitfalls, since different interpretations are possible. One is a more narrow, academic interpretation, while the other takes into account the larger set of societal, economic and political forces that impinge on every academic endeavour. Our task in this paper will be to critically examine the state of the art and, with occasionally polemical remarks, look for new future directions.

We proceed as follows: In the first chapter we analyse the historiography of STS. We claim that the historiography of STS is still not STS-like and we will outline what a more STS-like approach to the historiography of STS would have to take into account. In the second chapter we reconsider the link between STS and its subject of inquiry. We will argue that STS has never attempted to define its subject matter consistently. We attempt to offer such a definition by moving away from the terms Science and Technology replacing them with the terms of the production of new knowledge and things

as the defining focus of STS. The next two chapters start with the observation that STS on its way towards becoming a real discipline has lost some of its previous links to the other social sciences. We will inquire how STS could strengthen again its grounding in social theory. We do this, first, by showing what STS has to offer to social theory and how it could lead in reformulating social theory based on the assumptions of STS. Second, we will present some illustrative examples from social theories which are neglected inside STS, but could be very fruitful if taken up by STS. In the last chapter we present a polemical outlook on a utopia (some might say: a dystopia) of STS re-structured along our theoretical assumptions and developed organisationally towards becoming a profession.

Before developing these arguments further a precautionary remark is in order. What is termed STS in the following pages is necessarily an abstraction which can never encompass the entire, very broad and richly textured field of science and technology studies as it exists today. When we speak of STS, it stands for what we perceive to be the current mainstream of a scientific field in the sense used by Pierre Bourdieu. Mainstream therefore stands for structuring positions in a structured field. Mainstream is most visible and shown and told in a performative mood. When making this abstraction we follow the logic of the field and its specificity in accumulating scientific capital. We try to identify the strategies through which strands of knowledge are turned into scientific capital, who owns it and which kinds of strategies, methods and rhetorics are used in order to increase its accumulation. The diversity of the field STS may be an indicator of a relatively low degree of hierarchization inside the field, because it is relatively easy (and occasionally very useful) to accumulate scientific capital in neighbouring fields and import it. Nevertheless this does not mean that a hierarchy of themes, methods, theories and strategies does not exist inside the field. They form part of the tacit assumptions of all actors in the field. Although it is impossible here to attempt an even brief empirical study of the scientific field of STS we firmly base our analysis on the assumption that STS, just as any other scientific field, must be conceptualised in a sociological way to understand its functioning. To speak with Bourdieu, such an analysis of one's own field can even form a prerequisite for its functioning:

The particular difficulty which sociology has in conceiving science scientifically is related to the fact that sociology is situated at the very bottom of the social hierarchy of the sciences. Whether it rises to conceive other more scientific sciences better than they conceive themselves, or descends to record the triumphant image produced and propagated by scientific hagiography, sociology always encounters the same difficulty in conceiving itself, i.e. conceiving its own position in the social hierarchy of the sciences (Bourdieu 2000, 44).

And we should add: STS encounters the same difficulties as does sociology in conceiving the positions inside its own field. Such a conception of mainstream (a notion that is needed for the claim that there are heretics) obviously ignores important differences among national, disciplinary and political strands as well as a-synchronic developments occurring in different parts of the world. There also exists a built-in tension between what we perceive to be the mainstream and this Yearbook which needs to be explicitly acknowledged. The Yearbook, as described by Whitley (this volume), was established under historical circumstances, partly in confrontation with the mainstream of the time. It is certainly too simple to claim that one mainstream has been replaced by another and that the Yearbook's role has been, and continues to be, a kind of monitoring and steering

against the tide. Hence, when we plead for a critical re-examination of what the term science in science studies stands for and urge a re-thinking of the role of social theory for the future development of STS, we do not suggest that others have not also raised similar criticisms or opened up new directions. And yet, the tension between what we call the mainstream and what the contributions in this volume represent, is real and needs to be spelled out.

A further remark is necessary here. Our arguments against the mainstream require to clarify where we stand in relation to it and therefore to position ourselves among the meanderings of other streams that, together with the mainstream, make up the dynamics of the field. We do not consider ourselves to be outsiders of STS. On the contrary, we see ourselves as partaking in the successes and failures of STS upon which the empirical content of the subsequent analysis rests. One of the authors (Helga Nowotny) shares with the other first generation members of the Editorial Board of the Yearbook a common background. In fact, she had been a student of Robert K. Merton in her Columbia University days and, like others, found it necessary to depart from what Mertonian sociology of science had come to embody. The common intellectual interests that brought together the members of the editorial board have been recorded elsewhere in this volume. Her biography led her back to countries in Europe in which it would take decades (and sufficient self-reliance in her own institution-building efforts) to set up an institutionalized basis in which STS could take root. Both Austria and Switzerland share the situation of somewhat peripheral countries that, due to size and geography as well as due to the (different) legacy of their national histories, have remained outside the forces that have shaped the social sciences in countries like France, Germany, the UK and USA. This creates a tension between being a member of an international scientific community in which scientific reputations and scientific careers are made and a career in a more narrow sense as belonging to a quite different, and deviant, local community. Helga Nowotny's visibility and career were built on continuously interweaving opposites, combining also science with science policy analysis and advice. In the end came the (successful) attempt to institutionalize STS in places where it had not existed before. Having first had to build up an infrastructure confers perhaps immunity against hyper-reflexivity and certainly induces impatience with any move to give up too fast what had taken long to acquire.

Michael Guggenheim, the other author, found himself to be a student of the local *Volkskunde* at the University of Zurich, an academic sub-speciality of cultural anthropology, which exists only in German-speaking countries. His intellectual curiosity and other, more contingent, factors led him to discover science studies while still a student. What he found was an already prefigured and well-established field, which had its mandarins, its great books and exemplary models of how to conduct research on topics, which were also given. Like others in his generation, he had no choice but to try to identify gaps in research and to articulate theoretical lacunae that would allow to position himself at a time where the field was already structured with no revolution in sight. STS presented itself to him as any other academic discipline — with the difference that, unfortunately, it could not yet be studied in Switzerland. The local vacuum was in stark contrast to international developments, but it was easily filled by the passing through Zurich of the jet-set of STS luminaries. In any case, STS appeared to him much more coherent than the local variations of either sociology or *Volkskunde*. Compared to

their inferiority complex, he could not understand what the proclaimed identity problems of STS should be all about. This highly localized and somewhat idiosyncratic view of STS offered a habitus and a strong sense of identity. It could even be turned into a glamorous view from somewhere, where the view is best kind of attitude (remember that Switzerland is located in the Alps and part of the Swiss unconscious is to strive for places where the view is best). The authors joined efforts, curious about the convergence of their ideas and outlooks despite — or because — their different departure points and in search of establishing an intergenerational dialogue. But the present contribution can also be read as a prosaic attempt to pass on scientific capital from the older to the younger author.

HOW TO GET THERE: THE MISSING HISTORY OF STS

The missing future of STS is paralleled by its missing past. This does not mean that there are not many stories in circulation claiming to constitute its history. Rather, and curiously enough, these stories resemble the kind of history being told by any other discipline, or found in other introductory textbooks. It consists of standardised *topoi*, many of which are transferred uncritically from the founding generation to the next, without attempt to analyze their making. Where historians of science normally tread very carefully in unveiling the constructions of scientists as active participants in establishing their history, and scrupulously attempt to spell out the alternating functions served by historiography, there appears to be a collective blind spot within STS. It is perhaps indicative in this respect to note that the history of STS, as written by David Edge and published in the *Handbook of Science and Technology Studies*, is rendered as a first person account.⁸ It begins with Edge, as the lonely hero, arriving at Waverley Station in Edinburgh, on the night train from London, to start the Science Studies Unit at Edinburgh University. His office was empty, ...no phone, no books, no bibliographical resources, no files, no staff — indeed, it was tempting to think *no subject!* (Edge 1995, 2). Although this narrative of the history of STS is very thoughtful, its beginning is more reminiscent of the account of an explorer or an ethnographer in the classic era of colonialism than of a historian or sociologist of science. The reader, in this case the uninitiated younger generation of STS scholars, are invited to take part in the adventures of a founding father and to delight in the times when STS was still more of a pioneering venture than a discipline. STS in those days appears to have resembled finding Colonel Kurtz in the jungle: nothing, not even a subject. Now we have phones, books, bibliographical resources, files, staff and much more, such as computers and search engines. We even have a subject or — to be honest — some hundreds of subjects, programmes, theories. We have material cultures and experimental cultures, bodies and artefacts, factishes, actants and much more — but still not the history we deserve.

In this respect, STS is becoming like any other discipline, too normal, perhaps. The standard history of scientific disciplines is usually a history of progress, of emancipation. Mertonian sociology of science, the older sociology of knowledge and analytic philosophy of science were, with different degrees of emphasis, all caught in the unwarranted belief that the substantive content of science — scientific knowledge — was impenetrable, if not taboo. It was considered either too difficult to pry open this forbidden

zone, or the zone was thought immune to study by sociological eyes, since it contained the heart of Western science, namely rationality. Enter the courageous STS tribe!

Having acquired telephones, hired some staff, read some books and found a subject, they now fearlessly enter the laboratories whose threshold no social scientist had ever dared to cross before. They tunnel their way into historical archives and, in alliance with historians of science, who quickly become infected by their enthusiasm, search there for hard data, which turn out to exist in abundance. They reveal that science is not what it was thought to be, nor what it publicly claimed to be. It is a conglomerate of (cultural) practices, just like other fields of activities or other kinds of work.⁹

This is not intended to be a lament about the state of historical consciousness, or rather the lack of it, in STS. But the parallels with any other disciplinary history or with accounts of the pre-STs history of science are nevertheless striking. Irrational belief is overturned by real facts, brought to light by the researcher, who replaces mere speculation with data, texts and figures. One is reminded of the anecdote of the three soccer referees who explain to each other how they know when the rules have been broken. I whistle because a foul has been committed, says the first. I whistle because I have observed that a foul has been committed, says the second. For the third it is also clear. When I whistle, the foul occurs, he says. It may well be argued that a discipline, even if it takes pride in having discovered that fouls occur when referees whistle, also has to resort to the kind of history which shows the progress from a naive realism to a sophisticated constructivism. It may well be necessary for STS, too, to prove itself capable of providing students with a sense of disciplinary identity and of continuity over time linking them to their founding ancestors. STS also may be in need of its distinct founding myths as a form of social bonding. But reflexivity entered the social sciences precisely by critically examining how social functions mingle with substantive content, and it spoke out against naive dichotomies that tried to separate the two. It had argued in other cases that such unexamined constructions of history may lead to fateful misconceptions, uncritically favouring the actions of heroic individuals and promoting a notion of progress as inherent in the field. But in the end, STS turned a blind eye to the risk of a far too optimistic image of a discipline, the result of ignoring how changes in the broader institutional and political environment affect its own historical development.

We propose therefore an assessment of STS in terms of its own standards and criteria. This means that a socio-cultural history of STS must be written to take into account questions like the following: what is the connection between the development and institutionalization of STS and broader economic and political developments?¹⁰ How did the specific cultural and geographic patterns of STS and its diversity in orientation and research style develop as they did? Why are there relatively many women in STS as compared to, say, economics, history, or even sociology and anthropology? What were the factors leading to the establishment of STS units in the universities of some countries but not in others? What were the relations between STS and science policy and what has changed, if anything, with the growing importance of EU research policies? Why did the political strand inside STS undergo a relative decline? Such a history, were it to be written, would probably show how STS people fought for institutionalization inside their university and how those who succeeded were also fortunate enough to have used university reforms or other academic restructuring to their advantage. It would probably show how STS people have entered specific trading-zones with other academic tribes

to exchange concepts and methods, and how they have fared when encountering the numerically superior cultural studies tribe. It would tell how STS, despite its initial claims of overcoming the restrictions of Western science, remained caught in its hegemonic and patriarchal tradition. It would show how the feminist focus within STS, despite its considerable breadth and depth in conceptual and empirical studies, has failed to lead STS as a whole towards collective action against widespread gendered practices. It would show how Bruno Latour, as a wild capitalist of his own making, was to further his career as an entrepreneur in STS, mirroring the development of his rich oeuvre.¹¹ We could continue with more detailed chapters of this imaginary future history of STS, but need to content ourselves here with one further speculative hint arising from the criticism voiced above.

If we return to the history of STS as told by David Edge, an important but today often neglected strand of its origins is its use of the Marxist critique of science and its links with radical science and other protest movements. STS in this view grew at least partly out of the broad stream of other political and social movements of the late sixties and early seventies, as did feminism and the environmental movement.¹² Such a co-evolutionary development between social movements and their subsequent academic integration or appropriation is nothing new in itself. The constitution and establishment of the social sciences in parts of Europe in the late 19th century also grew out of a strong reform movement and for a while remained intellectually linked to its political agenda (Nowotny 1990). In contrast to social movements, however, whose *raison d'être* does not cease to exist as long as the grievances and conditions persist against which its protest is directed, the translation of social problems into scientific problems has to follow a different kind of logic. Science is under the obligation to bring forth continuous novelty, be it the discovery of novel phenomena or novel explanations or be it the novel recombination of older problem formulation. STS, therefore, no more than any other scientific field, cannot retain the same politically or socially inspired research agenda over time.

Given the origin of STS at least in part in the protest movements of the seventies, it is interesting to compare the subsequent development of STS with that of the environmental movement. In our view the environmental movement succeeded in transforming its initial moral protest into the refined structures of academic environmental studies programmes. These turned out a new generation of environmental *professionals* who found work and employment in NGOs with their world-wide networks, in the offices of national and international law-makers, in political parties and their think-tanks as well as by setting up their own small but thriving consultancies.¹³ Each of these heterogeneous sites of knowledge production contributes in turn towards further enhancing the dynamics of interaction between itself and the generation of new knowledge in other sites. In contrast, STS has succeeded merely in establishing its own academic base, which remains unsatisfactory and deficient in some countries and vulnerable to the calculatory whims of the new academic entrepreneurs in deregulated universities in others. STS has gradually lost contact to the social movements it was originally affiliated with, without building up other sorts of ties.

The point here is not to describe what happened as a form of depoliticisation or to lament the loss of moral outrage. Moral outrage needs to transform itself into another kind of sustainable programme that fits into (and may even subvert) already existing

structures, be they scientific, political or juridical.¹⁴ In contrast to the environmental movement, STS has simply failed to establish its own professional structures that would have linked its academic institutionalization to the wider political, economic and cultural world and the issues that dominate there. Was it, we might ask, because the habitus of STS people has become too divergent from that prevalent in social movements, making it difficult to find a pragmatic common ground for common work?¹⁵ Or is there something inside STS's mode of knowledge production which focuses its intellectual attention and analytic strength on the production of texts? Whereas each environmental studies programme is still grappling with some real issues of environmental change and degradation in addition to a host of other political, institutional, legal and cultural problems, the political impetus within STS seems to have become confined to professing its good intentions.¹⁶ As with cultural studies, whose origin, at least in the U.S., also lies in part in a wide-spread mood of unease and political protest against the white-male canon, STS followed the move and retreated into its academic niche.

It seems that we will have to reinvent the wheel, to use David Edge's phrase. He argues that the original aims of the STS pioneers [sic!] are still very much alive and relevant but that, for all the progress of the past quarter century the insights we have gained still seem, too often, to be almost wantonly disregarded. The potential audiences for our messages are stubbornly unreceptive (Edge 1995, 4). Edge sadly sees others, mainly outside the STS community, finding anew the old sense of practical urgency and unease at the social status and role of science and technology. In attempting to end this unease, they then reinvent the wheel and, in so doing, largely ignore STS's achievements. But reinventing the wheel turns out to be a tricky business, whoever the reinventors may be. Why the wheel, one might ask, and why blame the potential audiences for not having heard the message? And, while we share David Edge's amazement at the puzzling tenacity with which scientists still cling to the old, positivistic image of science as an abstract, timeless search for irrefutable truths, we cannot help wondering about STS's irresponsibility in having failed to see the interdependencies between the authority ascribed to Nature and the social authority acquired by those who act in its name. Was it only when society started to speak back to science that STS was forcefully reminded of its own analytical capabilities, which were far superior to those of the various Public Understanding of Science campaigns? Will it reinvent the wheel or is it ready to move towards the invention of a more advanced technology?

WHAT IS THE SCIENCE IN SCIENCE STUDIES?

The word science in science studies still seems largely to mean physics, although the weight is shifting in the direction of the life sciences. This statement does not imply that other fields and subjects are not studied, but that whatever is within the scope of STS is still held against some kind of standard image of science which is definitely not geology or psychology. We claim that science studies in their actual practices carry a hidden assumption of what science is. Our intention here is to highlight this assumption and to question it.

Despite the vast and continuously expanding range of topics, objects and fields of study of STS, a certain hierarchy among them persists. First and foremost, the science in science studies refers to the natural sciences. While this may be self-evident in

English, it is far less clear in German where STS, for instance, is called *Wissenschaftsforschung* and where the term *Wissenschaft* is known to comprise the social sciences and humanities as well. Among the natural sciences the most rewarding fields of study for STS are those having a long, established tradition and those that are considered to be hard. This confers prestige and renders attractive those scientific activities involving scientific instruments, experimental set-ups and visualising practices, or which at least focus on something tangible, like graphic representations capable of being shown and told. Scientific work carried out mainly in symbols or conducted in thinking and writing is only marginally represented in science studies.¹⁷ While this might partly be due to methodological considerations and relative ease of access, it is also the fascination with things that accounts for their privileged disposition in becoming favourite STS objects of study. The inclusion of the social sciences or of law has hardly begun to attract STS attention.¹⁸ It seems that STS has never been really interested in what it, as a social science, is most familiar with, thereby unwittingly reproducing the history of anthropology: one starts with the most exotic societies and their strangest customs before eventually being allowed to return home. The problem is that this exoticism veils the reason for an object's having been chosen for study in the first place. It eclipses the criteria according to which research objects are selected, because a choice based upon their unfamiliarity and strangeness obliterates the necessity to come up with theoretically based arguments.¹⁹

Strangeness, unfamiliarity or distance as criteria for choosing one's field of study are problematic terms, however. If the hierarchy of the sciences and the differences in reputation which result from dealing with the more or the less prestigious sciences were openly acknowledged, the reasons for the actual choices made would be clearer. The STS community consists of those who have a background in the natural sciences and those who come from a variety of social sciences or from the humanities. For the latter, the strangeness criterion applies more than to those whose disciplinary origins lie in the natural sciences, but there is also more prestige for them to gain from turning to the natural sciences. This is also the reason why the social sciences never attracted the attention of the former natural scientists. There is also a certain gap remaining between what is considered science in the sense of original research and what is considered further downstream in the sense of applied knowledge. Engineering and medicine are seen as being somewhat different and peripheral to science. This is partly so for historical reasons, because these disciplines were already thought to be open to social influences long before STS came into existence. Research on engineering and medicine continues to engage research communities not identical with STS, who gather in conferences of their own and publish in different journals.²⁰ Last but not least, science — with notable exceptions — refers to science in Western countries and STS accordingly seems to perceive itself as producing knowledge of relevance only to the Western world.²¹

Whatever the origins and explanations of these persistent hierarchies and how they differ in detail, they go almost unnoticed within STS and remain uncontested. Nor is it easy to locate attempts to delineate the field of STS and to analyze how particular boundaries became established. Explicit definitions of what science is are extremely rare. For example, David Hess, in his introductory book *Science Studies*, is very careful to delineate disciplinary boundaries *inside* STS but remains silent on what constitutes science (Hess 1997). Dominique Pestre argues that the history of science (for him co-

terminous with STS) should not attempt to define what science is, since it should only take as science the notions that were current in certain historical circumstances (Pestre 1995, 494). What might be the reasons for such self-imposed abstinence?

Mario Biagoli provides an answer in the introduction to his *Science Studies Reader* (Biagoli 2000). He argues that science studies, unlike other academic fields, do not have to define [their] subject matter in relation to neighboring disciplines; over the years, the scientists have done much of that work (Biagoli 2000, XI). He develops his argument with two comparisons. First, he compares STS to classical ethnography. As does ethnography, STS identifies tribes inhabiting a circumscribed territory as its chosen objects of study. But unlike ethnography, STS never was a colonizing force; rather, it was colonized by its subject. The second comparison is with literary studies. Biagoli maintains that the object of literary studies, i.e. literature, retains a passive role and that therefore literary studies must always debate their proper object — the literary canon — anew. STS, again unlike literary studies, does not face such a problem, since the scientists themselves delineate what counts as science. What remains for the student of science studies is therefore to investigate how science works (Biagoli 2000, XII).

Interestingly, such a self-imposed division of labour runs counter to the usual arguments of scholars in the humanities and the social sciences — from Foucault to Garfinkel, from Bourdieu to Giddens. They believe that the social sciences and humanities can never aspire to create a real theory, simply because their subjects never remain immutable and separate, but react in unforeseen ways to the interpretations of social scientists.²² There is one further problem with Biagoli's position: it is blatantly anti-STS. Why should the otherwise widely accepted notion of a co-construction between research and subject not hold for STS? Even if we grant that scientists as primary producers have a certain power to define and delineate what counts as science, why should the STS student be obliged to feel bound by such a view, especially since we know that such delineations are always subject to cultural differences and will be contested and altered over time? There is also a curiously a-historical tinge to Biagoli's argument. It may be true that STS in its present state takes a rather passive stance in defining its subject matter. Ironically, the science wars, which are for Biagoli evidence of the colonizing fervour of the natural sciences, came at a period when the delineatory ambitions of STS were low. Some decades earlier, demarcation of science from non-science was a hotly contested issue, especially among natural scientists and philosophers, who in the end had to withdraw. As Michael Aaron Dennis has shown in one of the rare articles on the history of the (social) history of science, the definition of science over time was closely linked to vested interests of U.S. research policies. Especially the debates about internalism and externalism and about the distinction between fundamental and applied science/technology were closely linked to the necessities of legitimating the U.S. (military) research budget when facing an ignorant public (Dennis 1997).

How did the curious renunciation of its own definition of science come about, leading surreptitiously to others' definitions seeping into STS? In the founding era of STS, one salient driving force was dissatisfaction with the Mertonian programme, since it left out the substantive content of science. Conventional wisdom holds that the reason for this was that even sociologists mistook science as existing outside society (that science is part of society should be a truism for any sociologist, since no human activity can exist outside society). Before Merton, the sociology of science was yet another sub-field

of sociology concerned with a societal sub-system, such as law or religion, which had arisen in the course of functional differentiation. SSK, the sociology of scientific knowledge, arose as a response to this grave deficiency, but subsequently parted company with sociology. It started with inquiries into the epistemic core of science and gave itself a name that reflected this task in a programmatic way. It focused on scientific *knowledge*, rather than on things, artefacts or technologies. Obviously, in order to show it could achieve its goals, it had to crack the hardest of all possible cases. In principle, SSK could have returned to general sociology/anthropology/history after having accomplished its mission and could subsequently have enriched the sociology of science with its newly gained insights and methodological tools for analysis. If that had happened, we could have today an equally vibrant and dynamic field of science studies, but one where science would still refer to the peculiar characteristics and processes of a societal sub-system, thereby making it also easier to link the sub-system of science to other societal sub-systems. Science would have been kept as an object of study, which thrives *within* society.

Unfortunately, STS took another route. *First*, it added technology to its name. While this may have been simply due to a pragmatic-strategic move through which its initially small territory could be expanded, it had unanticipated consequences. The technology strand in STS never gained the stature or weight that the science strand has, thereby creating an unresolved tension. Technology is definitely not a societal sub-system. Depending on the definition used, it is considered a strict coupling of cause and effect, whatever is taken to be the coupling mechanism (Luhmann), an ensemble of human-made artefacts, from household gadgetry to large technological systems, or a simple means-ends relation.²³ Since technology cannot be confined to any societal sub-system, but is diffused throughout society, its only link to science is at the point at which science forms the basis for the emergence or development of *new* technologies. This is in fact what the widely used term *techno-science* is meant to convey. But it hardly accounts for the multiform plurality of what Shinn calls the *regimes* of science and technology, which are defined by their thematic axes and markets of diffusion. The *transversal regime*, for instance, is engaged in producing generic instrumentation, such as the ultracentrifuge, lasers or spectrometers. The members of this community move between science and technology and its markets are widely diffused (Shinn 2000). The study of scientific instrumentation, embodying science but also giving rise to new science, seems for the most part to be left for historians to study. Technology studies in STS deal mainly with glitzy high-tech and with the emergence, impact and use of these new technologies and not of those already known and in use — a fascination it seems to share with the rest of society. But to limit the social study of technology to those areas where science gives rise to them (and the relatively short periods of techno-genesis in the strict sense) cuts out the vast terrain on which technologies actually emerge, evolve, are shaped by and shape in turn the societal fabric. Much of it occurs outside science strictly speaking or on the porous boundaries with the science system. Edgar Zilsel, one of the forgotten founders of STS, remarked that until the end of the 16th century new technologies emerged among craftsmen and not at universities at all (Zilsel 1942). Even today, most technologies such as telecommunication technologies and those of the entertainment industry are developed outside of the system of science or are byproducts of scientific research. One prominent example for the latter is the World Wide Web, which

originated within CERN, where it had merely been a means to process the huge amount of data generated by and for the use of an otherwise totally different scientific research agenda.

Second, STS usually argues explicitly or implicitly against functional differentiation (Knorr Cetina 1992) implicitly, because so many of its studies demonstrate the manifold influences of power, legal or cultural constraints or how economic factors and similar considerations intervene in the actual working of science. The explicit refutation of functional differentiation occurs when it is argued that science cannot be compartmentalised or when the seamless web characteristics of networks constituted by humans and non-humans are emphasised. But when STS is confronted with the question of what science is, if it is not a societal sub-system with its own distinct mode of functioning, the answers become vague, as shown above. The point here is not to argue that a specific theory of functional differentiation, such as Luhmann's, has to be espoused. The point is rather that *either* there exist different sub-systems or fields, which must necessarily have their distinctive characteristics and definable modes of operation, *or* no such distinctions can be made. And if this is the case — why should one still speak about science?

If STS is right in claiming that we cannot define science as something distinct and stable and if, indeed, negotiation is at the heart of science in the making, how else can science be defined? If overflows and the framing between different sub-systems, as claimed by Callon, are as essential as is their control, does this not also hold for science (Callon 1998a)? And what would be the substance of its overflows? As far as we can see, there are no commonly accepted or explicit assumptions about the macro-evolution of societies or about the dynamics through which science is part of these developments, with the possible exception of Latour's idea that societies become increasingly more complex by creating ever longer networks made up of humans and non-humans alike (Latour 1993).

If we were to accept the arguments made in the two points above, then we could reach an agreement on a new definition of what STS is all about: STS is not about observing, describing and analysing the societal sub-system of science. Rather, it is the study of *the production of new knowledge and the production of new technologies* (or things). This would imply that there is not one special and privileged site where to localize science, such as within universities, but that the sites of knowledge production are distributed throughout society. Scientists would remain the most important producers of scientific knowledge, but their great diversity would become more visible, as would their multiple interdependencies with other co-producers in many different and heterogeneous sites. Above all, we would come to see clearly what distinguishes STS from other social sciences and what it has to offer as a kind of vanguard: the basic, underlying assumption *that modern societies are predominantly shaped by knowledge and technology*. This assumption can then no longer simply be taken for granted by the social sciences, only to be ignored afterwards. Instead, it would have to become an utmost research priority and the basis of all further social scientific research, including the knowledge and technologies that underlie the functioning of the social sciences themselves. It would also greatly facilitate to link STS back to social theory and highlight at the same time the potential contributions of STS to general social theory.

THE PLACE (AND A PLEA) FOR STS IN THE SOCIAL SCIENCES

On its way towards institutionalisation as an academic field or even as a discipline having science (encompassing the natural sciences and/or the techno-sciences) as its central object of research, STS began to move out of the fold of the other social sciences. Like science itself, which is often seen as existing outside society, STS strove for a position with privileged analysis, putting itself above or even outside the other social sciences.²⁴ The price for having established a multi-disciplinary and relatively open scientific field was the severing of many previously existing links to general sociology, organizational sociology, studies of higher education, sociology of medicine and of technology, history, not to speak of that branch of economics which analyses the economics of research and innovation.²⁵ By adopting the concept of science as understood by scientists, and notwithstanding evidence to the contrary from its own highly diverse micro-sociological case studies, STS continued to treat science as if it were a coherent social institution. It lost sight of many developments and processes which had occurred on a macro-sociological, economic and political level, developments which jeopardize what is understood by science or at least point in the direction we have indicated as Mode 2 knowledge production. The reasons for this new mode of production having emerged at the present time are not hard to find. In the first place, Mode 1 has been eminently successful. However, over the years the number of graduates trained in the ethos of research together with some specialist skill has become too large for them all to be absorbed within the disciplinary structure of academic life. As a consequence, the number of sites where competent research can be carried out has increased. These individuals and the organisations they work in constitute the intellectual resources for, and social underpinnings of, Mode 2. Further, the development and rapid diffusion of the new information and communication technologies have created a capability which allows these sites to interact. They have set the stage for an explosion in the numbers of interconnections and possible configurations of knowledge and skill. The result can be described as a socially distributed knowledge production system, in which communication increasingly takes place across existing institutional boundaries. Even more important may be the fact that knowledge production in this societally distributed system takes place *in the context of application*, thereby also increasing its openness (Gibbons et al. 1994).

It is obvious that the social sciences could profit from science studies, just as we claim that STS has to re-establish its links to the social sciences and especially to social theory. What STS has to offer to the other social sciences is above all the understanding it has gained of *how knowledge is produced, maintained, stabilised, closed, contested or negotiated* — in short, it can bring its analytic insights to a domain which still works with the assumption that the concept of knowledge is largely unproblematic and uncontested, because it is treated as given. Many social scientists still operate along an invisible divide that takes the social as malleable and weak, while the rest, be it the natural world or the artificially constructed one, is assumed to be given and hence can safely be left out of social analysis.

The obstinence of this divide has deep historical roots, some of which have recently been illuminated by Stephen Toulmin. He locates the separation as one that took place more than three hundred years ago, when reason, seen as embodied solely in formal-

ised and deductive knowledge, took on the hegemonic lead and was split from reasonableness, the knowledge provided by the *sciences morales* about the standards and circumstances under which reason could be implanted and applied in the world of institutions and relationships, none of which can function without human judgement (Toulmin 2001). But the most pernicious of the distinctions still overshadowing the social sciences awareness of their own modes of knowledge production is that between hard and soft. By accepting the verdict that their knowledge is inevitably imprecise, subjectively distorted and inherently unable to make predictions, many social scientists remain trapped in their own inferiority complex. The only escape routes open then are either to harden their knowledge by imitating the natural sciences or to insist on their special status, thus reinforcing the alleged correctness of the divide.²⁶

The espousal of a thorough STS perspective would dissolve both the masochistic ritual of re-asserting the necessity to harden one's knowledge and open the door to a vast field of productive insights in the social sciences and humanities. From statistics to transcripts to the actual practices of social surveys as one of the main methodological instruments in use, from the conduct of clinical trials to how regulations come about and to other legal and administrative practices — there is hardly a sustained awareness of the processes and practices through which the social sciences produce their knowledge and how they shape their objects of research. The sociologists level of discussion about sociology is not yet sociological, writes Kieserling (Kieserling 1999, 405), to which we could add: their (the social sciences) level of discussion has not yet reached the level offered by STS. It is to the credit of the *Yearbook in the Sociology of the Sciences* that it has explicitly taken up the challenge; not all of its potential audiences, however, have yet been reached.

There are three important issues to be considered in deciding what a restructuring of the social sciences — and of the humanities²⁷ — from an STS perspective would imply, and which pertinent insights and findings it could generate. First, most of the definitions of what the social sciences are rest upon untenable assumptions. The differences from the natural sciences are usually reduced to the idea that the social sciences deal mainly with human beings, i.e. entities whose special features, qualities, behaviour or values make them distinct from all other existing entities. It is highly doubtful that such a view can be maintained, both in the light of the current knowledge being created in the neurosciences and other life sciences, in primatology as well as on the molecular genetic level. However controversial this new knowledge may be (e.g. cultural anthropologists object vociferously to primatologists applying the term culture to primate behaviour), there can be no doubt that the lines of what it means to be human are currently being re-drawn with vigour. New knowledge is continuously emerging about cognition and language abilities in humans and animals, and the interest in discovering the origins of life is being matched by new questions and new findings about how our early human ancestors evolved and lived.

Science studies in turn have shown over the years that the drawing and re-drawing of such boundaries always occurs for certain purposes, which need to be carefully examined in each case.²⁸ They are also keenly aware of the implications which the divide between humans and non-humans has for any comparison and distinction between social science knowledge and natural science knowledge, with different functions assigned to each in specific circumstances. Last but not least, a closer look at what the

social sciences actually do, in contrast to what they claim they do, reveals a great diversity in what it means to be human. The diversity of the human entities dealt with by the social sciences and humanities is at least as great in range of qualities, behaviours, values and other special features assigned to them, as that attributed to the gap held to separate humans from non-humans. Moreover, in actual practice and in theoretical understanding, humans must be deconstructed, if such different phenomena as the functioning of the economy, the behaviour of firms, voting patterns, collective and individual action, family ties, or innovation are to be properly analysed and explained. To move away from generic descriptions such as the human factor or *verstehen* as a unique domain of the humanities would not only result in a more adequate and refined picture of what social scientists actually *do*, but it would also further their self-understanding and help them to escape the trap of believing that they merely represent the less developed natural sciences. Perhaps even more importantly, the problem of what it is to be human would be integrated into the epistemological basis of the social sciences. Instead of insisting on an essentialist position which can never (and is never) followed in practice, the social sciences would have to confront the *question* of what it *means* to be human in specific circumstances, and of what follows from it.

The *second* issue is closely related to the first. Much of social theory still operates without an elaborated concept of how to deal with the realm of the *non-human*, be it the environment as a general sociological term separating the social world from the rest, whether artefacts and technologies of different kinds or an entity like nature as used in environmental discourse.²⁹ This is not to say that what is *non-human* is not taken into account, but it is often done either in an ad-hoc manner, treated as a given boundary condition for social life and action which does not invite closer analysis, or as an often recalcitrant factor or condition which resists and reacts to human action. An STS perspective would have much to offer by showing that it is impossible to refer to any environment or to the materiality of the non-human world without realizing that the established conceptualisations have originated in science and are shaped and altered by science. The power of defining the natural world still lies with the natural sciences. They retain a monopoly on scientific conceptualisations and safeguard the access routes to understanding and explanation, even if it is granted that this monopoly is not complete. For all practical purposes, there is no direct route from the social world to a *non-human* world, be it natural or artificially constructed. The social sciences, if they want to understand the growing complexities and expansion of the materiality in which humans live and act, have necessarily to pass through a language of description and explanation, including the meanings attached to them, as originated in and imprinted by concepts and practices from science and engineering. This is not to acknowledge that these technoscientific repertoires have to be accepted *tel quel*. STS teaches us that scientific conceptualizations are historically and culturally bounded. They are hence unstable and — within limits — open for contestation. If the social sciences were seriously to reconceptualise the materiality of their social practices and of the environment in which humans act, they would gain, as in the first question of what it means to be human, an understanding of the — flexible and malleable — meaning of this materiality and of the manifold processes and forms of interaction with it.

The *third* issue is the development of a much longer historical perspective for the social sciences, one in which the role played by science and technology has its integrated

place along with other institutional and epistemic transformations connected to the making of modernity. Due to the pioneering work of Wittrock and others, the multiplicity of modernities is today coming to the fore in a world-wide context, together with a better understanding of the emergence and spread of democratic nation-states, liberal market economies and a relatively autonomous science based in research-oriented universities (Wittrock, Heilbron and Magnusson 1996; Wittrock 1998). It is no longer sufficient to analyse the major transformations in the West as being premised on industrial-technical and on political revolutions, without including the epistemic and institutional transformations and novel practices which underlie them. The emergence of distinctly modern key concepts, such as the discovery of society, the role and function of historical enquiry, of language and new collective identities, needs to be situated in particular institutions and in intellectual practices. This forms part of a long-term agenda upon which the social sciences have embarked. In her work on the origins of knowledge, religion and politics in modernity, Rivka Feldhay has questioned the narrative in which the constitution of each as an autonomous sphere is usually told, highlighting instead the variable figurations they have formed over time. The idealisation of science tends to portray it as a prototype of objective discourse capable of producing value-free, neutral and factual types of knowledge. This kind of knowledge becomes disembedded. What is generally overlooked is that public forms of knowledge are mediated in complex ways through language and the construction of trust. They depend and are embedded in a wider arrangement consisting of other facts, values and interests (Feldhay 2001).

Far from remaining separate institutional entities, the embeddedness of scientific practices, their moral economy and ethos in the societal fabric of the day would become more visible, but so would the specific contributions and the functions of scientific and technical knowledge in the military and colonial endeavours of the nation-state and, more generally, the role played by science and technology in inventing, sustaining and changing the project of modernity in the West and its entangled history with the rest of the world. The STS perspective could be extended backwards in time, in close interaction with historians, historians of science and technology, economic historians, sociologists and political scientists.

THE PLACE (AND A PLEA) FOR SOCIAL THEORY IN STS

Mertonian sociology of science had never attempted to move into the substantive core of science but remained instead on the organisational-structural side and focused on the functioning of science as a sub-system. When Merton turned away from the Parsonian system to theories of the middle range, he nevertheless left room for partial theories still modelled on Western society in general. With the emancipation of STS from Mertonian sociology, such carefully delineated ambitions and assumptions were also left behind. *More studies on science meant fewer on society*. This went so far as to lead to the term social being declared outmoded (Latour 1993).

But the problem of situating the substantive core of science, of assigning it a place and a time in an environment, which retained a whiff of the social, still remained. It was solved by radically and thoroughly contextualising all scientific practices to be studied. Context would from now on become a concept replacing every thing previously thought to represent social structures or hierarchies, social differentiations or functions.

Since the level of analysis of STS had also taken a decisive turn towards the micro-level, context became even more ubiquitous and useful, since it could now be applied to everything surrounding the micro-objects under study, from instruments to the beliefs of those who operated them, from institutional arrangements to patterns of conflicts or co-operation among scientific practitioners.³⁰ The emancipation from philosophy of science largely meant a re-focusing on individual case studies and brought with it the thick descriptions taken over from anthropology. Issues of epistemology, if treated at all, were now also subject to this kind of thickening. No new epistemological patterns emerged. At most, a higher resolution of (old) philosophical generalities had taken place.

The double shift to case studies on the micro-level and the general substitution of society by a varying and particularistic context created a theoretical vacuum which was, however, rapidly filled with a stream of new concepts, languages and vocabularies. Attempts at systematising this sprawling but also highly stimulating conceptual mix finally succeeded. Actor-network-theory became *the (only) theory* at hand, widely accepted within STS and attracting followers even from outside. The ANT approach worked beautifully and was extremely helpful in connecting humans with non-humans, in showing the heterogeneity of network associations and, more generally, in providing a view which allowed many old, uncritically reproduced assumptions about science and its implications to be swept aside. In its focus on the immediate and the near-by, however, it also eclipsed some of the more important regularities, as well as attention to changing processes of societal development. The problem with ANT is that it works on two levels which are at opposite ends of the scale along which most social sciences operate: the ontological-metaphysical level and the level on which local and particular contingencies are at work. The theories of the middle range, which might connect these opposites, are not yet in place.

In a memorable exchange during the EASST conference in Bielefeld some years ago, social theory, personified by Niklas Luhmann, the grand master of systems theory, was invited into the inner halls of STS for a debate with Bruno Latour, grand master of STS and spokesperson for the excluded things, the actants. Luhmann patiently insisted on communication being the fundamental element that constitutes society. For Latour, the world could not be more different. For him, it was the materiality of things and their interconnectedness with the social world which constituted the specificity of science and technology, leaving no room for differentiation of any kind. What was most stunning was the missed opportunity of an encounter between such radically different but equally sophisticated theoretical descriptions of the world and to explore what followed from each. What would have happened if the world of things and actants, in all their rich heterogeneity, messiness and fleeting transformation into ever forming and dissolving networks, had taken seriously a world where distinctions have to be made all the time in order for communication to occur and observation to be possible?³¹

STS has been able until now to get on with this theoretical anomaly, in which it has de facto only one theoretical position, largely because it has continued to see one of its main tasks as — to use an old-fashioned expression — falsifying traditional notions of what science is, does or claims to be. As already noted, there appears to be an unlimited supply of such notions and representations, coming from many different sources all of which continue to replenish themselves. STS's success consists in showing that *it was not so*. There is nothing wrong with such a self-imposed task. Moreover, it fits well into

the tradition of the Enlightenment, with a small concession made for a bit of subversion and a whiff of heresy.³² This leaves one question open, however: if science is *not* what everybody, including the scientists and the public, thought it was — then what is it?

It is here, we believe, that a place must be made for social theory. What counts as theory in STS is usually tailored to the case at hand: it remains particularistic. Systematic attempts at comparison of case studies and their theoretical content are extremely rare, nor are collaborative efforts in such projects encouraged by the existing reward structures. Theory in STS is often understood in the sense of critique, as a way of questioning the arrangements one has found rather than of coming up with propositions which would allow one to find some regularities to explain different kinds and realms of order or disorder. If STS were to take seriously the *processes of contextualisation* impinging upon and transforming the production of scientific knowledge today, it would inevitably have to deal with social theory (Nowotny, Scott and Gibbons 2001).

This is not to be taken as a plea for any particular theory, but a general call for integrating social theory into STS, just as we have argued above for a much stronger integration of STS into the social sciences. At this point it might be objected that the age of grand theories, whatever their provenance, is over, or that STS, having successfully emancipated itself from sociology, should not fall back into a state in which theory is largely synonymous with the work of one great old (or dead) man. Our conception of social theory is different. While we acknowledge that the theoretical advances in sociology, to which we shall limit ourselves, have been made by a small number of individuals, we maintain that their impact on the field has only been possible through a widespread, collective effort. Moreover, most of these theoretically-minded individuals have been acutely conscious of the need to continuously ground, nourish and complement, as well as to repeatedly test, their own theoretical work with empirical data of different kinds, whether produced by their collaborators, themselves or, as in most cases, through the work of those in the field who have been inspired by their theories. This is as true for the founding fathers of sociology, Weber, Marx and Durkheim, as it is in the case of the more recent theoretical contributions made, for instance, by Bourdieu³³, Boltanski and Th venot, Elias, Luhmann or Wallerstein. We will concentrate on these last four theorists, since we believe that their work shows many, as yet unexplored, links to STS that could be followed. Although Luhmann and Boltanski/Th venot are considered important theorists in their own countries, their international influence has been remarkably low. Although we can only briefly allude to the kinds of questions their work in social theory raises for STS and although we do not maintain that their perspectives, insights and problem definitions are unique, we nevertheless believe that they exemplify the potential that social theory can bring to STS.

Norbert Elias was associated with the *Yearbook* from its beginnings until his death. Starting with the seemingly naive question of how the authority structure of the sciences, manifest in the position and function of scientific elites and in the pervasive hierarchies of disciplines, affects the production and development of knowledge, Elias moves on to show numerous interdependencies in long-term historical processes with planned but unintended outcomes (Elias 1982). Scientific elites, he maintains, first had to obtain their relative autonomy from other kinds of power elites. They had to strengthen the tendency to see one specific type of scientific knowledge as the only valid one. They differ from other power elites, however, since innovation and the production of new knowledge

cannot be rendered routine and must remain open to being tested in non-authoritarian ways. Moreover, knowledge production is seen as a long-term, collective, inter-generational process whereby scientists build upon work done by others. Yet maintaining control and/or a monopoly over the kind of knowledge produced by science also implies highly variable figurations of the established and the outsiders with reciprocal forms of dependence. Elias was very concerned with showing that scientific knowledge, as controlled and monopolized by the scientific establishment, is one special kind of knowledge. Knowledge, for Elias, is a nexus of human-made symbols used as a means of orientation and communication. It is acquired socially as much as it is learned by an individual. It has roots in biology, but is strongly social in the process of acquisition and in its progress. Knowledge advances by attaining ever higher levels of synthesis, such as the theory of (biological) evolution, which has moved beyond the state of law-like theorising towards process models and process-oriented theorising. Having reached a higher level of synthesis also means that the fit has increased between a symbolic (inner) representation made by humans and the outer world.

What Elias' theory of knowledge (or symbol theory, as he also called it) offers is one of the rare attempts to bridge a natural science perspective with a resolutely social perspective of the *longue durée*, in the course of which the mutual chains of interdependence between humans lengthen while their capacity for evolving new means of orientation (knowledge) also grows. Scientists (including social scientists), as a special case of an established power group that is capable to create and maintain a monopoly over the kind of knowledge it produces, therefore mediate between the two. Elias pleads for a sociological type of enquiry, one capable of working out process models of the development of knowledge, fitting into but not reducible to models of the long-term development of human societies. One cannot shuffle, he maintains, forms of thinking into one compartment, the objects of thinking into another, human experience into a third and the social groups within which standards of orientation and thinking develop into a fourth. The actual, observable development comprises all these factors closely interwoven. What will then become clear is that the social value — and hence the power ratio — of groups of scientists is related to the cognitive value of what they produce. Sociologists, according to Elias, would be well advised to reconsider their own position within the hierarchy of scientific monopoly-holders. If the relative autonomy of a scientific group is highly connected to the relative autonomy of its subject matter, they have to avoid becoming squeezed between two power blocks: between the physicists (and the hegemonic claims of nature) on one side, and the ideologies of the political world on the other.

Niklas Luhmann was one of the most proliferate and elaborate theorists social theory ever has seen, but he remains little recognized in the Anglo-American literature. For STS, Luhmann promises to be a productive partner in dialogue. He devoted considerable analysis and refined articulation to his theory of science as one of the subsystems of society (Luhmann 1992). His work displays parallels with, but also striking contrasts to, STS. If one takes the observation of science to be one of the tasks STS has set itself, one is quickly confronted with one of the basic assumptions of Luhmann's theory according to which each societal subsystem can only observe another in its own language. A primary and privileged point of observation does not exist. Reflexivity enters through second-order observation, when the system observes its own observations.

(Contrary to much reflexivity in STS, reflexive communication for Luhmann only concerns what can be publicly communicated. Luhmann denies that private experience or emotions can ever be made the content of reflexive communication.) All observations are made from a specific location within a system and remain within the system. This theoretical position strips the reflexivity debate of all its moralism, but also of the agonies of continuous self-doubt and questioning. According to Luhmann, there is nothing special about the position of social (or other) scientists. They only follow the communication rules of their own subsystem, and their analysis thus remains confined to what these rules allow them to do.

This position derives from Luhmann's theory of functional differentiation, which stood in such sharp contrast to Latour's position when the two publicly confronted one another in Bielefeld. Modern society is characterised by ongoing processes of functional differentiation, resulting in each subsystem's developing its own code or way of distinguishing between what belongs to the system and what is outside, i.e. in its environment. The code for science is the distinction between true and false; for law it is right/wrong, for politics ruling party/opposition. All decisions within a subsystem have to follow the distinctions upon which the code rests. This is important for science studies since Luhmann claims that it is impossible for a system to work with another code. If the legal subsystem has to decide on scientific questions, it will do so by means of the code of law. Another example is that of environmentalism. Luhmann diagnosed very early that environmentalism can only cause friction and irritation in other subsystems, since it lacks a code with which to operate. Moral content or ethical claims are not sufficient for setting up a code. Science therefore cannot absorb environmentalist ideas, except when it can subsume them under its own code of true/false. The same holds for law, where the idea of assigning a special value to nature cannot be integrated except by transforming it into its code. Luhmann predicted as early as 1986 that the environmental movement would be fully integrated into the existing subsystems, thereby losing all its distinctive features (Luhmann 1986).³⁴

Environmental disputes also figure prominently in the work of Luc Boltanski and Laurent Thévenot, and have been further expanded in a comparative mode in the work of the latter.³⁵ In their theory of justification, Boltanski and Thévenot elaborated several regimes of justification which are applied in modern societies in the case of conflict. The regimes are the following: the market performance regime, the industrial efficiency regime, the civic equality regime, the domestic local and personalities regime, the regime based on the inspiration expressed in creativity or religious grace and the regime of renown, based on fame. In contrast to Luhmann, these regimes of justification are not constrained to operate only in certain subsystems, but can be transferred from one to another. Since all of these regimes (later also called cultural repertoires) can be applied in principle, their actual use depends on the relative valuation accorded them in specific conflicts. What is more, they are all in conflict with one another and are all-encompassing, since every one of them is linked to a definition of what humanity should be. Thus it is possible to denounce one's opponents with a full regime, while one's opponent can do the same with another regime. For Boltanski and Thévenot, the outcome of conflicts depends on the valuation of different regimes and not, if we compare theirs to Luhmann's position, on the fit with the code of a subsystem where they are employed. This allows them to observe the relative worth of different

regimes of justification in different cultures or different times. It would also be possible to empirically investigate whether some regimes prevail in some parts of society or not, and thus it would be possible to observe whether there are different regimes used than those in use in law, for example. It is also possible to ask whether there emerge new regimes of justification, such as a green regime (Moody and Th venot 2000, Th venot, Moody and Lafaye 2000). If such a regime has emerged, as is claimed in Th venot's most recent work, then we could observe a shift from solely humanity-based regimes of justification to nature-based regimes of justification. If this is empirically observable, as Th venot claims, this would be a serious challenge to the humanistic grounds of this theory. It would probably also hint at a special kind of scientific worldview, one which puts the objects of scientific enquiry into the position of regimes. This would mean that what was usually hidden as expert truths are now turned into a regime of justification. Thus it would introduce a naturalistic fallacy as a resource in disputes.³⁶

Immanuel Wallerstein might at first sight be an unlikely choice of theorist with potential interest for STS. Over the last years he has made crucial steps away from his earlier Marxism, with its neglect of interpretive questions. In contrast to the fragmented and scattered empirical work of STS, Wallerstein still believes that a systematisation of social science research is possible and that the macro-structures of the *longue durée* can and need to be analysed. His world-systems theory not only encompasses the past, but also reaches out into the future. For the period from 1945 until the nineties he diagnoses a Kondratieff cycle and claims that the downswing of the cycle in the nineties correlates with a dissolution of the power of the state (Hopkins and Wallerstein 1996). What is of potential interest to STS here is the link Wallerstein construes between a decline in state power and a decrease in scientism as part of a macro-sociological shift.³⁷ Science and scientism were intimately tied to the idea of the state as a machinery kept in operation by science as the major means of guaranteeing its efficiency. Wallerstein's emphasis on scientism raises an interesting question about the emergence of STS as a research field and its own position vis-à-vis the state as a declining power. It could be argued that STS itself is part of the puzzle and that it co-evolved at a time when the authority and legitimisation of the state and, linked to it, of science, were already on the decline.

EPILOGUE: LE NOUVEL ESPRIT DU STS³⁸

Let us now perform a little thought experiment. Imagine that not every new discovery and development in the biomedical sciences or other rapidly developing branches, such as nanotechnology, were to result in the call for setting up yet another ethics committee. Instead, public uneasiness and scientific uncertainty would be channeled to STS for consultation and advice. The STS community would have set up a number of highly competent committees with flexible membership, including scientists and others from outside the STS orbit, in order to guarantee a broad range of expertise grounded, nonetheless, in an STS perspective. Of course, these committees would be thoroughly familiar with the writings and research of Brian Wynne, Sheila Jasanoff, Peter Weingart and others, and would possess the knowledge and communicative skills to gauge the anxieties of the public as much as its as-yet unarticulated desires. Scientists would be extremely relieved, since they would no longer feel constrained by the ever-changing or overly vague guidelines set up by the old ethics committees, guidelines which all-too-

eager legislators and regulators have also sought to translate into the binding form of laws and regulations. Instead, they would delight in a newly won freedom of self-regulation and self-enlightenment. Having learned to partake in STS reflexivity, they would know how to make their own hidden assumptions explicit, and therefore could move on to a higher form of reflexivity, one which leaves room for comparing their assumptions with those of other actors. Eventually, in a true spirit of collaboration with the other members of the committee, it could be shown how different repertoires or ways of describing the world also affect the world. The committees would now be in a position to give genuine policy advice, since they would have learned the answer to the question: What follows?

Let us continue with another little thought experiment. Imagine further that STS, in a move reminiscent of the strategy originally devised by Emile Durkheim to introduce sociology into the French system of higher education, would have succeeded in getting STS courses as mandatory requirements into the curricular training programme of all high school teachers. In order to set up such a subject in secondary education, two preliminary but crucial conditions would have had to be fulfilled. First, in order to promote its view of science, STS would have had to become much more pragmatic and give up some of its sophistication. Particularly the artful jargon practiced in its writings would have had to be toned down to a language which not only high school teachers but also their students could understand and follow. It would also have had to adjust its research agenda, at least to the extent of making room for studies which would allow it to evaluate its own efficiency in communicating, as well as its ability to absorb lay notions. The second condition consists in realizing that science teaching in high schools and universities is, despite serious efforts in science education as a research field, not very professionalized. In many countries science teachers are recruited into high school teaching after having undergone a scientific training themselves with a pedagogical layer on top. After gaining access to the teaching infrastructures at high schools, colleges and universities, STS would have focused on developing curricula for high school students and the education of their teachers. The excellent text books developed by the Open University would have served as additional incentive.

The success of this strategy would have become evident when STS became an obligatory rite of passage for all those concerned with science education. Since practically all highly industrialized countries would be facing a severe shortage in scientifically and technically trained man- and woman-power by now, the problem of attracting more good students into science and engineering could finally be solved thanks to the professionalised and responsible activities of STS in this domain. But this itself would not have been sufficient. The ultimate and intrinsic reward for STS would have come with a market test. STS had always claimed that if science students and their teachers only had a better (i.e. an STS-inspired) understanding of science, then the new generation of students could also make a difference by producing scientific knowledge which showed greater social sensibility. In our thought experiment the market test would have confirmed the expected results. Thanks largely to the STS-inspired curricula with which the new science graduates would have been trained, they would have turned out to be experiencing less difficulty in crossing academic disciplines. Their fungibility would have been considerably enhanced.

Another example in our thought experiment concerns science policy. As is obvious from the above, future policy advisors would also have undergone an STS-inspired training which would have taught them modesty and critical reflection upon their own role. In addition, they would have learned a great deal from Niklas Luhmann about how organisations function. Thus they would know, for instance, that in organizations crucial structures are being built up that function to deal *ex post* with decisions taken *ex ante*, respectively that serve to provide for future possibilities of the retrospective management of decisions that have to be taken now, and would act accordingly (Luhmann 1988, 167). Long gone would be the days when it was possible to get a position in any of the numerous institutions which dealt with research policy on the national, supra- or international level without showing one's STS credentials. STS would have learned from environmental studies its practical lesson in how to professionalise itself. It would now have come to the realisation that multi-layered links had to be set up, linking potential clients seeking advice and consultation for the practical problems they faced. In providing professional services to them, STS thereby would make its own knowledge an indispensable ingredient, diffusing into new laws and regulations and infiltrating into business contracts about exchanging, buying and selling of materials, organisms and other actants needed for the functioning of the research enterprise. STS would advise, but it would also help to regulate intellectual property-rights agreements which would have become even more important now that the funding patterns of fundamental research were able to come from any source, public, private or both, with any number of hidden or overt strings attached. Science and research policies now would be thoroughly in the hands of STS, which would have generated a need for the expertise it had to offer while educating the experts in demand and by expanding even into private consultancies to employ those experts. While the rhetoric about knowledge societies would persist, STS would not only take advantage of it in an opportunistic mood, but would make the issues it raised an integral part of its research agenda. Far from renouncing its critical stand, it would pursue an intelligent policy of professionalisation.

The last example of our thought experiment takes us back into academia. Although the university structures, management and leadership would have changed considerably, STS would have kept its home there after a somewhat difficult period of transition in which university officials would have been more inclined to favouring business and management studies and an economics-of-innovation approach. Imagine, therefore, STS having discovered that it not only *had a future outside academia*, through its many professionalised links, but *also inside*. Surprisingly however, its base within units devoted to further research in STS as understood previously, would have been kept small. Its main research activities would now be distributed all over the university, even in the most unlikely places and research groups, all of them keen on having an STS person as a member of their team. This would have occurred on the basis of genuine mutual respect for the knowledge which each of them, including the STS person, would have brought to their commonly defined problem. STS knowledge production would now be challenged to leave its critical stance behind (often practiced as critique in the past) and become constructively engaged in a joint effort. STS could no longer complain that its findings were wantonly disregarded by their audiences. It would have to attempt to satisfy demands and expectations of more audiences than it could always

manage. Perhaps surprisingly — but the demand would have been so huge and the problems posed so intellectually exciting — the split that some had feared before between a critical and a technocratic strand would not have occurred. It would have been understood in time that such a distinction was arbitrary, operating according to the moral standards of an STS person as an individual, rather than being the collective responsibility of the entire community and its collective vision of the field. Of course, there would remain sufficient room for critical debates, even for controversies, but critique would now no longer be directed at debunking what science was and what scientists did. Rather, STS would be constructively engaged in co-operation with other scientists to help build a world in which science and society met under one roof.

You may now ask how the thought experiment ends. The answer is rather simple and, we hope, not all too disappointing. After David Edge and others had rightly complained about the potential audiences of STS not listening, an anonymous sponsor would have offered a substantial and generous grant for research to look into the problem and to come up with a solution. Initially, some ignominious squabbles about who should carry out this research and receive the grant would have occurred. The sponsor would finally have put an end to them by insisting that an equally anonymous but highly prestigious consultancy firm be hired. Its diagnosis would have boiled down to a two-fold verdict: either no market existed for STS and its products, or STS was behaving like a company which had failed to adapt to changes in its environment. In the latter case, it deserved to go bankrupt and vanish. In the first case a market had to be created and, well, you have just read our recommendations as to how to go about that process.

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NOTES

- ¹ Although STS and the new history of science have moved much closer together, we do not explicitly include work in the history of science here. For a thorough assessment of the future directions of the history of science see the programmatic and comprehensive article by Michael Hagner and Hans-Joerg Rheinberger in this volume.
- ² We will use the two terms field and discipline almost interchangeably. We believe that it is currently not possible to tell whether STS is a discipline or not. If we follow the definition of Turner, then the discipline must be called a discipline, and the name [has to be] shared and used (Turner, 2000, 47). Obviously this does not really apply to STS since in many places in the world there exist different names for what is called STS here. But STS is also not only a field in the sense of Bourdieu or even Whitley, since it shows many characteristics of what commonly disciplines are said to contain: a shared body of literature, methods and theories as well as degree granting units and a career-structure of its own that enables indoctrination of the canon. It is also important here to notice that this text itself is part of a disciplining exercise, trying to strengthen STS disciplinarity. Thus the term field will rather be used with reference