



universität
wien

Diplomarbeit

Titel der Diplomarbeit

A Booming Discipline Short of Discipline?

26 years of STS-in-the-Making in the Journals *Science, Technology,
& Human Values* and *Social Studies of Science*, 1980-2005

Verfasserin

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angestrebter akademischer Grad

Magistra der Sozial- und Wirtschaftswissenschaften
(Mag. rer. soc. oec.)

Wien, im Oktober 2007

Studienkennzahl lt. Studienblatt: 121

Studienrichtung lt. Studienblatt: Soziologie

Betreuerin: Univ.-Prof. Dr. Ulrike Felt

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I. Introduction

There are two ways of reading the following thesis¹, neither of which is necessarily any more or less valid than the other. The first reading would be that this is a self-reflective account about the history of the field of STS², as displayed in the history of its two most far-reaching and longest-standing journals, *Science, Technology, & Human Values* (STHV) and *Social Studies of Science* (SSS), covering the period from 1980 to 2005. It is self-reflective insofar as the author of these pages is dabbling in the academic field of STS, and is indeed using some central notions and approaches taken from within the field in her treatment. It also should not be obliterated that this analysis was instigated as well as supervised by the editor of one of the two journals (Ulrike Felt, who edited *STHV* until shortly before completion of my thesis). At the same time, it was written by someone who has no first-hand experience or recollections of the times in which most of the articles were first published. Depending on perspective, this can either be regarded as a disadvantage, as such memories might help to put things into perspective and add further insights, or as an advantage, as distracting, highly subjective and unavoidably biased personal memories could be argued to obfuscate the analysis. If the thesis is interpreted this way, it must not be read as a neat, straightforward disciplinary history, but rather as an account that highlights the tensions and ambivalences, the differences of outlooks and perspectives that make up the field. It makes no attempt of defining what STS is or ought to be, how it got there or where it should go from here, but hopes to shed some light on the question of how others, at various times during the period covered here (1980 to 2005), have delineated and characterised it, with no effort made to deceive or obscure the inconsistencies and points of contention.

The second way of interpreting this thesis is slightly more abstract in emphasis. It focuses on how an account like this can be read not as a historical description of the development

¹ I have borrowed parts of the title – specifically, the phrase of a ‘booming discipline short of discipline’ – from one of the articles that constitute my empirical material. Unlike in the original text (Bowker/Latour, SSS 1987:4), I have decided to put a question mark next to the notion of a discipline.

² STS can be taken as an acronym for either ‘science and technology studies’ or ‘science, technology, & society’. I will spell out my methodological and conceptual reasons for not settling on either of these two phrasings in chapter II, which outlines my theoretical and conceptual framework. Chapter IV, ‘Searching for a Discipline’, will point out the differences in emphasis that the two terms, however similar they might seem from the outset, can display.

of one particular academic (inter)discipline (interesting mostly to practitioners of that particular branch of academia who undergo a burst of self-reflexivity), but as a case of an emerging discipline or interdisciplinary field, and asks about the role that academic journals can play as arenas where the contents, approaches, and indeed the very disciplinary status of a field are contested and negotiated. While I make no claim that STS is a paradigmatic case of the development of academic disciplines on the whole, or that the pattern to be found here is a general one displayed by other fields in just the same way, I do think that my account here could provide an interesting case for comparison with other disciplines, and that some aspects may well be recognised when looking at other academic fields. I will take up this notion again in my concluding statements.

But before I get to the conclusions, let me sketch out the contents of the following pages: Chapter II provides the theoretical and conceptual framing of the thesis, as well as describing the methodological procedure. This includes an introduction into the central notions and perspectives employed in the remaining chapters, but also a brief outline of the different ways in which STS has been conceptualised in the past. Chapter III, then, should serve as a thematic introduction to the two journals that my analysis is devoted to, giving the readers an idea of the character and developments of both journals and thus serving as a backbone for the following three chapters, which together make up the empirical core of my analysis. Taken together, these chapters are meant to convey an account of how the boundaries of the STS field are being drawn and negotiated, and of where the authors and editors position themselves in the emerging field; separately, they analyse how the authors delineate the object of STS research and writing (chapter IV), its disciplinary status and the relationship with adjoining academic fields and disciplines (chapter V), and the normative debates about the (past, present and desired) social and political agenda of STS scholarship (chapter VI). Finally, the conclusions (chapter VII) serve to draw these separate strands of analysis together and to reflect upon the significance of the findings.

II. Conceptual and Methodological Framework

The central question I have posed for this master's thesis is how the journals *Science, Technology and Human Values* (STHV) and *Social Studies of Science* (SSS) participate in constituting (themselves in) the field of STS. As the last bracket clearly implies, this question is two-fold: It asks about the relevance of these journals in constituting a field which, as such, arguably hasn't existed before the two journals were called into existence in the early 1970s. STS is usually conceptualised as a fairly young, heterogeneous, interdisciplinary research field (a subject on which I will say more in section 3 of this chapter). While my claim here is not that these two journals have determined the shape of the STS field on their own, I do want to argue that they, along with academic conferences, curriculum designs, the publication of handbooks and anthologies and other activities and institutions, have played an important role in shaping what has come to be understood as STS. A strong case in favour of this point is made by Pierre Bourdieu, who insists that "the established scientific order also includes the instruments of circulation, in particular the scientific journals which, by selecting their articles in terms of the dominant criteria, consecrate productions faithful to the principles of official science, thereby continuously holding out the example of what deserves the name of science, and exercise a *de facto* censorship of heretical productions, either by rejecting them outright or by simply discouraging the intention of even trying to publish them by means of the definition of the publishable which they set forward" (Bourdieu 1975, p.30; italics in the original).

At the same time, I do not want to claim a unidirectional influence: Just like the writings published in the two journals and their publication policies have influenced the shape of the field, notions of the field existing outside of these journals have been brought in and have come to shape what is deemed appropriate for submission and publication, as well as what is considered to fall outside of the scope of the two journals. In other words, my theoretical assumption is that of a co-construction of the two journals and the STS field. Yet the academic field of STS and its publication outlets do not exist in a vacuum (or the proverbial ivory tower) either; they have to be understood in the context of their political and social surroundings.

II.1 Boundary Work and Political Struggles

The most important notion here is that the boundaries of STS – perhaps more so than most other established fields of scientific investigation – cannot be assumed as fixed, but rather as continually constructed and knocked down, drawn and re-drawn. My main interest, therefore, is to analyse where and how these boundaries are being drawn and re-drawn by the editors, as well as the authors publishing in the journals – in other words, to analyse the ‘boundary work’ (Gieryn 1995, 1999) that takes place in the two journals. What this thesis is decidedly *not* going to provide, then, is a neat definition of what STS *actually* is. As Thomas Gieryn has spelled out, “[e]ssentialists *do* boundary work; constructivists *watch* it get done by people in society“ (Gieryn 1995, p.394; italics in the original). In this sense, my approach is clearly a constructivist one: My aim is not to add another definition of what the STS field is and what falls within its scope, but rather to watch and analyse the different definitions that have been given by the various editors and authors throughout the 25 years covered by my analysis, thus revealing some of the ambiguities and disputes that have taken place about where and how to draw the line around STS.

To be sure, Gieryn’s own main concern is not with the boundary work between different scientific disciplines or fields – although he does acknowledge the importance of these boundaries, too (Gieryn 1999, p.34) – but rather with the boundary between science and non-science, for example science and religion, science and engineering, or ‘real’ science and ‘junk’ science. In contrast with essentialist positions that assert the importance of demarcation principles to distinguish science from other cultural enterprises, Gieryn claims that “no demarcation principles work universally and that the separation of science from other knowledge-producing activities is instead a contextually contingent and interests-driven pragmatic accomplishment drawing selectively on inconsistent and ambiguous attributes“ (Gieryn 1995, p.393). These attributes are what interest me in my analysis – which qualities do authors and editors in the two journals underscore as characteristics of STS, and how do they differ between the two journals, between individual authors and over time? Unlike Gieryn, my interest is not so much in the distinction between science and non-science, but rather in the distinction between different scientific fields, specifically between STS and its neighbouring fields. However, just like Gieryn’s analysis brought him to investigate the boundaries between natural and social science (Gieryn 1999, chapter 2), in my thesis I will also touch upon the issue of the (non-)scientificity of STS in two

instances: First, there is the question of whether STS, and the social sciences more generally, can be considered a science in the same sense as the natural sciences, and secondly and more centrally, there looms the debate about whether STS is primarily a scientific discipline or a political or social movement. Both of these points of contention show how difficult it is to investigate the boundaries between different scientific fields or disciplines without also taking into account the boundary between science and that which is deemed 'not science'.

As Gieryn (1999) himself has noted, it can be difficult to avoid getting involved in 'boundary work' of one's own. For example, even my decision to talk about 'STS', rather than e.g. 'social studies of science', 'sociology of science' or 'history of technology', can be considered as a type of boundary work, in the sense that it favours one particular interpretation of the field over others. Then again, this is difficult to avoid: referring to the field as 'the entity variously referred to as STS, social studies of science, etc. etc.', of course including every single name that has ever been employed for it, would not make for the most readable text. Not to mention the difficulties of acquiring a complete list of these names, or the fact that it would be misleading to treat the differences as purely nominal, as different words for the same entity. As Tony Becher has remarked in his study about disciplinary cultures, "[c]hanges of nomenclature may themselves prove significant" (Becher 1989, p.143), as what is at stake is more than just the name of a discipline or specialisation, but rather a particular orientation or perspective of research. This explains precisely why it is so interesting to investigate the nomenclature employed by the authors, yet it is also what makes my decision in favour of one term over another problematic. To be sure, my choice was not random: 'STS' is a very general, encompassing term, which does not favour one particular disciplinary approach over another (the same cannot be said of e.g. 'sociology of science', which clearly poses the field as one of sociological enquiry), and it extends to science as well as to technology. What's more, the term itself is marked by a certain 'interpretative flexibility' (Pinch/Bijker 1987), in the sense that it can, and does, stand for at least two different meanings: 'STS' is the abbreviation for 'science and technology studies' as well as for 'science, technology, and society' – two phrasings that I will say more about in chapter V, 'Searching for a Discipline'. By adopting a term that is thus marked by ambivalence, I have tried to keep my own boundary work to a minimum, at

least leaving some of the possible meanings open. At the very least, this serves to illustrate that boundary work is not necessarily something that is done on purpose, with the explicit aim of instituting a particular definition of the field, but rather something that the authors engage in whether they want to or not.

This is not to say that boundary work is insignificant: Drawing and contesting boundaries is part of a struggle over power, authority and resources. While the decision to name or present a discipline in a particular way or to define the object of analysis one way and not another may seem arbitrary or random, it is precisely this type of denomination that shapes the field of enquiry and establishes notions of the academic enterprise, and thus also what is considered an acceptable or valuable contribution to the field. As Pierre Bourdieu stresses, “epistemological conflicts are always, inseparably, political conflicts“ (Bourdieu 1975, p.21). He goes on to explain: “In the struggle in which every agent must engage in order to force recognition of the value of his products and his own authority as a legitimate producer, what is at stake is in fact the power to impose the definition of science (i.e. the delimitation of the field of the problems, methods and theories that may be regarded as scientific) best suited to his specific interests, i.e. the definition most likely to enable him to occupy the dominant position in full legitimacy, by attributing the highest position in the hierarchy of scientific values to the scientific capacities which he personally or institutionally possesses [...] The definition of what is at stake in the scientific struggle is thus one of the issues at stake in the scientific struggle, and the dominant are those who manage to impose the definition of science which says that the most accomplished realisation consists in having, being and doing what they have, are or do.“ (ibid., p.23f.) As Bourdieu stresses elsewhere (Bourdieu 1992), the different definitions of science involved in the struggle can be completely at odds with each other – for example, a publication in a particular journal can be considered a title of nobility by one scholar, but a stigma by another. What the different scholars are working on is thus precisely a “change of the rules of the formation of prices on the academic market, and thus on a maximisation of their own chances of profit“³ (ibid., p.45, translated by AS).

While the actors in Bourdieu’s approach are clearly individuals with an investment in

³ Original text: “Veränderung der Gesetzmäßigkeiten, denen die Preisbildung auf dem universitären Markt gehorcht, und damit an der Erhöhung ihrer Profitchancen“.

imposing definitions of science and criteria of legitimacy, he also counters the idea of an ‘idealistic constructivism’ in which an individual is considered to have absolute power over these constructions of science: “Contrary to what an idealistic constructivism implies, the participants in the field do indeed produce the scientific facts themselves, and to some extent they are also responsible for the shape of the field, but they do so from a position in the field which they did not create and which in fact contributes to the determination of their possibilities and limits. In opposition to the Machiavellian illusion which overpowers some sociologists of science – maybe because they ascribe to the scientists their own ‘strategic’, not to say cynical view of the scientific world – one has to bear in mind that nothing is more difficult, not to say impossible, than to ‘manipulate’ a field.”⁴ (Bourdieu 1998, p.22, translated by AS) The purpose of this analysis, then, is also not to accuse any one author of attempting to or even succeeding in manipulating the boundaries of the field; rather, the various constructions of the field, which are, as Bourdieu has pointed out, influenced by the individual positions of each contributor, add up to one or several collective constructs of what STS is. In summary, the goal here is to analyse the ways that various authors draw the boundaries around STS, and how they define the disciplinary status, the object of analysis, and the political or social agenda of the field. In doing so, the articles under analysis are treated, as Alex Demirović phrased it, “not as an opus which is to be hermeneutically interpreted, but as an activity [...] which intervenes and attempts to create a constellation and a context”⁵ (Demirović 1999, p.37, translated by AS), and which thus contribute to and intervene into social conditions.

⁴ Original text: “Anders demnach, als ein idealistischer Konstruktivismus glauben läßt, machen zwar die Beteiligten des Feldes die wissenschaftlichen Tatsachen selbst, und bis zu einem gewissen Grad sind sie auch für die Gestalt des Feldes verantwortlich, alles aber von einer Stellung im Feld aus, die sie nicht geschaffen haben, die vielmehr dazu beiträgt, ihre Möglichkeiten und Grenzen festzulegen. Entgegen der machiavellistischen Illusion, von der bestimmte Wissenschaftssoziologen übermannt werden, vielleicht, weil sie den Wissenschaftlern ihr eigenes ‚strategisches‘, um nicht zu sagen zynisches Bild der Wissenschaft unterschieben, ist daran zu erinnern, daß nichts schwieriger, um nicht zu sagen unmöglicher ist, als ein Feld zu ‚manipulieren‘.”

⁵ Original text: “nicht als hermeneutisch auszulegende Werke, sondern als Aktivität [...], die interveniert und eine Konstellation und einen Kontext zu erzeugen sucht“

II.2 Disciplinarity and Interdisciplinarity

Above all, this analysis can hope to make a contribution to the blooming (or looming?) debates about the assets and limits of disciplinarity and interdisciplinarity, of traditional disciplines and inter- or transdisciplinary approaches.

As Julie Thompson Klein has remarked: “Ask three scientists what interdisciplinarity means, and they will likely give three answers.” (Klein 2000, p.3f.) While it is therefore impossible to provide a glossary of terms that will hold true for every mention of ‘interdisciplinarity’ or ‘transdisciplinarity’ quoted anywhere in this thesis, it nonetheless makes sense to attempt a definition of some of these central notions that will serve as a guide for thinking about disciplinarity and non-disciplinary approaches.

It makes sense to start with the most basic term, that is, the notion of an academic discipline or of *disciplinary* work. While there exists a large body of sociological and historical work on the development and significance of scientific disciplines, there is no consensus about what the defining characteristics of a discipline are. The main dividing line is the conceptualisation of disciplines in terms of epistemic content, or in terms of social structure. For example, Andrew Abbott’s account of the ‘chaos of disciplines’ “is in the first instance purely cultural; my account is, in that sense, internalist. By contrast, most current views of intellectual succession are externalist; knowledge is somehow wed to power and power propels change.” (Abbott 2001, p.4) Abbott proposes a framework of social scientific knowledge that is based upon the *contents* of scientific knowledge, not on the local practices and interactions that add up to it. On the opposite end of the spectrum in terms of thinking about disciplinarity, there is Stephen Turner, who views disciplines entirely in terms of market forces. His definition of a discipline rests on two elements: “The first is nominal: the discipline must be called a discipline, and the name is shared and used. The second is not: there must be actual facts of employment involving persons trained in the name of the discipline and there must be the beginnings of a market.” (Turner 2000, p.47) Turner later defines disciplines as “cartels that organize markets for the production and employment of students by excluding those job-seekers who are not part of the cartel” (ibid., p.51). In contrast with Abbott, Turner downright rejects the idea

that disciplinarity has anything to do with the contents of knowledge: “Everything else about the notion of disciplinarity, including notions about canons and common intellectual cores – that is to say about the nature of knowledge contents – is, I think, open to challenge.” (ibid., p.51f.) Rudolf Stichweh’s account of the stability of scientific disciplines (Stichweh 1993) also factors out the contents of knowledge, identifying the curious convergence between the research system and the higher education system (that is to say, the fact that scientific disciplines function as a structuring unit not just in the research system, but also in secondary and tertiary education) as the main reason for the persistent stability of scientific disciplines. He thus links the persistence of the disciplinary system to processes of secondary professionalisation and to ties with state, economy and the secondary school system.

Of course, not all accounts of disciplines fall squarely into one of these camps. Tony Becher, in admitting that the “concept of an academic discipline is not altogether straightforward” (Becher 1989, p.19) because it is always open to interpretation whether a field constitutes a discipline of its own or a branch of its mother discipline, establishes several criteria for identifying an independent discipline: “The answer will depend on the extent to which leading academic institutions recognize the hiving off in terms of their organizational structures [...], and also on the degree to which a free-standing international community has emerged, with its own professional associations and specialist journals. [...] International currency is an important criterion, as is a general though not sharply-defined set of notions of academic credibility, intellectual substance, and appropriateness of subject matter.” (ibid.)

After this brief outline of different perspectives on disciplinarity, I will discern between three different approaches of non-disciplinary academic work: ‘multidisciplinarity’, ‘interdisciplinarity’, and ‘transdisciplinarity’. In the following, I will largely follow the framework proposed by Malte Schophaus, Hans-Liudger Dienel and Christoph-Friedrich von Braun (Schophaus et al. 2003), which uses a similar terminology as Michael Gibbons and his co-authors of ‘The New Production of Knowledge’ (Gibbons et al. 1994, p.27-30), but provides a more systematic overview over the different terms.

Multidisciplinary work (Schophaus et al. 2003, p.7f.) usually refers to work that is carried

out independently by researchers from different disciplinary backgrounds, without linking their work to each other. While these researchers thus work on the same topic or problem, and the results might later be synthesised into a shared research report or an edited volume, the research itself is carried out along traditional disciplinary lines, without any spillovers from research in one field into the other – in other words, multidisciplinary “is characterised by the autonomy of the various disciplines and does not lead to changes in the existing disciplinary and theoretical structures” (Gibbons et al. 1994, p. 28). Thinking in terms of academic journals, the main concern of this thesis, a multidisciplinary journal on science and technology would publish articles that deal with issues of science and technology from various disciplinary perspectives, but the articles themselves would be stay within the bounds of one particular discipline.

The concept of *interdisciplinarity*, on the other hand – although it is often used as a generic term for all kinds of non-disciplinary work – implies that some kind of exchange between different disciplines takes place, that there is a “formulation of a uniform, discipline-transcending terminology or common methodology” (ibid., p.29). As Schophaus, Dienel and von Braun point out, “[i]nterdisciplinarity refers both to the cooperation of researchers across disciplinary boundaries and to the fusion of various domains of knowledge in an individual researcher’s mind.”⁶ (Schophaus et al. 2003, p.6, translated by AS). Again, translating the notion to that of a journal, an interdisciplinary article could either be a collaboration between authors from different disciplinary backgrounds, or a single-authored paper (or multi-authored paper whose authors all had the same disciplinary training) that combines perspectives or approaches from different disciplines.

Finally, a *transdisciplinary* approach “includes the cooperation of various actors from society and science, in order to solve complex societal problems”⁷ (ibid., p.8, translated by AS). Unlike multidisciplinary or interdisciplinary work, this approach also involves participants who have no academic background at all, or who are at least not currently working in an academic context. Not only are perspectives from different academic

⁶ Original text: “Interdisziplinarität bezeichnet sowohl die Zusammenarbeit von Forschern über disziplinäre Grenzen hinweg als auch die Verbindung unterschiedlicher Wissensbereiche im Kopf eines einzelnen Forschers”

⁷ Original text: “schließt die Kooperation von unterschiedlichen Akteuren aus Gesellschaft und Wissenschaft ein, um komplexe gesellschaftliche Probleme zu lösen”

disciplines brought in, visions from outside academia are included as well. A transdisciplinary journal therefore might not be of a purely academic character, also bringing in perspectives from outside of the academic domain, for example from industry representatives, policy makers, or even laypeople. In their discussion of the implications of transdisciplinarity for scientific publications, Gibbons et al. observe: “Not only is the average number of authors increasing, but much more significantly, so are the diversity of specialties and disciplines involved in the writing of a single paper and the range of institutions and organisations from which the authors originate.” (Gibbons et al. 1994, p.34)

As the preceding paragraph has already hinted at, ‘transdisciplinarity’ is also one of the central notions of what Michael Gibbons, Helga Nowotny et al. (Gibbons et al. 1994; Nowotny et al. 2001) have referred to as the ‘Mode 2’ of knowledge production. In a nutshell, ‘Mode 1’ is the traditional mode of knowledge production, organised along disciplinary lines, carried out “in a context governed by the, largely academic, interests of a specific community” (Gibbons et al. 1994, p. 3) and guided by standards of quality control defined by the academic peers. ‘Mode 2’, on the other hand, is characterised as being carried out in a context of application, in transdisciplinary settings and with additional criteria of quality which take into account “a diverse range of intellectual interests as well as other social, economic or political ones” (ibid., p.8), such as cost-effectiveness or social acceptance. The claim is not that ‘Mode 1’ is being supplanted by ‘Mode 2’ knowledge production, but rather that it is being supplemented by these new trends – in other words, the two modes exist side-by-side, but interact with each other and thereby affect the entire institutional research landscape.

While the authors proclaim their goal to be the diagnosis of transformations that currently take place in the scientific system (ibid.) as well as to extend their reflections to the impacts on society (Nowotny et al. 2001), critics have pointed out that “the authors of the transdisciplinarity, post-normal, post-modern, and Mode 2 schemes are oscillating between empirical and normative statements” (Weingart 2000, p.36). Other concerns have been raised about the validity of the underlying ‘Mode 1’ scheme, which Stephen Turner has described as “questionable” (Turner 2000, p.62) from a historical perspective, as “this

particular combination of rapid changes in resource is historically unique and the ‘stage’ of disciplinarity that Gibbons et al. regard as stage one, and that many commentators regard as normal, is in fact entirely anomalous” (ibid.). What is at stake in this debate is thus both whether the historical postulates of Gibbons et al. (that is, the assumption of a ‘Mode 1’ of knowledge production) are correct, and whether the transformations described under the heading of ‘Mode 2’ are actually taking place, or are in fact (at least partially) a covert normative prescription of the authors.

Several authors have questioned this second issue, arguing that interdisciplinarity and transdisciplinarity are not nearly as prevalent as usually assumed. Terry Shinn and Bernward Joerges have called into question the “allegation of heterogeneity as an all-embracing and presuppositionless characteristic of social interaction”⁸ (Shinn/Joerges 2004, p.77, translated by AS). While they are thus dealing with social interaction more generally, rather than limiting their argument to the discussion of (inter-)disciplinarity, Shinn and Joerges nonetheless make a clear statement that bears on the question of how common interdisciplinarity really is: not very. In their account, intensive cooperation leads to the formation of groups, which work on an issue in homogenous terms. While heterogeneous cooperation does exist, it is usually marked by ephemerality and instability, therefore dissolving into homogenous cooperation after a short while, unless a new field manages to emerge – which, again, means not a continuation of heterogeneous cooperation, but rather a homogenisation of a formerly heterogeneous approach. Shinn and Joerges do identify a pattern of successful, long-term heterogeneous cooperation, but view it as coupled to very specific conditions and thus conclude: “Heterogeneous cooperation occurs much less frequently than is generally assumed or claimed. Homogenous cooperation, which is favoured by a broad array of ubiquitous conditions, remains by far the prevailing configuration.”⁹ (ibid., p.98, translated by AS)

Peter Weingart makes a similar point with his identification of a contradictory pattern in the debate about interdisciplinarity, arguing that “interdisciplinarity (or transdisciplinarity and similar derivatives) is proclaimed, demanded, hailed, and written into funding

⁸ Original text: “Unterstellung von Heterogenität als einer umfassenden und voraussetzungslosen Eigenschaft sozialer Interaktion”

⁹ Original text: “Heterogene Kooperation kommt viel seltener vor als oft angenommen oder behauptet wird. Die überaus dominante Konfiguration bleibt die homogene Kooperation, die durch eine breite Palette ubiquitärer Bedingungen begünstigt wird.”

programs, but at the same time specialization in science goes on unhampered” (Weingart 2000, p.26). His investigation of the question to what extent the proclamations of interdisciplinarity are actually linked to organisational reality turns out that the funding agencies and research centres in Germany which officially aspire to interdisciplinary research in fact make no effort of establishing criteria for interdisciplinarity or of monitoring the degree to which this aspiration is realised. In other words, interdisciplinarity is often paid lip service to, but only rarely actually enforced. His conclusion is not that interdisciplinarity does not exist as a phenomenon, but rather that “interdisciplinarity and specialization are parallel. They are mutually reinforcing strategies, and, thus, complementary descriptions of the process of knowledge production.” (ibid., p.40)

Julie Thompson Klein agrees with this assessment, but rejects Weingart’s phrasing of a ‘paradox’ of interdisciplinary discourse, arguing instead that we are dealing with terminological confusion: “The relationship between disciplinarity and interdisciplinarity is not a paradox but a productive tension characterized by complexity and hybridity.” (Klein 2000, p.8) Unlike Shinn and Joerges, she does not think that successful interdisciplinary practice commonly results in the establishment of a new discipline: “Practitioners share resources through interdisciplinary programs, societies, conferences, and journals while remaining identified with their ‘home’ disciplines.” (ibid., p.19) Nor does she necessarily agree that disciplinarity and interdisciplinarity are strictly parallel phenomena, as “interdisciplinary activity these days may be *in* the heart of disciplinary practice” (ibid., p.8; italics in the original). In establishing that the “boundary between disciplinarity and interdisciplinarity ‘flows’” (ibid., p.24), Klein also hints at something that should go without saying for anyone who shares Thomas Gieryn’s notion of ‘boundary work’ and the idea that the boundaries between disciplines as well as that between science and non-science cannot be taken for granted: that the boundary between disciplinarity, interdisciplinarity and transdisciplinarity is no more ‘natural’ or ‘self-evident’ than those between disciplines or between science and other cultural practices.

This brief outline served to show that there is no consensus about either the extent or the shape of inter- or transdisciplinary work in contemporary science. However, one point that seems to go relatively uncontested, regardless of whether interdisciplinary research is

considered to be almost the norm these days or a very rare exception to the rule of disciplinary work, is that it needs to be held together by different forces and concepts than disciplinary practice. I will briefly outline two concepts that have been suggested to this effect: Peter Galison's *trading zones* (Galison 1996; 1997) and the *boundary objects* (Star/Griesemer 1989; Star 2004) proposed by Susan Leigh Star and James R. Griesemer.

Peter Galison's concept (which is applicable for different specialties within a discipline as well as for collaboration across disciplines) of the 'trading zone' takes the notion of a disunified science as a starting point, arguing that neighbouring fields are like two distinct cultures that live close enough to trade with each other and therefore are able to "share some activities while diverging on many others. What is crucial is that in the local context of the trading zone, *despite* the differences in classification, significance, and standards of demonstration, the two groups can collaborate" (Galison 1997, p.803; italics in the original). Such collaboration, however, is only possible if there is a common basis for communication, a common language for discussion – what anthropological linguists would call a 'pidgin' or 'Creole' language: "a contact language constructed with the elements of at least two active languages" (ibid., p.831), used at the boundary between different groups. Whereas a 'pidgin' language is adopted by native speakers of different languages in order to establish a basis of communication with each other, usually by way of simplification of existing languages and in order to fulfil only limited communicative functions, a 'Creole' language has become more stabilised and can thus serve as "the language of a self-supporting subculture with enough structure and interest to support a research life without being an annex of another discipline, without needing translation into a 'mother tongue'" (Galison 1996, p.153) – in other words, 'creolization' is the process of a pidgin language becoming complex and comprehensive enough to serve as the native language for a new generation, whereas the less comprehensive and less stable pidgin language can be used for limited communication across communities.

The starting point for Susan Leigh Star and James Griesemer is the notion that science is a collective, heterogeneous enterprise and that "actors trying to solve scientific problems come from different social worlds and establish a mutual *modus operandi*" (Star/Griesemer 1989, p.388; italics in the original). The notion of a 'boundary object' is an important

element of such a *modus operandi*. A boundary object is internally heterogeneous and inhabits multiple social worlds, which enables it to serve as a common frame of reference for researchers from different backgrounds. “Boundary objects are objects which are plastic enough to adapt to the local needs and constraints of the parties that use them, but which are also robust enough to maintain a common trans-local identity. They are weakly structured objects in common usage and become strongly structured objects if they are used locally.”¹⁰ (Star 2004, p.70, translated by AS) Boundary objects therefore allow cooperation in the light of different backgrounds and of a lack of consensus, without requiring homogenisation or what Galison would call ‘creolization’ – their strength is precisely in the fact that they mean *something* to all participants, but not necessarily the same thing. Unlike Galison’s concept of the trading zone, the boundary object does not include a notion of stabilisation, as it allows cooperation across specializations and scientific fields, but does not revise the boundaries of the fields. The cooperation thus maintains its heterogeneous character. Where Galison’s account can elucidate the stabilisation of interdisciplinary fields and the emergence of new disciplines, Star’s concept is more applicable for temporary interdisciplinary cooperation on a more limited scope.

The preceding pages were intended as a brief introduction into the debates about disciplinarity, interdisciplinarity and transdisciplinarity and as a framework for questioning the disciplinary status of STS, as well as of the journals *SSS* and *STHV*. Again, what my thesis is not going to provide is an answer to the question whether STS *really* is a newly established discipline, an interdisciplinary field or something else entirely, let alone a definitive declaration of the merits and dangers of disciplinary and interdisciplinary work. As Peter Weingart has remarked, “there is a substantial body of literature on interdisciplinarity but [...] the large majority of titles are normative and speculative” (Weingart 2000, p.31). My thesis, then, is not going to be making any normative points about disciplinarity and interdisciplinarity, nor is it going to tread far into speculative territory. Rather, it tries to understand interdisciplinarity-in-the-making, to observe the

¹⁰ Original text: “Boundary objects sind Objekte, die plastisch genug sind, um sich an die lokalen Bedürfnisse und constraints der sie verwendenden Parteien anzupassen, aber auch robust genug, um eine gemeinsame translokale Identität zu bewahren. Sie sind schwach strukturierte Objekte in der gemeinsamen Benutzung und werden zu stark strukturierten Objekten, wenn sie lokal genutzt werden.”

crossing of disciplinary boundaries in practice. The contribution it hopes to make is one of highlighting some of the ambivalences and differences of perceptions, pointing out some of the different ways of framing the STS field that exist side-by-side, seemingly without impeding each other.

II.3 A Fragmented (Inter)Discipline-in-the-Making

The following section is meant to give the reader a brief overview over the ways that various authors have – in a variety of handbook articles, contributions to yearbooks and introductions to anthologies – defined and delineated the field of STS, of how they have presented its history and scope. It will not provide a neat disciplinary history or a straightforward account of what STS is or is not – too heterogeneous and contradictory are the sources that it is based on. Rather, the aim is precisely to highlight some of these tensions and inconsistencies, many of which will also be found back in the empirical chapters of this thesis.

One thing that most authors do agree on, however, is that STS has a fairly young history, reaching back only a few decades. Heterogeneity (Felt 2003), disunity (Biagioli 1999), fragmentation (Hilgartner 2003) and instability (Bowden 1995) are all words that are frequently used to describe the field, as is the idea that we are dealing with a “still emerging field” (Jasanoff et al. 1995, p. XI) or an “emerging discipline” (Hilgartner 2003, p.207). There also seems to be widespread agreement that “no collection of essays can declare the canon of this field” (Biagioli 1999, p.XIV) and that the editors of a handbook for the field could not “act as neutral gazetteers of already chartered territory” (Jasanoff et al. 1995, p.XI). Such a handbook would thus have to be “something more than the traditional, treatiselike handbook that would clinically describe the world of STS. The field, in our view, had not yet achieved the hoary respectability that merits such dispassionate, and unimaginative, treatment.” (ibid.)

This is where the consensus ends, however. Mario Biagioli thinks that, for all its disunity in terms of methodological approaches and institutional settings, STS (or what he calls ‘science studies’) is unified in one important sense: its common object of analysis, science. To him, what ‘science’ means can be taken as self-evident and is of no analytical interest:

“Science studies does not define its subject matter because, in some significant way, its subject matter comes prepackaged.” (Biagiolo 1999, p.XII) This view appears rather controversial. For example, in David Edge’s account of how he founded the Science Studies Unit at the University of Edinburgh in 1966, the object of analysis cannot be taken for granted, as Edge reports that, upon his arrival, he found just a bare office, with “no phone, no books, no bibliographical sources, no files, no staff – indeed, it was tempting to think, *no subject!*”(Edge 1995, p.3; italics in the original) Other authors (Guggenheim/Nowotny 2003, p.237f.; Whitley 2003, p.3) have pointed out that the term ‘science’ in STS has often implicitly been used to mean the hard, natural sciences, especially physics, and only rarely the social sciences or law. To Stephen Hilgartner, a one-word summary for the object of analysis of STS would not include the word ‘science’ at all, but rather the term ‘knowledge’, which is, he argues, more general, encompassing different types of societies and time periods, as well as both science and technology (Hilgartner 2003, p.202).

Indeed, to what extent technology and also medicine are part of the object of analysis for STS is another matter of contestation. To Biagioli, these matters are clearly related to the study of science, but at the same time, academic studies of technology and medicine are “large and actively practiced fields whose literature is as sizeable as that of science studies itself” (Biagioli 1999, p.XVI), and are thus to be conceived of as self-contained fields, more or less independent from science studies proper. To Guggenheim and Nowotny (2003), technology is currently very much an object of analysis for STS, but has only become so during the 1980s. Gary Bowden (1995) provides a slightly different account of the history of STS, according to which the first approaches that broke with the standard view of science and technology as autonomous entities in the 1960s actually dealt primarily with issues of technology, calling into question the idea of technology as a neutral tool and focusing on the social impacts of technology. He sees the journal *STHV* as one of the figureheads of this development, while *SSS* is to be considered part of a different development, which took place in the 1970s and focused on sociological scrutiny of the contents of scientific knowledge. For Bowden, these two movements have existed in isolation from each other, until a turn to technology in the latter brought the two communities closer together – a convergence that can be seen in the newly acquired status

of *STHV* as the official journal of the *Society for Social Studies of Science* (4S) in 1988.

These different accounts hint at the possibility that perhaps the object of enquiry of STS, contrary to what Mario Biagioli would have us believe, cannot be taken as self-evident after all – a notion that I will explore in chapter IV, ‘Searching for an Object’. They also point at the different historiographical narratives that are given about the development and history of the field. A big difference in these narratives (and, again, one that will be found back in one of the later chapters of the thesis: chapter VI on ‘Searching for an Agenda’) lies in the extent to which the various scholars see the origins of STS in a purely academic environment or in a social movement. Among those who stress the academic roots of STS are Sal Restivo, who sees STS marked by “interdisciplinary competition, especially between sociology and philosophy” (Restivo 1995, p.95), as well as Stephen Hilgartner, who argues that “STS began as a set of subfields that over time grew increasingly linked, forming an interdisciplinary domain” (Hilgartner 2003, p.207)¹¹. On the other end of the spectrum, David Edge (1995) traces the origins of STS back to three different strands: a tradition of research on science as a social system, mostly serving as an underpinning for ‘more rational’ science policy decisions; an endorsement of a more encompassing and socially responsible education for scientists and engineers; and a democratic impulse by social movements advocating the democratisation of science and technology. Michael Guggenheim and Helga Nowotny also stress the origins of STS in the protest movements of the 1970s, but add that it has meanwhile “retreated into its academic niche” (Guggenheim/Nowotny 2003, p.237).

Another point of contestation is the extent to which STS can be considered as a discipline of its own (still emerging or already stabilised), as an interdisciplinary field, as an addendum to established scientific disciplines, or as something else entirely. To some authors, it seems quite clear that, at the core, STS is a combination of different traditional

¹¹ Other examples of accounts that describe the development of STS in terms of a primarily academic (and especially philosophical and sociological) discourse include Andrew Pickering’s (1992) brief run-down ‘From Science as Knowledge to Science as Practice’, as well as Sergio Sismondo’s (2006) textbook introduction into Science and Technology Studies, which begins with a description of the philosophical pre-history of STS before turning to synopses of the Kuhnian ‘revolution’ in history of science and sociological functionalism (and its critical opponents). The different schools of STS, which are then described, are explained as critical developments out of these philosophical, sociological and historical traditions, rather than as a social movement.

disciplinary approaches to the study of science (and technology) – among these would be Sal Restivo, who sees the field defined by two opposing forces: “[O]ne force tends to push the field in the direction of an increasingly sophisticated sociological analysis. [...] An opposing force pushes the field away from sociological approaches toward more philosophical approaches.” (Restivo 1995, p.109) Helga Nowotny and Klaus Taschwer go even further in highlighting one particular disciplinary approach when they point out that the “sociology of science (or ‘social studies of science and technology’, as the field is also called)” (Nowotny/Taschwer 1996, p.XIV) has become a “home for colleagues who had originally been trained in some other discipline, but who now chose to work in a sociological vein” (ibid.)¹². Gary Bowden allows for a larger number of distinct disciplinary inputs: “Based on current practice, then, STS is a multidisciplinary field [...], an amalgamation of contextualist approaches that have their roots in each of these disciplines” (Bowden 1995, p.72). Bowden contrasts this view of STS with interdisciplinary or transdisciplinary visions of the field, arguing that those other visions may have more active advocates among STS scholars, but do not accurately describe current research practice.

Other accounts of the field see the relationship between the STS field and traditional disciplinary structures as less clear-cut. Ulrike Felt writes: “Being transdisciplinary from its very conception, this research area proved complex to get institutionally settled in ways which allow for a rather open development on the one hand, while conforming sufficiently with classical academic ‘discipline’ on the other hand. [...] As a consequence, much of STS research has never left the disciplinary structures (sociology, philosophy, history, psychology, communication science, etc.) which also meant that interaction between the disciplines working in the domain was left to the individual researcher.” (Felt 2003, p.11f.) In contrast, Stephen Hilgartner’s view of the field stresses that STS *has* left its traditional disciplinary homes, arguing that “as STS matured, it began to transcend its disciplinary roots, creating [a] new intellectual domain” (ibid., p.203). While Hilgartner thinks that STS

¹² Steve Fuller’s account is quite similar, as he writes: “Although surprisingly few of these researchers [meaning a list of important STS scholars he had just recited - AS] are actually trained as sociologists – and in fact they often betray the influences of anthropology, psychology, economics, politics, and literary criticism – they can all be broadly defined as ‘sociological’ in the sense of denying an ‘internal’ history of science that is distinguished in its categories and methods from the history of the rest of society.” (Fuller 1993, p.10) Fuller then goes on to describe this sociological approach as primarily directed against philosophical explanations of science, thus also stressing the antagonism between sociologists and philosophers expressed by Sal Restivo (1995).

still has some way to go in terms of institutionalization and that “the inflexibility of the system of academic disciplines is clearly the greatest impediment to the success of the field” (ibid., p.205), he nonetheless thinks that STS currently has to be considered as “a discipline-in-the-making” (ibid., p.207). To what extent STS can be considered to have ‘broken free’ from its traditional disciplinary roots is thus a matter of contention; Hilgartner expresses the view that STS has already gone some way towards its institutionalisation as an independent discipline, while Felt stresses that ties with the traditional academic disciplines have not been cut entirely.

Like many of the self-reflexive texts quoted in this sub-chapter, Hilgartner’s paper is prescriptive as much as it is descriptive: After outlining the choice between three different institutional futures for STS – a ‘subfield’ model, in which STS would have the status as “a set of subfields housed mainly within traditional disciplinary departments” (ibid., p.205); an ‘interdisciplinary’ model with variable degrees of institutional robustness and autonomy; and a ‘new discipline’ model “with all of the autonomy, resources, and stability that disciplinary status conveys” (ibid., p.206) – Hilgartner endorses STS to strive for disciplinary autonomy. Gary Bowden’s (1995) advocacy of STS as a multidisciplinary field goes into the opposite direction, but is no less explicitly a prescriptive statement. It goes to show that descriptions of the field are often very closely tied up with visions for the future. As Michael Guggenheim and Helga Nowotny have observed, the way that STS scholars present the history of their field shows remarkable “parallels with any other disciplinary history or with accounts of pre-STs history of science” (Guggenheim/Nowotny 2003, p.235), that is to say, it describes the development of the field in terms of a progress from naïve realism to sophisticated constructivism. One thing that is curiously missing, argue Guggenheim and Nowotny, is “an assessment of STS in terms of its own standards and criteria” (ibid.). This statement touches upon an interesting circumstance: Much of the writing on the history of STS has been based on anecdotal evidence and unverified observations, usually expressed from a very personal perspective. However, the terminology used by Guggenheim and Nowotny is problematic – what are the ‘own standards and criteria’ of STS, and who gets to determine them? What’s more, the term ‘assessment’ implies an evaluative component, an impression that is also supported by the prevailing tone of the article. The implication is that STS should be

assessed in terms of how well it lives up to its own promises and standards, as if those promises and standards were monolithic and self-evident; as if all that was needed was for someone to come along and apply them.

My thesis will *not* provide such an ‘assessment’; if anything, it hopes to shed some light on what those promises and standards could be, in all their diversity and inconsistency, not as a groundwork for an evaluation, but because they are interesting in their own right for an understanding of the field. More importantly, it hopes to contribute to an analysis of the development and scope of STS that goes beyond highly individual accounts and instead gives a more systematic, if still partial, view of the field.

II.4 Some Methodological Considerations

It should be noted that the following chapters will jump back and forth between an approach of comparing the two journals with each other, of treating both journals together as one unit of analysis, and of comparing findings about the journals with scraps of evidence about the STS field that come from sources outside of the two journals. This jumping between different frames of reference is no accident, but rather a deliberate attempt to do justice to the complexity, and indeed the inconsistencies, of the underlying material. It is an attempt to avoid, as Charles Bazerman would have it, to “treat the features of texts simply as isolated conventions” (Bazerman 1988, p.4), and instead to take into account “the worlds in which these texts served as significant activity” (ibid.). For *STHV*, the journal *SSS* is part of those worlds; for *SSS*, *STHV* is; and for both of them, various departments, conferences, books and other institutions and practices serve as reference points. To be sure, it would be possible, and perhaps also valuable, to take these institutions and practices into account more systematically than I have done in the following chapters; however, doing so would go beyond the scope of this thesis. It would also necessitate a choice of which actors, institutions and publications to follow beyond the confines of the two journals, and thus risk falling into the trap of telling a one-sided story, unduly emphasising the perspective of just a few (putatively) important actors and institutions – a charge that actor-network theory accounts of science and technology have frequently had to meet (e.g. Star 1991; Fujimura 1992). By focusing on the articles published in the two journals, I have avoided making an a priori choice of which authors

and scholars to consider worthy of analysis, and taken as my research data not the writings of any particular individuals, but rather the totality of publications in the two journals. The systematic analysis thus extends to 26 years (1980-2005) worth of articles and other published formats in the journals *SSS* and *STHV*. At some points, where the convergence of developments in the two journals was striking enough, I have decided to discuss them together, treating both journals together as one unit of analysis; other times, where it seemed more useful to highlight differences, I have instead opted to discuss them separately, bringing in a comparative perspective.

When I claim to have systematically analysed ‘26 years worth of articles and other published formats’ in the two journals, I do not mean to imply that I have read and interpreted every single line published in the two journals during those 26 years. The focus has been on the abstracts and introductory paragraphs of each article (in the broad sense, thus also encompassing formats not listed in the ‘articles’ section of the table of contents, e.g. discussion papers, research notes, etc.), although, on the basis of the tables of contents and abstracts, I have also compiled bibliographies on particular issues that allowed me to, as it were, zoom in on a theme and also take into account the rest of the article rather than just sticking with an analysis of the abstracts and first paragraphs. Specifically, the bulk of chapter III, which provides a general overview of the developments and publication patterns in the two journals, is made up of a content analysis of the editorial material published in the journals, while chapter VI (‘Searching for an Agenda’), has emerged out of a bibliography of articles that deal with the normative or political role of STS. The remaining chapters, chapter IV (‘Searching for an Object’) and chapter V (‘Searching for a Discipline’), are based mostly, although not exclusively, on an analysis of the abstracts and introductory paragraphs of the articles. This restriction was born of necessity, as the source material for these appraisals of the way the authors chose to frame their object of analysis and of the way they positioned themselves in a mesh of academic disciplines was, by definition, every article published in the journals during the appropriate period; an in-depth qualitative analysis of such a large number of papers thus simply would not have been feasible. On the other hand, the delimitation was not random: Since the kind of statements I am interested in for the purposes of this analysis are ones in which the authors position themselves and their work in an academic landscape, it did not seem far-fetched to focus

my efforts on the abstracts, and especially the opening paragraphs, in which such statements of positioning are very often placed. In this decision to look for instances of ‘boundary work’ in the introductory sentences of a paper, I have followed the advice of Pierre Bourdieu: “From this point of view, few things are as illuminating as the prefaces, introductions, preliminary remarks, which, often under the cloak of a methodologically indispensable preliminary methodological explanation, conceal more or less skilful attempts of making a virtue out of necessity, that is to say, out of the immanent constraints and limits of a position and career.”¹³ (Bourdieu 1992, p. 49, translated by AS)

In addition to those quotes drawn from the introductory paragraphs of the papers, the chapters also include my findings from the analysis of obituaries and formats such as erratically published ‘country reports’, which are meant to give the readers an overview over the STS field in a particular country. Looking at these types of articles in full length, rather than just analysing the opening paragraphs, proved fruitful because they form occasions where authors stop to recapitulate about the current state of their discipline or field. While the ‘country reports’ are by definition limited to one particular country, they nonetheless form an interesting part of an appraisal of the field in general, especially if a comparative perspective is brought in as a reference point. Obituaries, on the other hand, are even more specifically focused around one person, but no less interesting for that, as they are places where authors reflect on the standing of the deceased in a field, on his or her achievements and impact – in a sense, an inventory of the relevant academic environment is made. Obituaries also have a clearly commemorative function, and as Pnina G. Abir-Am has pointed out, commemorative rites “offer a unique window of opportunity to follow the complex relationship between the commemorating present – which appeals to the past for the legitimation of diverse and even contrasting conceptual, social, political, and ethical agendas – and the commemorated past” (Abir-Am 1999, p.28). Abir-Am also stresses the importance of these commemorations for the establishment of a collective memory in science; what’s more, they are “particularly suitable for observing not only the outcomes, but the very process of assembling a collective memory” (ibid., p.27). On a

¹³ Original text: “Unter diesem Gesichtspunkt ist nichts aufschlußreicher als die Vorworte, Einleitungen, Vorbemerkungen, hinter denen sich, häufig unter dem Deckmantel einer methodologisch unentbehrlichen methodologischen Vorklärung, mehr oder minder geschickte Versuche verbergen, aus der Not – das heißt den immanenten Zwängen und zumal Grenzen einer Position und Laufbahn – eine wissenschaftliche Tugend zu machen.”

related note, Tony Becher (1989) has stressed the importance of shared idols, symbols and myths for the development of a disciplinary community – again, elements that can be found in obituaries.

As I have already hinted at, chapter III is in large parts based on an analysis of editorial material published in the two journals between 1980 and 2005, including editorials and letters from the editor published in the pages of the journals as well as introductions to special issues and special sections devoted to a particular theme. On top of that, it also includes my observations gathered while (again and again and again) looking through the tables of contents and article abstracts of the journals, attempting to sketch out some overall developments of the journals, as well as a brief comparison of the editorial boards in order to identify institutional overlaps between the two journals. Through this combination of various methods, I have attempted to give a cohesive account of the character and development of both journals. After originally considering enhancing this material with a quantitative analysis of publication data for the two journals, I have ultimately decided that the qualitative analysis could provide such a cohesive account on its own. Quantitative approaches of measuring the field via publication patterns and citation counts have been attempted before, and do not seem adequate to the job of capturing the substantial changes of character as well as the continuities in the two journals. For example, in a citation analysis of various STS journals, Loet Leydesdorff and Peter van den Besselaar saw themselves forced to exclude *STHV* from their analysis, remarking that “this journal changed in character in the middle of the period (1988) when it became the journal of the *Society for the Social Studies of Science (4S)*” (Leydesdorff / Van den Besselaar 1997, p.164) and that it was thus incommensurable with the other journals. For me, precisely this change of character is of central interest – not something to be taken for granted, but rather a focus of analytic attention. Once I had come to terms with the change of character of *STHV*, it also came to serve as a valuable resource of comparison with *SSS* – rather than limiting myself to parallel, but unconnected outlines of the development of both journals, an interesting focus turned out to be whether *STHV*'s change of character found a matching trend in *SSS*, and how the developments in both journals related to each other. Rather than inhibiting my analysis, the substantial changes of *STHV* came to act as a point of orientation, as a structuring element of my narrative.

Another element that structured my account was the concern for gender aspects in every strand of analysis. In earlier drafts of this thesis, a separate chapter was intended to sketch out the thematic development of gender issues in the two journals. The fact that this chapter did not materialise in the finished version is not indicative of a change of opinion on the importance of paying attention to gender issues, but rather of a conviction that such a concern should not be treated as a separate strand of examination, but should rather guide the analysis in every phase. Consequently, gender themes now play an important role in each of the three core empirical chapters: In chapter IV, gender comes into focus as an object of analysis for STS scholarship; in chapter V, reflections on the relationship between STS and gender studies appear on the agenda; and in chapter VI, feminist beliefs are treated as one of the many potential social and political agendas that can guide or motivate STS research.

Nowhere in this thesis do I claim to give a complete overview over the object, discipline and agenda of the articles published in the two journals between 1980 and 2005; clearly, such an attempt would be futile, given the scope of my empirical material. Rather, I have attempted to sketch out some general lines of development, to put the spotlight on a few interesting points of debate. In deciding where to put these spotlights in the first place, I have largely followed my own findings and impressions while systematically digging through the abstracts of the articles for the first time, trying to figure out how to best approach this analysis and where to put my analytic focus in later stages of the research process, and readjusted my focus again at various stages of the analysis. In this sense, my empirical approach owes a debt to the tenets of *grounded theory*, which hold that “the researcher begins with an area of study and allows the theory to emerge from the data” (Strauss/Corbin 1998, p.12).

Another conceptual/methodological debt is owed to the tradition of *discourse analysis*, in particular to its insistence on the “assumption of overlapping correlations of reference between the texts in the shape of discursive structures of the production of statements”¹⁴

¹⁴ Original text: “Annahme textübergreifender Verweisungszusammenhänge in Gestalt von diskursiven Strukturen der Aussageproduktion”

(Keller 2005, p. 270, translated by AS), where individual statements or texts are not treated as something to be judged on their own and then classified in a typology, but are instead put into relation with one another so that a discourse can be reconstructed from the various singular instances. While I did not follow any particular model of discourse analysis step-by-step, my thesis does share a “descriptive-constructive approach, which refrains from classical explanations in terms of independent variables [and] determines the various elements and dimensions of the realm of the object as constituting and stabilising each other”¹⁵ (Keller et al. 2003, p.11, translated by AS). In this sense, my approach can be described as a discourse analysis, if discourse analysis is taken not as a method, but as a socio-scientific research programme, akin to Reiner Keller’s formulation of a *Wissenssoziologische Diskursanalyse*, which “reconstructs processes of the social construction, circulation and mediation of ways of interpretation and behaviour on the level of institutional fields, organisations, social collectives and actors. [...] Such a perspective presumes the normality of symbolic struggles, of a contest of discourses. This is not just a rivalry of ideas, quite on the contrary: The reality-constituting effects of symbolic orders and the constitution of discourses as a *concrete and material*, thus *real societal practice* needs to be stressed.”¹⁶ (Keller 2005, p.188, translated by AS; italics in the original). Of the different kinds of research questions that can be posed in the context of a discourse analysis that Keller goes on to sketch out, the question about how phenomena are constituted (ibid., p. 260f.) in the first place is the most relevant for my analysis. It is also in this context that discourse analysis and grounded theory intersect, as Keller points out: “The identification of data for a discourse analysis is therefore a rather open process of searching in different directions, which can only provisionally orient itself by themes, phenomena of reference, key terms etc. After all, an essential goal of discourse research is exactly an answer to the question which knowledge, objects, connections, properties, positions of the subject etc. are purported as ‘real’ in discourses, by which means – such as schemes of interpretation, classifications, structures of phenomena, storylines, moral and

¹⁵ Original text: “deskriptiv-konstruktives Vorgehen, das auf klassische Erklärungen durch unabhängige Variablen verzichtet [und] die verschiedenen Elemente und Dimensionen des Gegenstandsbereichs als sich wechselseitig konstituierend und stabilisierend bestimmt”

¹⁶ Original text: “rekonstruiert Prozesse der sozialen Konstruktion, Zirkulation und Vermittlung von Deutungs- und Handlungsweisen auf der Ebene von institutionellen Feldern, Organisationen, sozialen Kollektiven und Akteuren. [...] Eine solche Perspektive unterstellt die Normalität der symbolischen Kämpfe, des Wettstreits der Diskurse. Dabei handelt es sich nicht um ein bloßes Wettfeiern der Ideen, im Gegenteil: Betont werden sollen die wirklichkeitskonstituierenden Effekte symbolischer Ordnungen und die Beschaffenheit von Diskursen als einer *konkreten und materialen*, also *wirklichen gesellschaftlichen Praxis*.”

aesthetic assessments – this happens, and which different rules and resources of formation are at the core of these processes.”¹⁷ (ibid., p.260, translated by AS) To be sure, my approach differs from the kind of analysis envisaged by Keller, in that my data was defined from the outset, as my interest was delineated not in terms of a particular discursive theme, but rather in terms of its arena (the two journals under investigation). Therefore, my data came pre-packaged, but my analytic focus within my rather unwieldy research material did not, it had to be carved out in the ways described by Reiner Keller. The following chapters will demonstrate how I have followed through in this task.

¹⁷ Original text: “Die Identifikation der Daten für eine Diskursanalyse ist deshalb ein eher offener Suchprozess in verschiedene Richtungen, der sich immer nur vorläufig an Themen, Referenzphänomenen, Schlüsselbegriffen usw. orientieren kann. Denn ein wesentliches Ziel der Diskursforschung ist ja gerade die Beantwortung der Frage, welche(s) Wissen, Gegenstände, Zusammenhänge, Eigenschaften, Subjektpositionen usw. durch Diskurse als ‘wirklich’ behauptet werden, mit welchen Mitteln – etwa Deutungsschemata, Klassifikationen, Phänomenstrukturen, story lines, moralische und ästhetische Wertungen – dies geschieht, und welche unterschiedlichen Formationsregeln und -ressourcen diesen Prozessen zugrunde liegen.”

III. Overview of the Two Journals

III.1 Science, Technology, & Human Values

The journal *Science, Technology, & Human Values* (STHV) emerged out of a newsletter project, which was started in the early 1970s under the name of *Newsletter of the Program on the Public Perceptions of Science* (1972-1976). In 1976, the name was changed to *Newsletter on Science, Technology, & Human Values*, with the first part of the name being dropped in 1978 (Blanpied, STHV 1982:3). For the entire period I am analysing here, the journal was published four times a year (a switch to bi-monthly publication was made in 2006). However, making any general descriptive statement about the journal covering this entire period is extremely difficult – it makes more sense to distinguish between two main periods, the years 1980-1987 and the years 1988-2005. 1988 is an important turning point for the journal on several counts: Not only did the journal appear in a new format and layout and was issued by a new publisher, it also became officially affiliated with the *Society for Social Studies of Science* (4S) and joined forces with another journal, *Science & Technology Studies*. Finally, 1988 was also the year that Susan Cozzens took over editing duties from Marcel C. La Follette.

Up until 1988, the origins of the journal as a newsletter were very much visible in the character of the journal. Besides printing articles and commentaries, *STHV* also devoted a lot of space to printing summaries or excerpts of reports published elsewhere, historical documents, papers or discussion excerpts from workshops and conferences, and so on. Apart from that, there were also news items (covering anything from announcements about conferences and fellowships to accounts of new public understanding of science initiatives or calls for a coordination of research activities of researchers working on a particular issue), a bibliography section (which compiled a list including ordering info and short descriptions of publications deemed relevant for *STHV*'s readership, more in catalogue-than review-like style), and a meetings calendar (covering conferences in the social sciences, humanities and natural sciences, as well as more policy- and action-oriented symposia). The articles section was also quite diverse, with plenty of very short articles thrown in – in fact, the line between articles and comments (and sometimes even between articles, comments and historical documents) was a rather blurry one. What's more, the

authors of the articles not only came from disciplines as diverse as political science, history, sociology, political economy, law and public policy analysis, but there were also a number of articles written by authors from policy or industrial sectors. In 1983, Marcel C. La Follette (editor of *STHV* throughout the entire 1980-1987 period¹⁸) described the journal as “an unusual scholarly ‘meeting place’ where those who work in the sciences and those who study the humanities, where professionals, practitioners, and policymakers can engage in joint consideration of contemporary problems” (La Follette, *STHV* 1983:1, p.4), featuring a “mix or refereed scholarly articles, pertinent research reports, excerpts from primary documents, lively commentaries, news, and our unduplicated ‘Bibliography’ section” (ibid.). These quotes nicely sum up that *STHV* during those years was something fairly different from a standard scholarly publication fostering discussion in an (more or less clearly delineated) academic community. In another editorial published three years later, La Follette described *STHV* as “a communications channel between the academicians and the practitioners – public and private – in science and engineering policy” (*STHV* 1986:4, p.2), but “not necessarily a traditional scholarly journal, either in content or format, but rather something more like a magazine” (ibid.). In the same editorial, La Follette stressed that the journal intended to be “useful to those who must advise and must decide” (ibid.). The articles should thus be written in a language that is “clear, crisp, and intelligible to all its readers – regardless of disciplinary training” (ibid.) and devoid of “the mantle of academic rhetoric and intellectual pomposity” (ibid.).

An important institutional change that occurred during the 1980-1987 period was the switch to a new publisher, John Wiley & Sons, in 1983. Before that, the journal had relied on “grants from private foundations and the Federal government” (La Follette, *STHV* 1983:1, p.3) and had been co-owned and sponsored by the Program in Science, Technology, and Society at MIT (La Follette’s institution) and the Kennedy School of Government at Harvard University. This institutional affiliation with the MIT and Harvard University carried on until *STHV* officially became the journal of the *4S* society in 1988. Considering this affiliation with two American universities, coupled with a strong interest

¹⁸ LaFollette was not, however, the first editor of the journal: The original editor of the newsletter was William Blanpied (1972-1975), who was followed up by Vivien Shelanski (1975-1978). Marcel LaFollette took over her editing duties in 1977, thus roughly around the same time the publication made the change from newsletter to journal (Holton, *STHV* 1982:3).

in being relevant to public policy and to policymakers (which, more often than not, meant concentrating on the policy of one particular country or at the very most a comparison of two countries), it perhaps comes as no surprise that these early issues of *STHV* had a very strong focus on the USA, both in the sense that most authors were located in the United States and in the sense that many articles dealt exclusively with domestic American issues. While the desire to change this and to widen the “journal’s international perspectives, encouraging more contributions and advice from colleagues outside the United States” (La Follette, *STHV* 1983:1, p.3) was already expressed in 1983, this situation did not change overnight.

As already mentioned, some rather drastic changes took place in 1988. After being a university-sponsored publication for more than a decade, *STHV* became the official journal of the *Society for Social Studies of Science* (4S) in 1988. With this change came a change of publisher (Sage Publications, which still publishes the journal today), of editorship (Susan Cozzens became editor of the journal and remained so until 1993), of format (a reduction in size by about half) with an accompanying change of layout, as well as a merging with another journal, *Science and Technology Studies*.

These changes also roughly mark the period of a gradual shift towards more ‘standard’, longer academic papers rather than the newsletter-like conglomeration of news items, information about new publications, and updates about upcoming conferences and meetings (although it should be noted that these three sections of the journal already disappeared a year earlier than the aforementioned changes, thus in 1987). In the course of this shift towards longer academic papers, abstracts at the beginning of full-length articles started to be included in 1989. Editorials gradually became more rare: Whereas nearly every issue in the 1980s started with an editorial (introducing the issues dealt with on the following pages, positioning the journal, or simply commenting on current events, e.g. science policy matters), only very few editorials were published since the early 1990s, usually announcing major changes. Apart from that, only special issues or special sections were preceded by editorial material (usually under the heading of ‘Introduction’), but these introductions were generally published under the name of a guest editor and related quite specifically to the articles compiled in the issue.

The new affiliation with *4S* also brought in new formats, however: From 1989 to 1999, nomination and acceptance speeches for the J.D. Bernal Prize (issued by *4S* for distinguished contributions to the field) were printed in the journal. Apart from that, various keynote speeches and presidential addresses from *4S* conferences were also published occasionally. In 1988, even the entire collection of abstracts for the joint conference of *4S* and its European sister society, the *European Association for the Study of Science and Technology* (EASST) was re-printed in *STHV*.

The 1988-2005 period saw four different editors of *STHV*: Susan Cozzens (Rensselaer Polytechnic Institute, USA, 1988-1993), Olga Amsterdamska (University of Amsterdam, the Netherlands, 1994-1998), Ellsworth R. Fuhrman (Virginia Polytechnic Institute and State University, USA, 1999-2002), and Ulrike Felt (University of Vienna, Austria, 2002-2007). This rather frequent change of editors is not a coincidence, nor an indicator that the editors did their job badly or wanted to get rid of it again after a short while, but rather a conscious decision on the side of *4S* against having any one person in charge for more than four or five years. Susan Cozzens described how it came to this decision in a 'Letter from the Editor', announcing that *STHV* was looking for a new editor, in 1991: "Two years ago, the *4S* Council faced the question of succession of editors for the first time. At my request, they reappointed me to a five-year term (ending in 1993), bringing my total *4S* editorship to eight years. They also set the regular term of the editor at three years and rejected a proposal to rotate the editorship on a three-year basis. At the time, however, it seemed to me that rotation on some basis was appropriate in an organization like *4S*. The editor's term should be long enough to allow him or her to learn the job and use the acquired skills and knowledge, but not so long as to identify the journal with an individual rather than the society. I therefore resolved to step aside at the end of my five-year term and allow the regular process of replacing the editor to begin." (Cozzens, *STHV* 1991:2, p.264)

In the editorials after the new liaison with *4S*, Cozzens mostly stressed that the journal should continue to cater to a large and varied group of authors and readers: "From its inception, this journal has been multidisciplinary; that approach will continue. Scientists and engineers, raising fundamental ethical and values issues in their professional societies and political organizations, have an important role to play. Equally necessary are the

contributions of social scientists and humanists, whose professional perspectives challenge and benefit from those of practitioners. *Science, Technology, & Human Values* remains committed to helping all these voices speak and be heard in their distinctive ways.” (Cozzens, *STHV* 1988:1/2, p.6)

However, Cozzens also emphasised that the new role of the journal meant that it had to “reflect the full diversity of the Society” (Cozzens, *STHV* 1989:3, p.227), while also, as far as possible, seeking to stimulate “interaction across a wide range of perspectives and extending the scope of effective participation in the field” (*ibid.*). She went on to highlight the importance of diversity in geography, in methodology and approaches, and along the descriptive/prescriptive dimension. While none of these types of diversity are in explicit contradiction with the aim of giving space to the perspectives and insights of practitioners and policymakers, it should nevertheless be mentioned that they were no longer explicitly put on the agenda, either. Nor would the importance of bringing together those who study science with those who do or police science, which had played such a central role in the presentation and identity of the journal throughout much of the 1980s, be stressed in the editorials of the following years.

III.2 Social Studies of Science

The journal *Social Studies of Science* (SSS) was co-founded in 1971 by David Edge and Roy MacLeod, originally under the name *Science Studies*, and had its name changed to SSS in 1974. This change of name also went hand in hand with a change of publisher: While *Science Studies* had been published by MacMillan, the newly named SSS was (and continues to be) published by Sage Publications (Edge, SSS 1991:2, SSS 1996:4). Overall, the history of the journal is marked by far less radical changes than that of *STHV* – a fact that is probably closely linked to the remarkable stability of the editorship of the journal: It was (co-)edited by David Edge and Roy MacLeod from its very inception up until 1991, when MacLeod resigned from his position. Edge, on the other hand, continued to edit the journal for another 11 years – Michael Lynch took over the position of editor in 2002, only a few months before David Edge’s death in January 2003. While the 1991 editorial which announced MacLeod’s resignation from the editorship mentioned that the editor would be appointed on a three-year basis (Edge, SSS 1991:2), later editorials made no remarks about the circumstances of Edge’s reappointment; the fact that he continued to edit the journal

did not appear to be remarkable or unexpected enough to require a special mention. When Edge announced his retirement from his position at the Science Studies Unit in Edinburgh a year later, thus concentrating on the journal full time, he declared his intention to “continue in this Editorial Chair for as long as our publishers and their advisers allow” (Edge, SSS 1992:2, p.212).

While originally, the journal had been (at least officially) equally edited by Edge and MacLeod, only Edge was credited as ‘editor’ after MacLeod’s move to Australia in 1983, whereas MacLeod was now referred to as ‘co-editor’. As the (co-)editors announced with a touch of irony, “we have thought it desirable to counterbalance the altered axis of the journal by stressing the primacy of meridional Britain. Henceforth, readers will note that David Edge will be styled ‘Editor’ and Roy MacLeod ‘Co-editor’ [...] Edinburgh will be our ‘Clapham Junction’” (Edge/MacLeod, SSS 1983:1, p.i).

It is probably difficult to overestimate the importance of the role David Edge played for the development and character of the journal, considering how much time and dedication he devoted to it and how much effort he put into the right placement of every comma (as almost everyone who wrote an obituary for Edge pointed out in their reflections upon his work). As Michael Lynch put it in the editorial published after the death of David Edge: “Without question, he was the major source of the effort and inspiration that enabled *Social Studies of Science* to become the flagship journal in its field. Indeed, David and the journal helped establish and shape the field of science and technology studies.” (Lynch, SSS 2003:1, p.5)

This stability of editorship contributed to accusations that SSS was the ‘house journal’ of one particular school of thought within STS – the ‘strong programme’ of SSK or the so-called ‘Edinburgh school’, or more broadly speaking, constructivism in general. While Edge made no effort his association with this ‘school of thought’ – while responding to a letter to the editor in order to clear up some misunderstandings about constructivism, he concentrated on showing how the ‘strong programme’ differed from the accusations brought up by the reader, only to “apologize profusely to regular readers for the fact that this Editorial is so defensively ‘Edinburgh-centred’” (Edge, SSS 1996:4, p.732) and to point out that he would be happy to publish similar cases from other strands of

constructivism, but he himself “can only write about what I know” (ibid.) – he did go to great lengths to convince his readers that this did not mean that only papers subscribing to this particular school would be considered for publication.

The editorial cited above is not the only instance where the editors of *SSS* became defensive about the claim that the journal was primarily associated with a particular school of thought. In another editorial published ten years earlier, Edge and MacLeod faced similar charges. Summarising some of these criticisms that had been directed at the journal, they tried to set some “widely held, and frequently misleading, images” (Edge/MacLeod, *SSS* 1986:1, p.4) straight: “One is the belief that *Social Studies of Science* is the ‘house journal’ of just one subgroup of the whole field – those engaged in contemporary and historical studies in the sociology of scientific knowledge, for instance, or perhaps ‘relativists’ in general. Others believe that the journal serves the interests of the Europeans, or of the British – or even of the Scots. Others detect an Anti-American bias, or a tendency to reject policy-related studies. Readers can form their own opinions as to the realism and justice of these beliefs simply by inspecting the journal itself. If we are to be hanged, let it be for the right reasons!” (ibid.)

Later in the same editorial, Edge and MacLeod stressed, “in our house there are many mansions. We do not wish to see *Social Studies of Science* become the ‘house journal’ of *any one* network, with the work of others systematically excluded.” (ibid.) However, they concluded, perhaps *SSS* should become the ‘house journal’ of several, conflicting research networks. Indeed, they added: “Perhaps it already is, for certain sections of the sociology of scientific knowledge.” (ibid., p.5) The possibility that this might be exactly what the critics mentioned earlier criticised in the first place was not discussed by the editors. While the editors thus rejected the notion that only articles following the tenets of a particular school of thought would get published in *SSS*, David Edge did stress that some kind of reference to current theoretical debate in the field should be made in the publications: “We vigorously affirmed that we are *not* interested in perpetuating any ‘orthodoxy’. On the other hand, we also clearly agreed that any paper submitted to this journal should address its empirical material within *some* current context of theoretical development and debate; and that, if it departed significantly from what has been, to date, a ‘dominant’ approach, readers were owed an *apologia* – some short discussion of the present literature, perhaps, ‘placing’ the contribution in our readers’ realm of discourse, and signalling and justifying

the particular ‘position’ chosen.” (Edge, *SSS* 1996:1, p.5f; emphasis in the original) Even then, Edge stressed, this does not refer to any particular school within several lively social science traditions: “We happily welcome papers set in any current and lively ‘social science’ tradition, and will do our best to offer fair scrutiny to any submissions which have such serious intent.” (ibid., p.6)

Edge’s successor, Michael Lynch, also emphasised the need for *SSS* “to resist the tendency to associate itself (or be associated by others) with a narrow disciplinary programme, theoretical orientation or research network” (Lynch, *SSS* 2002:1, p.6). It is perhaps interesting to note that Lynch implicitly confirmed that such a tendency does indeed exist – a point which he made more explicit in another editorial two years later: When discussing the sometimes ambivalent relationship between the journal and the legacy of Robert Merton, he conceded that “this journal had strong association (in terms of place and perspective) with University of Edinburgh’s Science Studies Unit and the Strong Programme in the sociology of scientific knowledge (‘Strong’ in contrast with, among other programmes, Mertonian sociology of knowledge).” (Lynch, *SSS* 2004:6, p.828) Notice how Lynch presented this association as a thing of the past, of Edge’s editorship. In doing so, he implicitly confirmed some of the criticism that has been directed at the journal in the past.

One difference between *STHV* and *SSS* is of a geographical nature: While *STHV* was edited in the United States until 1993, the original editors of *SSS* were located in Scotland (David Edge) and England, later Australia (Roy MacLeod, who held a position at the University of Sussex when the journal was founded, moved on to London a few years later and held a position at Sydney University from 1983 until after the end of his editorial activities). The current editor, Michael Lynch, is at the University of Cornell in the United States, with two collaborating editors in the UK and one in Canada as well as in the USA. While *SSS* is thus less strongly focused on the United States than *STHV* was until 1993 (with stronger institutional ties particularly to the UK), *STHV* has established some strong institutional ties with Continental European academics, and thus scholars outside the English-speaking world, in the last 12 or 13 years (two of the three editors who were in charge of the journal between 1993 and 2005 were located in Continental European countries). *SSS* does of course also welcome publications from authors in other countries, and has several editorial

advisors in Western Europe, but the editor and collaborating editors are all located at universities in English-speaking countries. In both journals, editorial contributions from European countries are generally limited to Western Europe, while ties to South American, African or Asian countries are virtually non-existent (the only exception being an editorial advisor to *SSS* from Venezuela).

On the topic of geographical distribution and orientation, it is also interesting to mention an ‘experiment’ conducted in 1983, when the editors of *SSS* decided to publish a paper in German, giving the following explanation: “Since we are advised that the sense of the material quoted in this paper would almost certainly be seriously distorted by translation into English, we have agreed to its publication in German. We hope that an occasional paper in a Continental language will be appreciated by our European subscribers, and others, and invite readers’ reactions to this experiment.” (Bayertz, *SSS* 1983:3, p.355)

This experiment is interesting in two respects: Firstly, it indicates an orientation towards the Anglo-American and Continental European parts of the world – a paper which is published in a language other than English is meant to appeal to the ‘European subscribers’ (with an ‘and others’ added as an afterthought), not to those whose native language is not English in general, nor to those who understand German specifically. The possibility of publishing in a language that is neither English nor a ‘Continental [European] language’ is not mentioned. Secondly, it is interesting because it remained a one-off experiment. While there is no way of telling what the readers’ reactions asked for in the editorial note were (or, indeed, if there were any), an editorial three years later stated that there would not be any papers printed in languages other than English in the future, although there would be increased efforts to assist authors whose native language is not English: “One major problem we have faced is that of language barriers: these tend to consolidate other boundaries between networks. We have decided to abandon our attempts to *publish* in languages other than English. However, we intend to increase our ability to *assess and edit* papers in four other languages – French, German, Italian and Spanish. The final version of such papers will be published in English translation. [...] If the experiment is successful, we may be able to extend it to other languages.” (Edge/MacLeod, *SSS* 1986:1; italics in the original) Whether such an extension ever took place remains unclear; at any rate, it was not mentioned in another editorial again.

Unlike in *STHV*, abstracts for the main articles in *SSS* are available for the entire period of analysis, 1980-2005, as they were already introduced to the journal in 1977 (in a separate section for abstracts during the first year, then at the beginning of each paper). Like *STHV*, *SSS* also upgraded from four to six issues a year, but they did so almost ten years earlier, in 1997.

Unlike *STHV*, which – especially up until the late 1980s – positioned itself as a meeting place between science studies and science policy and explicitly did conceive of itself as a pure scholarly journal, *SSS* has always clearly been a scholarly publication, with a focus on publishing original research. However, some different formats were introduced that served a similar purpose as the mix of different article formats in *STHV*, if not to the same extent. In 1983, the purpose of the categories of ‘Discussion Papers’, ‘Notes and Letters’ and ‘Course Bibliographies’ was explained as follows: “[W]e plan to build on our special strengths: empirically-based studies, both contemporary and historical, in the sociology of scientific knowledge, and in the political economy, political sociology and social history of science and technology. [...] While we remain unequivocally a scholarly journal, we have no intention of ignoring those critical issues that shape our time. To this end we will continue to cultivate fresh contributions in our special categories of ‘Discussion Papers’, ‘Notes and Letters’ and ‘Course Bibliographies’, and from time to time, ‘Special Issues’.” (Edge/MacLeod, *SSS* 1983:1)

Here is a very noticeable difference between the two journals around the early/mid-1980s, then: Whereas in *STHV*, the desire to pick up and discuss themes of societal relevance was what seemed to drive the entire journal (and certainly formed the central subject in the *STHV* editorials of this period), it appeared to be more of an also-ran for *SSS* – something that can and should be discussed in various (often short) special formats in the journal, but parallel and in addition to the real focus of the journal, which was the publication of empirical scholarly papers.

I have already discussed some differences and commonalities between the two journals, but it also pays to look at if and how the relationship between them is presented in the journals themselves. When Ulrike Felt, editor of *STHV* at the time, was appointed as an

editorial advisor for *SSS* in 2004, this was announced as a step “in order to enhance cooperation and co-ordination between the journals” (Lynch, *SSS* 2004:2, p.148). While it was not specified what this coordination would look like exactly (or whether, indeed, it was largely a symbolic gesture), similar – yet more concrete – considerations had apparently been dismissed nine years earlier when a joint meeting of the *4S*, the *History of Science Society* (HSS) and the *Philosophy of Science Association* (PSA) served as a meeting place between the editorial teams of both journals. After some reflections on what distinguished an *SSS*-paper from an *STHV*-paper, a question to which no clear answer could be found (more on this below), David Edge discussed and ultimately dismissed the possibility of a coordination between the two journals: “[B]y ducking the challenge of drawing boundaries, we run the risk of encouraging simultaneous, overlapping submissions. The notion of trying to minimize this risk by some informal liaison between two related journals was firmly rejected in our discussions, as both potentially dangerous and unworkable.” (Edge, *SSS* 1995:1, p.6)

The fact that such an arrangement was deemed ‘unworkable’ might in fact be related to the blurry boundaries between the two journals, as an editorial from nine years earlier implied that a similar (if perhaps less strong) working agreement exists or used to exist between *SSS* and *Scientometrics*. After stressing that *SSS* was happy to consider publication of papers coming from *any* tradition in the social studies of science, Edge and MacLeod went on to state: “The only exception to this rule concerns work which emphasizes quantitative methods; since the advent of *Scientometrics*, we have had a friendly but informal arrangement whereby we referred to that journal those contributions which seem to us to be of principal interest of that network (of which *Scientometrics* is the *soi-disant* house journal). Even in this area, however, we still publish papers in which such methods play a central role, or which discuss the status of quantitative techniques” (Edge/MacLeod, *SSS* 1986:1, p.5; italics in the original).

The reason that this informal agreement was considered ‘workable’ seems to be related to the relatively well-defined boundaries drawn between the two journals in question, which made it easier to determine where a given submission would be a ‘better fit’. While the question of where to draw this boundary between *SSS*-papers and *STHV*-papers has been given thought by the editors of both journals, neither came up with any clear distinction. The issue was first raised in an *STHV* editorial in 1989, in which Susan Cozzens (*STHV*

1989:3) mentioned this as one of the questions posed to her at a joint meeting of the *4S* and *EASST* societies. Cozzens provided no clear answer, instead writing a few paragraphs about what kind of journal *STHV* was supposed to be, without relating it to the profile of *SSS*. Presumably, she had no precise answer, either. The difficulty of answering this question was made more explicit a few years later in an *SSS* editorial: “First, we pondered a question that both I and Olga [Amsterdamska, then editor of *STHV* - AS] are frequently asked: what (if anything) distinguishes an ‘*SSS*-paper’ from an ‘*ST&HV*-paper’? Can we offer guidelines to confused authors, uncertain where to submit their papers? Some ingenious attempts to draw such boundaries were made, but the overall consensus was clear: almost everyone agreed that it was both impossible and unnecessary to try to make such distinctions with the kind of clarity that would be needed. Each journal in our field must make its own profile and standards as clear as possible, and then let things flow where they will.” (Edge, *SSS* 1995:1, p.5)

Since the general observation can be made that the two journals have over time become more similar in character, it is also interesting to investigate whether these increasing commonalities are reflected in the organisational structures of the two journals – specifically, whether and to what extent the editorial boards of the journals overlap. In order to establish this, I have taken a sample in which I compared the editorial boards as printed in the first issue of every even year (e.g. the first issue of 1980, of 1982, etc.). Since most people tend to stay on board for a relatively long time, it seemed justified to me not to compare the boards as printed in each and every issue, but rather to take a sample in intervals of two years.

The result of this comparison was that there is indeed a noticeable increase of people who in one way or another serve on the boards of both journals over time. While the overlap in 1980 and 1982 was only one person (Nicholas Mullins serving as a consulting editor to *STHV* and as an editorial advisor to *SSS* in 1980, and Loren Graham serving as an editorial advisor to both journals in 1982), the number of people associated with both journals has been slowly increasing since the mid-80s. In the last year that I considered in my comparison, 2004, there was an overlap of 10 persons. For sure, this increase is also related to the fact that both journals have expanded the size of their respective boards (as they

have expanded in number of issues published each year and page numbers), especially where the (largely symbolic) position of editorial advisors is concerned¹⁹, thus making overlap more likely, but it still seems indicative of a coherence of the field, or at least of a convergence of the two journals, that both publications take on collaborators from overlapping pools of scholars. With the inclusion of Ulrike Felt among the editorial advisors of *SSS* in 2004, the editors of both journals were each part of the board of the other journal: Michael Lynch, editor of *SSS*, has been part of the *STHV* board since before he took over editorship from David Edge (and remains so), while Ulrike Felt became editorial advisor of *SSS* while already serving her period as editor of *STHV*.

¹⁹ Just to give an impression of the scales we are talking of: In 1980, the editorial board of *STHV* consisted of 23 scholars (1 editor, 5 consulting editors, 17 editorial advisors; there was also one production editor and 3 editorial assistants listed, but these appear to be purely administrative/technical positions). In 2004, 37 persons were listed (1 editor, 1 managing editor/book review editor, 9 contributing editors and 26 editorial advisors). Similarly, in 1980, 22 persons were listed as collaborators to *SSS* (2 editors, 5 collaborating editors, 15 editorial advisors); by 2004, the number has increased to 36 (1 editor, 4 collaborating editors, 1 reviews editor, 30 editorial advisors).

IV. Searching for an Object

In linking their own work to a particular kind of discourse or disciplinary research context, many authors refer to the relevant field using a format of ‘sociology/ philosophy/ social studies/ etc. of ...’. This way of naming or labelling a field suggests that the first part of the phrase defines a particular disciplinary approach, while the second part makes reference to a particular object of study. It is the choice of this *object* of scholarly analysis that I will have a look at on the following pages, while the disciplinary approach adopted will concern me in chapter V, ‘Searching for a Discipline’. By analysing this issue, I attempt to show that there is nothing self-evident about the object of analysis of STS and thus call into question Mario Biagioli’s (1999) assertion that STS (or science studies, as he denominates it) need not concern itself with the question of how to define its object, as such a definition allegedly comes pre-packaged and is in no need of further elaboration.

IV.1 ‘Science’ as an Object of Analysis

Unsurprisingly enough, considering that the word ‘science’ appears in the title of both of the journals I am investigating, for the vast majority of authors publishing in these two journals, ‘science’ is an important object of analysis throughout the entire period of my analysis. From various disciplinary approaches, authors approach ‘science’ as their main object of analysis – but of course, this raises the questions of what they find worthy of investigation about science and which aspects they have in mind when they set out to study it. While the differences have become much more blurry in the last one and a half decades, during the 1980s, the focus in *SSS* was especially (although certainly not exclusively) on laboratory sciences, while *STHV* was more concerned with sciences in their context of application, e.g. in the policy sector. The focus of inquiry was different, too – while many authors in *SSS* focused on epistemic aspects of scientific knowledge, many articles in *STHV* put more emphasis on institutional aspects of the scientific enterprise, as well as on the role of science in a social and economic context. For both journals throughout the 25-year period I have analysed, ‘science’ predominantly means ‘natural science’, but several exceptions can be found; in *SSS*, these exceptions where non-natural science disciplines are dealt with are more common than in *STHV*. In particular, there is a fairly strong tradition in *SSS* to publish research on psychology, especially in the context of publication or citation

analyses – be it an analysis of whether it makes a difference if published data or data from curricula vitae are used to charter academic careers (Heinsler/Rosenfeld, SSS 1987:1), of author judgements about the works they cite (Shadish et al., SSS 1995:3), of the view of knowledge presented to students in introductory textbooks (Smyth, SSS 2001:3), or of the role of readability for the impact of a paper (Hartley et al., SSS 2002:2). An application of Latour’s notion of graphism, which links the use of graphs in scientific literature to the hardness or softness of a scientific discipline, also goes into detail with an analysis of psychological papers in order to analyse the same pattern within different specialties of one discipline (Smith et al., SSS 2000:1). While all of these papers take for granted that psychology can be studied with the methods and concepts of STS and that such analyses fall well within the scope of the science studies field, and indeed refer to the publications they investigate as exemplars of scientific writing and to practitioners of psychology as scientists, one article stresses the differences between psychology and the natural sciences in regard to the way scientific knowledge is presented in textbooks, presenting psychology as an ‘uncertainty science’, which “continues to problematize its statements while biology does not” (Smyth, SSS 2001:3, p.390). Even less clear is the status of psychoanalysis, the scientificity of which is heavily contested – a topic which is taken up by Thomas Osborne (SSS 1993:1), who points out that it is impossible and unreasonable to give a general answer to the question whether or not psychoanalysis constitutes a science. On a related note, the status of parapsychology as a ‘deviant science’ is also given scholarly attention in two articles (Gordon, SSS 1982:1; Hess, SSS 1987:3).

These examples notwithstanding, many authors dealing with human or social sciences in their articles appear to feel the need to justify that their research falls within the scope of STS. The authors of a study on survey interviews go so far as to “propose a Sociology of Social Scientific Knowledge (SSSK) along the lines of the Sociology of (Natural) Scientific Knowledge (SSK)” (Maynard/Schaeffer, SSS 2000:3, p.324), thus clearly indicating that they consider traditional STS/SSK research to deal exclusively with the natural sciences. One author dealing with economical disputes (Yonay, SSS 1994:1) explicitly positions his work in the context of similar controversies in the natural sciences, going to lengths to show the parallels between the natural and economical sciences; another author who deals with economics and finance sets out to “argue for an end to the neglect of finance theory and financial engineering by science studies and to encourage the

intensification of the current awakening of interest” (MacKenzie, STHV 2001:2, p.117) – indeed, in another article two years later, he already finds that “[e]conomics and economies are becoming a major focus for social studies of science” (MacKenzie, SSS 2003:6, p.831), which still implies that at some point, they were not. Even an article on archaeology refers to the “special nature of archaeology as a discipline. Evidence in archaeology is not like evidence in experimental sciences, for luck plays an enormous part in whether there is any evidence at all. Researchers cannot easily design experiments and thereby will data into existence the way particle physicists or molecular biologists can.” (Fahnestock, STHV 1989:1, p.27) Elisabeth Clemens also draws a category of ‘nonscience’, which she defines as “a residual category encompassing anything from magic to sociology” (Clemens, SSS 1986:3, p.421). The experimental natural sciences are thus taken as a ‘natural’ reference point, against which all other disciplines are to be compared. The ‘experimental’ is a key term here – while physics is one of the core disciplines traditionally given research attention in STS, even studies of *theoretical* physics seem to require special justification: In an article dealing with an ethnographic study of theoretical physicists, Martina Merz and Karin Knorr Cetina find that where the theoretical work of scientists is concerned, “students of science usually look the other way” (Merz/Knorr Cetina, SSS 1997:1, p.73) and thus set out to wonder whether “these sort of operations [are] still within the scope of our interest? More important, are they even within our reach?” (ibid.). Daniel Kennefick also points out that so far, there has been very little empirical work on theoretical physics, in spite of the fact that it is widely cited “as an exemplar of how science works” (Kennefick, SSS 2000:1, p.5).

What all of these articles share is the view that at the core of STS research lies the analysis of experimental natural science; many of them also argue explicitly in favour of an extension of STS into other domains of scientific knowledge, thus extending the definition of ‘science’ beyond these narrow terms. It seems telling that the two most systematic of such endeavours have been made for psychology and economics, both disciplines that hark back to a strong tradition of working with mathematical models and that display an affinity towards experimental research methods. Of course, several articles also refer to a wider definition of science: In his article on the Dutch science shops, Joseph Wachelder talks of science “in its widest meaning, thus including the humanities and the arts” (Wachelder, STHV 2003:2, p.244f.). It is this wider meaning that many of the authors discussed above

have in mind when they consider the social sciences as an object of analysis for STS; but the fact that they explicitly justify such a move indicates that it can by no means be taken for granted that such research falls within the scope of STS research.

So far, I have consciously not included articles that deal with the state of STS itself for this analysis, but such self-reflection also gives some interesting indications about the definitions of 'science' held by the authors. While Susan Cozzens (SSS 1985:1) sees no problem with using literature from the sociology of science for a comparative study of citation context between two different disciplines (comparing a paper from the sociology of science with one from the field of neuropharmacology), Steve Woolgar also counts the sociology of scientific knowledge as a science, but points out that this characterization is "contentious" (Woolgar, STHV 1991:1, p.21). In a discussion between Michael Lynch and Simon Cole, they both agree to personally "fall on the side of the view that social science is not really science" (Lynch/Cole, SSS 2005:2, p.285). This statement is made in the context of a debate about how Cole should have presented his own expertise in front of a court, and specifically, how he could have dealt with the question of whether he is a 'scientist' or not. Upon probing by an attorney, Cole had presented himself as a historian rather than a scientist, referring to himself as "not a scientist in the sense of a laboratory scientist" (Lynch/Cole, SSS 2005:2, p.284). Marion Hercock, too, talks of "boundaries (real and perceived) between science and social studies of science" (Hercock, SSS 2003:1, p.117), although she also explicitly aims to cross these boundaries, and feels that as a "geographer, who is both a 'physical' scientist and a social scientist" (ibid.), she is in a good position to do so. In summary, while there are conflicting views of whether STS or social science in general *should* be considered under the definition of 'science', these self-reflective articles tend to start from a standard definition of 'science' as meaning primarily the experimental natural sciences.

Many articles make reference not to 'science' in general, but rather to particular academic fields, ranging from geology to computer science, sometimes even to the social sciences or humanities. Some of the authors refer to these highly specialised fields as academic disciplines in their own rights – for example, Rob Kling concludes his article with "observations about the sociology of computer science as an academic discipline" (Kling,

STHV 1991:3, p.342). In the same way, some articles reference only a particular aspect of science, for example by referring to work on the sociology of Big Science or on Public Understanding of Science.

The terms ‘knowledge’ or ‘scientific knowledge’ are also employed quite frequently and might, at first glance, appear to have a rather similar meaning as the term ‘science’. A more thorough look at how these terms are used, however, displays that they do not simply mean the same thing. The substitution of ‘science’ by ‘scientific knowledge’ can be understood as a deliberate attempt to move the contents of science, rather than just the social settings of those who do science, to the centre of analysis. According to Steve Woolgar, there has been a move away from “a mere sociology of scientists”²⁰ and into a direction where “the very content of scientific knowledge” (Woolgar, SSS 1982:4, p.481) is analysed.

The term ‘... of knowledge’ or ‘...of scientific knowledge’ is most commonly used in reference to the sociology of scientific knowledge, often abbreviated as SSK. This is a peculiar term because, while, taken at face value, it simply seems to indicate a sociological approach to the contents (rather than purely the social structures) of science, there is also a rather different meaning to be found behind these words: The term has come to be quite closely linked with one particular approach or theoretical school within the field of STS or sociology of science – that is, with the so-called ‘strong programme in the sociology of scientific knowledge’, harking back to the works of David Bloor²¹. Thus, while there are

²⁰ Woolgar associates such a ‘sociology of scientists’ with the classics of sociological theory, such as Robert Merton’s (1985) account of the structure and workings of science, which rests upon a scientific ethos (and specifically, on the scientific norms of communalism, universalism, disinterestedness and organised skepticism) shared by the members of the scientific community. While Merton thus factors out the actual contents of scientific knowledge and explains science in terms of the norms shared by its practitioners, Woolgar describes a move towards analysing precisely those contents of knowledge.

²¹ For those unfamiliar with the central tenets of the ‘strong programme’, a short explanation may be in order, especially as it has already been mentioned in the description of the development of SSS (whose long-time editor, David Edge, was a colleague of David Bloor’s at the Science Studies Unit in Edinburgh) and will be referred to again in various instances in this thesis: In his book *Knowledge and Social Imagery*, originally published in 1976, David Bloor sets out to develop a sociology of knowledge that can “investigate the very content and nature of scientific knowledge” (Bloor 1991, p.3). Contrary to previous sociological approaches to the study of science and knowledge, which use social factors to explain only the existence and persistence of knowledge and beliefs that are in contradiction to current scientific orthodoxy, Bloor calls for a more ‘symmetrical’ treatment. Outlining four tenets for the sociology of knowledge, he argues that a sociological explanation should be 1.) interested in the causal explanation of beliefs, 2.) impartial with respect to truth and falsity, 3.) symmetrical in its style of explanation, using the same types of explanation for true and false

many articles that appear to take the term literally and use it to delineate a field of enquiry, there are also those that, explicitly or implicitly, associate the term with one particular theoretical school. For example, when Scott, Richards and Martin discuss common tenets of “both traditional positivist approaches and also [...] the sociology of scientific knowledge” (Scott et al., *STHV* 1990:4, p.474), it is quite clear that by ‘sociology of scientific knowledge’, they do not just mean the sociology which concerns itself with issues of scientific knowledge – if that was the case, positivist approaches would be part of the field, rather than something to be pitted against SSK. Indeed, one page further into the article, our suspicions are confirmed: here, the authors discuss the strong programme’s (and, in particular, David Bloor’s) criticism of positivist approaches. In this instance, thus, the term ‘sociology of scientific knowledge’ was used to refer to one particular school of thought, rather than to a scientific field or discipline. A counter-example for this use would be one of Bloor’s own writings, in which he does talk of the sociology of scientific knowledge as “an empirical discipline” (Bloor, *SSS* 1996:4, p.839), thus employing the term to delineate an academic discipline rather than one school of thought within a broader discipline – a textbook example of boundary work, considering that this conception of the discipline would almost inevitably centre around his own work. A paper by Hicks and Potter (*SSS* 1991:3) also refers to SSK, along with citation studies, as a ‘field of enquiry’, rather than as a particular school within such a field.

Another approach to which some authors in both journals link their analyses is the so-called ‘laboratory studies’ approach. From the perspective of its advocates, this approach is seen as the logical move when it comes to an empirical study of scientific practice – in the words of Trevor Pinch, it offers “detailed accounts of the ‘nuts and bolts’ of scientific activity” (Pinch, *SSS* 1985:1, p.3). Unlike the term ‘SSK’, which, as I have pointed out, is used by some authors to delineate a scientific field, while others use it to refer to a particular school of thought, ‘laboratory studies’ carries no such ambivalence. It is used exclusively to mark a particular focus of analysis, not to refer to a scientific field in its own right. In the instances of authors referring to ‘laboratory studies’ which could be found in the analysed articles²², the authors either refer to ‘laboratory studies’ as a sub-field of

beliefs, and 4.) reflexive, in that its patterns of explanation should have to also be applicable to the sociology of knowledge itself.

²² For what it’s worth, these instances tend to appear in the journal *SSS* – while there are, of course, also

‘science studies’ or ‘social studies of science’, or they present it as the work of sociologists and/or historians.

A similar case is that of authors who, rather than referring to science in general as the object of their study, instead embed their publications into the discourse of analysis of scientific controversies. However, unlike in the case of laboratory studies (where it is always clear that the authors adhere to a laboratory studies approach themselves, but view this as part of a broader academic field), several authors here talk of ‘controversy studies’ (Scott et al., *STHV* 1990:4), ‘studies of scientific controversy’ (Guice, *SSS* 1998:1) or ‘science and technology controversy studies’ (Mitchell, *STHV* 2000:1) as a discipline in its own right. Of course, I am not claiming that these authors see the field of controversy studies as completely unconnected to STS as a whole, nor that they position themselves exclusively in the context of controversy studies throughout their careers or even at a particular point in their careers. They do, however, choose to tie these particular publications only to other analyses dealing with scientific controversies, rather than linking them to a broader discourse on science; the study of controversies is thus given special prominence.

The focus in this account has so far been on science as a cognitive or social activity, but another strand of researching and writing about science deals with science in its contexts of application. Examples of such a focus include the use of scientific expertise in legal settings as well as in policy settings²³ or the discussion of indicators for the evaluation of science (e.g. Morison, *STHV* 1982:1; Brooks, *STHV* 1982:1). Also related to this focus is an interest in research policy and funding²⁴. Of course, these two strands of looking at

articles in *STHV* about laboratories as locus of scientific practice, none of them make reference to laboratory studies as an approach in its own right, although one article (Restivo, *STHV* 1981:2) does mention ‘laboratory life studies’ in a discussion of various theoretical schools within the sociology of science. Arguably, this is no coincidence, but rather a consequence of the need, stated by David Edge (*SSS* 1996:1) and quoted in Chapter III, that authors publishing in *SSS* position themselves within a current, dominant theoretical approach.

²³ Examples of the discussion of scientific evidence in a legal setting include articles by Herlihy (*STHV* 1982:3), Smith (*SSS* 1985:1) and Simon (*STHV* 1985:4); on the other hand, Mullins (*STHV* 1981:4), Katz (*STHV* 1984:1) and Rushefsky (*STHV* 1984:3) are some of the authors that have written about the use of scientific evidence in policy decisions, e.g. in science advisory boards. The predominance of *STHV* articles in this list is no coincidence, even if only selected articles have been mentioned.

²⁴ This focus is stronger in *STHV* (e.g. Caldart, *STHV* 1983:2; Stine/Good, *STHV* 1986:3) than in *SSS* (e.g. Rocca, *SSS* 1981:2).

science (science as an activity or science in its application context) do overlap and intersect, for example where the effects of science funding on science as a cognitive or institutional activity (e.g. Ashford, *STHV* 1983:2; Tatarewicz, *SSS* 1986:1; Gillmor, *SSS* 1986:1) are concerned. A strong line of research, especially from the late 1980s onward, on popularisation activities and the public understanding of science also evades the binary opposition between science in action and science in application, but elaborating on this tradition goes beyond the scope of my thesis.

In summary, I have tried to elucidate the various conceptions of science and ways of tackling it as an object of analysis in the two journals. This includes the discourse about how to delineate science (e.g. does science only mean the natural sciences, or also the social sciences and humanities), where we have seen that most articles use the natural sciences, and especially physics, as their standard model against which to compare all other disciplines, even if not all authors adhere to a definition of science that includes only the natural sciences. Also, some authors prefer to tackle science as a cognitive activity, others as a social/institutional one; some focus on the level of production of scientific knowledge, others on its application, others on the study of scientific controversies. All of these different ways of researching and writing about science also display different conceptions of what science is in the first place, and where STS should put its focus of analysis.

IV.2 'Technology' as an Object of Analysis

So far, I have discussed various labels that authors have applied in order to position their work in an academic discourse on science, but I have not yet touched upon another main object of STS research: technology. While the two journals *SSS* and *STHV* share an expression of interest in scientific issues in their respective journal titles, only *STHV* explicitly professes to deal with technology in its title. Indeed, an analysis paying attention to historical developments of the treatment of technological themes in these two journals shows a rather different development of the two. While technology has always been a central theme in articles published in *STHV*, the same cannot be said for *SSS*. In the early 1980s, technology only appeared as a topic of research in *SSS* in the context of *science-based* technologies or technologies that are employed in the process of doing science, thus

not as a topic in its own right, but rather as an occasional add-on to studies of science²⁵. This gradually started to change in the mid-80s. However, the new topic did not just slip into the journal undercover, but rather through theoretical discussion about the potential of looking at science and technology through the same set of conceptual glasses. Trevor Pinch and Wiebe Bijker kicked off the debate in 1984, outlining the “need for an integrated social constructivist approach towards the study of science and technology” and complaining about the “separation of science from technology” (Pinch/Bijker, SSS 1984:3, p.399) practiced in science studies as well as in technology studies. Their own article sketches the outline of an approach that combines these two sets of work.

The next article in *SSS* dealing explicitly with technology, almost two years later, was also of a theoretical/conceptual nature. In his commentary, Thomas Hughes (SSS 1986:2) first discusses the argument between historians of science and historians of technology about the status of technology *vis à vis* science, and then outlines his own notion of a ‘seamless web’ of science and technology. In the same year, this notion was adopted by Boelie Elzen, who refers to it as a centrepiece of the “promising new approaches in the sociology and history of technology“ (Elzen, SSS 1986:4, p.621). An article by Hans Radder in the same issue also discusses this “growing interest in technology on the part of historians, sociologists and philosophers of science” (Radder, SSS 1986:4, p.663).

A few scattered approaches to treating science and technology as one unit rather than as separate entities with few points of contact can already be found in those years, but for the most part, the articles dealing with technology published in *SSS* at that point have remained on the level of expressions of will (sometimes enhanced by empirical case studies), rather than being full-fledged realisations of a ‘turn to technology’ in science studies. Nevertheless, in 1988, Brian Wynne writes: “Recent issues of this journal have demonstrated that there is now considerable momentum behind the attempt to apply sociology of scientific knowledge (SSK) to a better understanding of technology.” (Wynne, SSS 1988:1, p.147) While some articles discussing technological products and systems can be found in the following issues, none of these contain explicit reference in the abstracts or opening sentences to work in science studies or SSK – rather, they are embedded exclusively into a discourse on technology. It is only in the early to mid 1990s that articles conceiving of science and technology as one unit, to be treated the same way

²⁵ A few exceptions can be found, such as an article on social movements that involve protest against specific technological developments (Leahy/Mazur, SSS 1980:3).

in research, appeared on a regular basis. The terms employed to describe this kind of research agenda range from ‘constructivist studies of science and technology’ (Radder, SSS 1992:1) and ‘social studies of science and technology’ (Jasanoff, SSS 1996:2) to the already familiar terms of ‘science and technology studies’ and ‘science, technology, and society’ (both commonly abbreviated as STS). I will write more on the use of these terms in the next chapter, ‘Searching for a Discipline’.

As I have already hinted at, unlike in *SSS*, technology has been an object of study for authors publishing in *STHV* all along. That is not to say, however, that the meaning of the term ‘technology’ has remained the same throughout the years and decades and that the *types* of technology written about in *STHV* has not changed. Throughout most of the 1980s, the kind of object written about under the catch-all term of ‘technology’ was quite exclusively limited to ‘large’ technologies, especially nuclear energy, but also nuclear weapons and space technology. Other than that, technology was sometimes a topic in relation to science and technology policy, and technology assessment was also sometimes an issue of consideration. Even if ‘smaller’ technologies were written about, which happened very rarely indeed, it was usually in relation to technologies considered threatening – examples would be articles about lie detectors, where the author warns of the negative impacts and non-neutral character of this piece of technology (Simon, *STHV* 1983:3), and about asbestos (Murray, *STHV* 1986:3). In the late 1980s, the focus changed somewhat, and information technologies²⁶ started to enter the agenda, as did considerations about images of technology conveyed by the media (Goldman, *STHV* 1989:3) and perceptions held by the public (Daamen et al., *STHV* 1990:2; Michael, *STHV* 1992:3). Also in the late 1980s and early 1990s, there was increased attention to theoretical aspects of the study of technology²⁷ and discussion of different theories of technological development, especially in a special issue dealing with ‘Theories of Technological Change’ published in 1992²⁸. Gradually, even ‘smaller’ technologies, and technologies

²⁶ Examples include articles by Morrison (*STHV* 1986:4), Kraft/Siegenthaler (*STHV* 1989:2), Bennett (*STHV* 1991:1) and Kling (*STHV* 1991:3).

²⁷ Beckwith, *STHV* 1989:4; Woolgar, *STHV* 1991:1; Bennett, *STHV* 1991:1; Martin/Scott, *STHV* 1992:4; Pinch, *STHV* 1993:1; Bijker, *STHV* 1993:1; Winner, *STHV* 1993:3

²⁸ This special issue included articles by Misa, Sørensen/Levold, Schot, Gökalp, and Roland (all *STHV* 1992:1).

even closer to the human body, started to enter the picture, such as hormonal drugs²⁹, contraceptives³⁰, and prostheses and implants³¹. On the other hand, while earlier studies had focused on the impacts of existing technologies, a new focus developed on the design and testing process³² and the users of technology gradually entered into analyses³³. Often in relation to design considerations and a focus on users, gender aspects also became an issue of reflection³⁴, a subject I will discuss in more detail below.

Curiously, it was also in *STHV* that a few articles questioning the ‘turn to technology’ in STS were published in the early 1990s. Steve Woolgar (STHV 1991:1) coined this term of a ‘turn to technology’. Woolgar warns his colleagues of such a move, arguing that the central analytic ambivalence at the heart of SSK approaches (that is, the issue of reflexivity: SSK questions realist assumptions of scientific knowledge, but by presenting its own alternative account as the only correct one, depends on a kind of realism itself) would be even exacerbated by the application of SSK to technology. Moreover, social studies of technology have “little of the bite and controversy associated with the treatment of scientific knowledge as an object” (ibid., p.36), because technology does not have the same kind of prestige or philosophical backing that scientific knowledge traditionally enjoyed. Technology is thus viewed as “merely another exemplification of the SSK formula rather than an occasion for questioning the idea of applying formulas. [...] SSK becomes a reified technique: a purportedly neutral instrument to be applied indifferently to more and more new kinds of objects.” (ibid., p.44) Woolgar fears that this move would therefore mean to lose the analytic ambivalence and the epistemological significance of the SSK approach – unless, he argues, “a reflexive articulation of the notion of technology as text” (ibid.) is sought. Apparently, then, the idea of applying SSK to technology is not altogether doomed to failure after all. A year later, Knut Sørensen and Nora Levold publish a paper in which they criticise approaches that attempt to adopt notions from the sociology of science to the social studies of technology for neglecting the meso level of analysis, and

²⁹ Oudshoorn, *STHV* 1993:1; Guillemin, *STHV* 2000:4.

³⁰ Clarke/Montini, *STHV* 1993:1; Oudshoorn, *STHV* 1999:2; Van Kammen, *STHV* 1999:3.

³¹ Blume, *STHV* 1997:1; Jain, *STHV* 1999:1; Blume, *STHV* 2000:2.

³² Pinch, *STHV* 1993:1; Henderson, *STHV* 1998:2; Weedman, *STHV* 1998:3; Garrety/Badham, *STHV* 2004:2; Pollock, *STHV* 2005:4.

³³ Gjøn/Hard, *STHV* 2002:2; Summerton, *STHV* 2004:4.

³⁴ Weber, *STHV* 1997:2; Moore, *STHV* 1997:4; Oudshoorn, *STHV* 1999:2; Van Kammen, *STHV* 1999:3; Oudshoorn et al., *STHV* 2004:1.

for mistakenly assuming a high degree of similarity between science and technology – according to the authors, this is incorrect because “technology is usually so much more heterogeneous than science” (Sørensen/Levold, *STHV* 1992:1, p.31). It thus turns out that neither of these articles question the fruitfulness of taking up technology as an object of study (although they are, to different extents, quite critical of the notion of using a unified approach for the study of science *and* technology), nor do they suggest that *STHV* isn’t the place to publish about technology studies – what they put into question is just a particular *approach* to the social studies of technology, not the value of a social scientific analysis of technology per se. Outside of these few critical articles, the idea of treating technology the same way conceptually as science appears quite uncontroversial for most authors. As the preceding analysis shows, this has not always been the case: While articles about technology within the two journals (and in particular, in *STHV*) have been around for a while before that, a symmetrical, unitary treatment of science and technology – a treatment of science and technology as *one* object of analysis, rather than as two separate ones – seems to be a product of a development of the 1980s.

IV.3 ‘Medicine’ as an Object of Analysis

Medical knowledge is another object of study that is considered for analysis (and frequently treated as a separate object from scientific knowledge) in both journals. I will not cover this topic in any amount of detail here, but a few remarks about ‘medicine’ as an object of analysis in *STHV* and *SSS* are in order. While earlier issues included only the occasional article touching upon medical issues (and then usually in the context of studying medical *knowledge*), it has become a much more common topic in the last decade, particularly in relation to biomedical topics. A case in favour of bringing medical sociology as well as anthropological and historical approaches to medical knowledge “into line with recent work in post-Kuhnian sociology of scientific knowledge” (Richards, *SSS* 1988:4, p.653) was already made in 1988; yet even eight years later, Evelleen Richards still saw the need to remark upon the relative lack of “detailed SSK studies of medical knowledge and practices” (Richards, *SSS* 1996:2, p.338), pointing out: “Constructivist studies of medicine owe more to Foucault (or, more recently, postmodernism or social world theory) than to SSK” (*ibid.*). In the following decade, however, (bio)medical and biotechnological topics increasingly entered the research agenda of the two journals.

Most articles dealing with medical knowledge or medical technologies do not contain any explicit positioning statements in a particular discipline by its authors; however, there are some exceptions. One author embeds her analysis in the field of ‘social studies of medical technologies’ (Oudshoorn, *SHTV* 1993:1), another one refers to the ‘anthropology and sociology of medicine’ (Hogle, *STHV* 1995:4), and ‘sociology of medicine and science’ (Karnik, *STHV* 2001:3). Perhaps most interestingly, a recent presidential address held at a meeting of the *Society for Social Studies of Science* and printed in *STHV* (Bijker, *STHV* 2004:2), makes reference to the field of ‘social studies of science, technology and medicine’. If this is any indication, a field which was at first mainly held to deal with science and was later extended to also embrace the study of technology is slowly going through another extension to encompass medicine as well.

IV.4 ‘Gender’ as an Object of Analysis

As I have already hinted at, ‘gender’ as an object of analysis is also worth considering, even if (or precisely because) it lies across the objects dealt with so far, in the sense that gender in itself is not usually considered the object of STS – it is always gender in relation to science, technology or medicine which the authors in the two journals are concerned with. In the early and mid-1980s, specifically, it was the issue of women in science which got some (very moderate) attention on the pages of the two journals. In *STHV*, this period saw the publication of various anonymous news items about the status of women scientists (*STHV* 1983:3c; 1983:3d; 1983:4a; 1983:4b), as well as one short article (Bruer, *STHV* 1984:3) describing the continuing discrimination of women scientists. Articles dealing with gender aspects of science or technology are just as rare in *SSS* during the first half of the 1980s – the only examples to be found are a short comment describing an empirical survey among women university teachers of natural science, which concludes with the construction of “an approximate ‘average’ academic woman scientist” (Burrage, *SSS* 1983:1, p.152), and a comparative article about the estrogen replacement controversy in the USA and the UK, which also takes into account perspectives of feminist and consumer groups (McCrea/Markle, *SSS* 1984:1). That gender wasn’t considered a very important focus of analysis also becomes obvious in an article discussing the problem of junior-authored papers in the construction of citation counts, where only male biochemists were taken as the empirical basis, while female doctorates “were excluded due to their relatively

small number and the difficulty of obtaining complete information on those who did obtain their degrees” (Long et al., SSS 1980:2, p.129) – apparently, a focus on possible reasons for this mismatch was not considered to fall within the scope of the article. In the course of the 1980s, however, gender issues started to be given some attention in both journals bit by bit.

Curiously, the first article to be published in *SSS* which suggests that the journal might do well to consider gender aspects more seriously in its analyses is one which explicitly presents itself as “an outsider’s reading of some literature from the traditional and new history and sociology of science” (Delamont, SSS 1987:1, p.163), written by an author who introduces herself as an experienced sociologist, but new to the field of sociology of science. In this article, Sara Delamont identifies several blind spots in the literature, among them a “failure to examine gender divisions in scientific practice and knowledge, and especially the sexual division of labour in science” (ibid., p.164), suggesting that this blind spot might be related to an undue concentration on the study of elite, high-status science, where fewer women are involved. As Andrea Burrows (SSS 1992:3) criticises in a book review of the *The Outer Circle* (Zuckerman/Cole/Bruer 1991) five years later, even when women scientists are being considered, the focus also tends to be on elite figures, so that inadequate attention is given to the ‘average’ female scientist. In the following year, Margaret Rossiter alludes to the lack of attention for gender issues (and specifically, for the unequal recognition of scientific achievements), pointing out that this blind spot also serves to support the current discriminatory system: “Rather than denying that this is the case, as has been the sociologists’ practice to date, the sexist nature of much of the women’s systematic under-recognition should be acknowledged, noted and even highlighted in the sociology of knowledge or science” (Rossiter, SSS 1993:2, p.337).

In *STHV*, the issue of gender in science and technology is not brought to the table by a self-identified ‘outsider’; but here too, a stronger concern for gender issues begins with complaints about the gender-blindness of science studies (Keller, *STHV* 1988:3-4). Keller argues that this gender-blindness is based on a misunderstanding, which conflates the issue of ‘gender and science’ with that of ‘women and science’, a misconception which leads to the assumption that “the question of gender can only be understood as a question about the

presence or absence of actual women” (ibid., p.238), rather than integrating the study of gender issues into various research questions.

From the late 1980s onwards, this integration of gender issues into STS research has started to take place, although, to be sure, reluctantly, as evidenced by the fact that the first article in *SSS* dealing with gender issues after Delamont’s criticism about the blind spot is rather critical of recent work in the field of gender and science, accusing feminist studies of science of being “increasingly at odds with results emerging from the social studies of science” (Richards/Schuster, *SSS* 1989:4, p.698). Nonetheless, gender issues did increasingly come under consideration in the two journals during the following years, and not always in the context of the kind of studies about the status of women in science that occasionally marked the 1980s. These articles, where gender comes under consideration as an object of analysis in different contexts, include studies about sex hormones and contraceptives (Oudshoorn, *SSS* 1990:1, *STHV* 1993:1; Clarke/Montini, *STHV* 1993:1), about the depiction of female scientists in popular media (LaFollette, *STHV* 1988:3-4), about ‘masculine’ and ‘feminine’ values in research and development (Sørensen, *SSS* 1992:1), about different vocabularies employed by male and female scientists in the same research field (Adams/Burnett, *SSS* 1991:3), and about gender differences in the response to science and technology-related news (Hornig, *STHV* 1992:4). This marks a clear departure from earlier publications on gender issues, where the sole focus had been on the institutional standing and scientific output of female scientists. Of course, this is not to say that these topics dropped off the agenda entirely in the 1990s: A few articles were published on differences in scientific productivity between male and female researchers, and on the effect of motherhood on productivity (Kyvik, *SSS* 1990:1; Kyvik/Teigen, *STHV* 1996:1). In the late 1990s and early 2000s, a series of articles dealing with reproductive technologies, where gender issues are generally a central point of the analysis, appeared in both journals, slightly more so in *STHV*.³⁵

So far, it has been implied that the study of gender as an object of analysis equals the study

³⁵ To substantiate this claim: Papers in *STHV* include those by Rapp (*STHV* 1998:1), Van Kammen (*STHV* 1999:3), Guillemin (*STHV* 2000:4) and Harbers (*STHV* 2003:3); in *SSS*, Singleton (*SSS* 1996:2), Casper and Clarke (*SSS* 1998:2) and Timmermans and Leiter (*SSS* 2000:1) have published on reproductive technologies, taking into account gender issues.

of women or femininity in relation to science or technology. From the late 1990s onwards, this no longer holds true, as gender aspects are now also being called into question in relation to men and masculinity. The issue of male identity and masculinity is brought up in the context of reproductive technologies such as in vitro fertilization (Van der Ploeg, *STHV* 1995:4) or contraceptives (Oudshoorn, *STHV* 1999:2; Van Kammen, *STHV* 1999:3), but also in relation to men's love affair with technology, in the field of engineering (Kleif/Faulkner, *STHV* 2003:2), among car mechanics (Mellström, *STHV* 2002:4) and among rock guitarists and people who tinker with their musical instruments (Waksman, *SSS* 2004:5).

The preceding paragraphs were just a quick glance at the ways in which the issue of 'gender' started to become an object of analysis for the authors in the two journals, not a complete overview over everything that has ever been published on the topic. It does, I hope, give some insights into the contexts in which gender issues were first brought up in *SSS* and *STHV*, and how the treatment of gender issues both became more common and (partially) changed its character over the years. Yet while there are far more articles published on such topics in the last few years than during the early period of the two journals, publications dealing with gender issues still remain a side-line among plenty of articles that do not reflect upon them at all – for example, a 2003 article in *SSS* under the title of 'Is Science Really a Young Man's Game?' (Wray, *SSS* 2003:1), which tests this hypothesis on the basis of a list of revolutionary discoveries cited by Thomas Kuhn (1976) and the age of scientists responsible for the discoveries, only addresses the age, but not the gender of the scientists in question. The fact that the cited authors were indeed all male goes by unremarked upon. Yet as the account above has shown, gender issues have increasingly become an issue of reflection in both journals over the last decades. While recent descriptions of the STS field may treat gender as a self-evident topic for STS research and point towards the "rich tradition" (Sismondo 2006, p.128) of feminist STS analyses, Evelyn Fox Keller's account that "it took an organized effort on the part of feminists to rouse the attention of historians, philosophers, and sociologists to the marks and significance of gender" (Keller 1995, p.92) seems to be spot on: The consideration of gender as an object of analysis was not a matter of course in either of the two journals during their early years, but had to be carved out as a legitimate focus of analysis.

V. Searching for a Discipline

Much like various interpretations about the object of study of STS (or whatever any given author prefers to name their field) can be found, there are also many different understandings of what kind of ‘discipline’ we are even dealing with here. Two main types of definition can be distinguished: one which conceives of the field as an interdisciplinary area or as a fairly recently emerged discipline breaking away from more traditional disciplines; and one which instead draws upon traditional academic disciplines, such as history, sociology or anthropology. The latter conception would describe the two journals as thematically oriented fora for publications dealing with the kinds of objects described in the previous chapter: either as disciplinary journals depicting the state of the art in a particular specialisation of that discipline (much like *Work and Occupations: An International Sociological Journal* presumably sets out to depict developments within the sociology of work or *The Medieval History Journal* is an outlet for historical research on the medieval age), or at best as a meeting ground for scholars from various disciplines who share a common object, where they can exchange some ideas before they return back to their own disciplinary turf. The other conception of the field takes that last thought of a meeting ground as a starting point, but holds that this meeting ground has stabilised and been established as a new field in its own right, and that many of the scholars who operate on it thus do not need to return back to their own turf: This is their turf. Within that perspective, too, there is some variety (some of which has already been pointed out in chapter II.3), as some see this as an interdisciplinary field, others a newly established or about-to-be-established discipline. Nor has a common name been found for such an enterprise: Some refer to it as ‘science (and technology) studies’, others as ‘social studies of science’, others again as ‘Science, Technology, and Society’ – the list is not exhaustive.

My claim here is not the two different perspectives outlined above are necessarily irreconcilable – there are many instances where the terms are used (at least apparently) interchangeably, and it is certainly possible to conceive of the field as a whole in terms of a new (inter)discipline, yet to still make reference to certain disciplinary approaches which can be found within this field. Yet despite some overlap and grey areas, it makes sense to distinguish for analytic purposes between instances where the authors conceive their own

research as taking place in the context of a specialisation in one academic field (e.g. ‘history of science’ or ‘sociology of technology’), and those where they place it in the context of a field that connects and maybe even transcends various disciplinary perspectives on a particular subject.

The following chapter will thus consist of two sections: The first section discusses the various ‘traditional’ disciplinary approaches that go into the study of science and technology, and the second part describes various conceptions of a field that transcend these traditional disciplinary distinctions. An interlude that reflects upon the relationship of two fields that share a similar status of institutionalisation, STS and gender studies, connects these two sections.

Throughout my analysis, I have decided to pay special attention to those authors who chose to position themselves as ‘outsiders’ looking in on the field, or at least as not fully accepted members of the core STS community. To be sure, my approach here is not to identify specific authors as ‘objectively’ (not) part of the STS community, but rather to take into account how they present themselves – my intention, after all, is not to draw boundaries of my own, but rather to analyse how they are drawn. This decision to present oneself as an insider or an outsider might, at least in some cases and partially, also be a strategic one, but unearthing true intentions of the authors is not part of my analysis. Rather, I am interested in how authors explicitly draw the boundaries of the field, and in the perspectives they assume when they do so.

V.1 Traditional disciplinary perspectives

The following section includes five sub-sections, outlining the way that disciplinary approaches from five different fields are discussed in the pages of *SSS* and *STHV*: sociology, history, philosophy, anthropology, and psychology.

V.1.1 Sociology

Sociology is, without a doubt, the discipline on which more authors of the two journals recur than on any other one. Without wanting to turn this into a counting exercise, it can be said that this is true both for the number of authors referring to just one discipline, and for

the amount of mentions in combination with other disciplines – that is to say, whenever an author positions themselves within a combination of disciplines (along the lines of ‘sociology and anthropology’ or ‘history, philosophy and sociology of science’), chances are that sociology is going to be among the disciplines listed.

Another cue at the dominance of sociology would be the fact that, while references to other disciplines tend to either be to the sub-field concerning itself with science and/or technology, or to the discipline in general, a third kind of reference to sociological approaches can be identified: references to specialisations within sociology that have no direct relation with science or technology. Some of these references have the function of linking an article to the discourse going on in a particular sub-genre of sociology, while others only take certain elements out of that discourse in order to transfer them into the field of STS. The sub-disciplines upon which the authors in *SSS* and *STHV* draw are quite varied: they range from the sociology of culture (Hennion, *STHV* 1989:4) to the sociology of expectation (Hedgecoe, *STHV* 2003:4) or the sociology work, occupation and professions (Watson/Meiksins, *STHV* 1991:2; Star, *SSS* 1985:3; Roth/Bowen, *SSS* 2001:4). This is quite in contrast to the way other disciplines are generally brought up, where reference is made either to the discipline as a whole (e.g. ‘an anthropological approach’), or the sub-field dealing with science and technology (e.g. ‘philosophy of science’). The only other exception here is history, which I will discuss in the next subsection. At the same time, an author who introduces herself as coming “to the field as an experienced sociologist but a novice in the sociology of science” (Delamont, *SSS* 1987:1, p.163) explicitly positions her article as “an outsider’s reading of some literature from the traditional and the new history and sociology of science” (*ibid.*), thus implying that a familiarity with sociology itself is not enough to qualify as an ‘insider’ to the field. While general sociology or specific specialised sociologies (and not just the ‘sociology of science/technology’) are thus sometimes employed as a resource in these journals, it also would not be right to position them as general sociology journals.

Nor would it be correct to claim an absolute dominance of sociological approaches throughout the years. For a start, the importance given to sociological approaches varies greatly between the two journals under investigation – references to sociology are far more

common in *SSS* than they are in *STHV*, especially during the 1980s. In fact, at a first glance, the occurrence of invocations of sociological approaches seems to have undergone an almost opposite development in the two journals: In *SSS*, references to sociological research and traditions have been extremely dominant over other disciplinary approaches throughout the 1980s, but have lost considerable ground since then, as more interdisciplinary conceptions of the field (symbolised in terms such as the ‘social studies of science’ or ‘science and technology studies’) have become more common. In *STHV*, on the other hand, references to sociology have actually become more common since the journal became the official journal of the *4S* society in the late 1980s. Before that, references to sociology were sparse, and if they appeared at all, it was often, although not always, in conjunction with references to philosophical and historical work (e.g. Caplan, *STHV* 1980:4; Burnham et al., *STHV* 1987:2). Yet simply calling this the opposite of the development in *SSS* would be short-sighted: As the description in chapter III has already let on, *STHV* made a more general shift towards becoming a more traditional scholarly journal around the time it became the official *4S* journal, leaving behind its roots as a thematic bulletin for questions related to science and technology (especially in application and policy contexts), where few articles were explicitly positioned in any particular academic tradition at all. In this sense, the development of the journal underwent was not just one of becoming more thoroughly *sociological*, but of becoming more *academic*. That this also, and perhaps especially, implied a strong influx of explicitly sociological approaches is probably due to the set-up of the academic niche in which the journal now positioned itself, and in particular to the composition of *4S*. Even then, most of the theoretical discussion of sociological concepts still took place within the pages of *SSS* (as the source of most quotes in the following paragraphs will go to show), whereas in *STHV*, most disciplinary references to sociology were of a more casual nature.

In the light of this, the ‘dominance’ of sociology over other disciplines is certainly not complete. Yet at the same time, it is true to talk of its ‘dominance’ in another sense of the word: While none of the articles under analysis find any historians or philosophers denying the value of a sociology of science outright³⁶, the same cannot be said of the opposite direction. Especially during the first decade covered by my analysis, authors who explicitly

³⁶ However, an article (Roth/Barrett, *SSS* 1990:4) discussed in more detail in section V.1.3 comes close to providing such a rejection of a sociological approach to the study of science.

positioned themselves as sociologists launched some rather staunch attacks on other disciplines that are traditionally concerned with the study of science, especially history and philosophy of science. Presumably, this is partly due to the fact that, while history of science and philosophy of science are relatively well-established specialities within their respective home disciplines, many sociologists of science seem not to have this kind of security, and are out to prove that it is in fact possible to study the production of scientific knowledge (and not just science as a social institution) with sociological means and methods. For example, Trevor Pinch writes: “Many issues which are normally thought of as technical matters within science have been shown, in recent studies, to be amenable to sociological analysis.” (Pinch, *SSS* 1981:1, p.131) On a similar note, David Kirby, more than twenty years later, points out that the “last 20 years of research in the sociology of science have convincingly revealed the social component in the production of scientific knowledge” (Kirby, *SSS* 2003:2, p.231) – the focus on scientific knowledge rather than the institutions of science (also see chapter IV), of course, being the key term here. These quotes demonstrate that sociologists of science, especially during the early years of the journals (and in particular, of the more sociologically-oriented *SSS*), felt the need to justify the very nature of their scientific endeavour. It is in this context that instances such as Steve Yearley’s gloating about the fact that “sociologists of science have very successfully prised the study of scientific development from the hands of philosophers and internalist historians” (Yearley, *SSS* 1990:2, p.313), can be better understood.

After all, the type of sociology of science that usually filled the pages of *SSS* (and, to a lesser extent, *STHV*) is not just in opposition to traditional philosophy and internalist history of science, but also to certain traditions within sociology itself. Steve Woolgar has described this as a “move towards a sociology of science and away from a mere sociology of scientists” (Woolgar, *SSS* 1982:4, p.481), in the course of which the actual contents of scientific knowledge have moved to the centre of sociological attention. Scientific practice itself has also become an important object of the study of scientific production (Pinch, *SSS* 1985:1; Merz/Knorr Cetina, *SSS* 1997:1). These approaches are sometimes referred to as simply the ‘new sociology of science’ (Giere, *SSS* 1997:3) or the ‘new sociology of scientific knowledge’ (Lynch/Fuhrman, *STHV* 1991:2). This ‘new’ approach, frequently associated with the ‘strong programme’ in the Sociology of Scientific Knowledge or with

constructivist approaches, often presents itself in opposition to sociological functionalism and especially to Mertonian sociology of science³⁷. For example, Steve Woolgar writes that the “daring extension of sociological purview which is recommended” (Woolgar, *SSS* 1981:3, p.365) by researchers associated with the ‘strong programme’ has deeply unsettled traditional philosophers as well as sociologists within the Mertonian tradition of research. In his discussion of Robert Merton’s work in the sociology of science published in *SSS* shortly after Merton’s death, Stephen Cole, himself one of Merton’s former students and colleagues, has accused the very journal in which he placed his obituary, as well as the *4S* society, of being a “source of sometimes virulent criticism of the Mertonian research program” (Cole, *SSS* 2004:6, p.841) and therefore partly responsible for what he sees as the ‘closing-down’ of the Mertonian research program, as it, according to Cole, fell completely out of grace with the mainstream of the field. Michael Lynch, in the editorial published in the same issue of *SSS*, does concede that the journal used to be strongly associated “with University of Edinburgh’s Science Studies Unit and the Strong Programme in the sociology of scientific knowledge (‘Strong’ in contrast with, among other programmes, Mertonian sociology of knowledge)” (Lynch, *SSS* 2004:6, p.828), but also points out that the current state of the field of social studies of science, technology, and medicine “is neither ‘Mertonian’ nor ‘anti-Mertonian’. Nevertheless, Merton’s legacy and ‘anti-legacy’ are indelible features of the history and development of the exciting and contentious transdisciplinary research presented in this journal’s pages over the past 35 years.” (ibid.). That this ‘anti-legacy’, and thus the explicit condemnation of Mertonian sociology, has taken a back seat in the immediate aftermath of Merton’s death probably comes as no surprise. Indeed, Wiebe Bijker, in a memorial speech for Robert Merton (as well as for David Edge and Dorothy Nelkin) held at a *4S* meeting and reprinted in *STHV*, refers to Merton as being “by all standards – the founder of the field of social studies of science” (Bijker, *STHV* 2004:2, p.131).

Other ‘classical’ sociologists have also been commemorated after their death for their impact on the development of the field, while at the same time alluding to the fact that their approaches were often at odds with current ones. For example, Ron Westrum (*SSS* 1986:3)

³⁷ Stewart (*SSS* 1986:2) explicitly distances this approach from sociological functionalism, while Woolgar (*SSS* 1981:3), Sonnert (*SSS* 1995:1) and Ashmore (*SSS* 1996:2), among others, stress the dissociation from Mertonian sociology of science.

points out in his obituary of Joseph Ben-David that he had been strongly opposed to the relativistic tendencies of the Strong Programme, while Stephen Turner refers to Edward Shils as “a ‘sociologist of knowledge’ of the old style” (Turner, *SSS* 1995:2, p.397) who had “declined to give a ‘sociological’ account of the contents of science” (*ibid.*). As we have already seen, this move towards the sociological explanation of the contents of scientific knowledge is often referred to as a more radically sociological approach (Woolgar, *SSS* 1981:3) or as a ‘social turn’ (Lynch, *SSS* 2003:1). Yet others argue that these developments are in fact a move away from sociological explanations: In an obituary for Pierre Bourdieu, Daniel Breslau regrets the move away from the more traditional sociological explanation of the kind given by Bourdieu, arguing that this was an “eclipse of the social in the science of science” (Breslau, *SSS* 2002:4, p.634), as “the intellectual strategy for science studies has been one of establishing its independence from existing disciplines by emphasizing its intimacy with the local contexts of science, and neglecting the social relations that extend beyond this context” (*ibid.*). What we witness here is the use of obituaries as a place to commemorate not just the life and work of a person, but to also use them as an arena to reminisce about what may have been lost in the move towards a new focus of research and (allegedly) away from more traditionally sociological approaches. Whether this move is in fact more or less sociological than the sociological classics themselves is something that the authors in the two journals (and especially in *SSS*, which has printed more obituaries, as well as more in-depth discussions of sociological concepts than *STHV*) do not necessarily agree on, and is not a question that should concern us here anyway, except to assert that quantifying approaches as ‘more’ or ‘less’ sociological is probably a futile task. As the preceding paragraph has shown, sociological discourses and figures are often brought up in carving out an academic specialty for the journals, but usually with an element of critical demarcation from these approaches; most sociologists (that is, people who present their work as sociological research, a collective that is not necessarily identical with those who have a degree in sociology) publishing here position their work as an explicitly sociological approach, but in opposition to some of the major traditions of sociology (of science). The element that is most commonly stressed as setting it apart from mainstream sociology is the rejection of essentialist principles of how science works, as well as the focus on scientific knowledge rather than science as a social system or institution.

V.1.2 History

The discipline besides sociology on which authors in *SSS* and *STHV* recur the most is undoubtedly history. References to historical literature or approaches are common both on their own and in combination with references to sociology – the combination ‘history and sociology of science’ can be found a lot, especially in the 1980s volumes of *SSS*, thus before the terms ‘social studies of science’ or ‘science and technology studies’ really caught on. For the most part, authors tend to link their articles to work in the history of science (or, much less commonly, the history of technology), although there are also plenty of articles that make reference to the field of history in general. Very rarely, references to other sub-fields of history can also be found, such as ‘cultural history’ (Marx, *STHV* 1983:4; Saari/Miettinen, *STHV* 2001:3, among others) or the work of ‘historians of biology and of early twentieth-century social thought’ (Bix, *SSS* 1997:4).

Despite all the references to the history of science that can be found in both journals, there also seems to prevail a certain ambivalence towards historical approaches. I have already mentioned the charges against internalist traditions in the history of science (Yearley, *SSS* 1990:2), which are criticised for explaining scientific developments purely from within science, without recognising social factors that influence science; however, as some authors point out (Hughes, *SSS* 1986:2; Andersen, *SSS* 1988:3), within history of science, a counter-tradition of ‘contextual history’ has emerged, which cannot be incriminated of internalism. A more common criticism, then, holds that history of science, for all its richness of detail, does not have much to offer in terms of theoretical accounts or rigorous concepts. This point is expressed by Michael Gorman and W. Bernard Carlson, in regard to the work of historians and biographers doing research on invention: In spite of the detailed studies “of the lives and accomplishments of individual inventors” (Gorman/Carlson, *STHV* 1990:2, p.132), there is a “conceptual lacuna” (*ibid.*, p.133) when it comes to understanding the act of invention as an intellectual or cognitive process. Now, this is not presented as criticism from the outside (one of the authors was trained as a historian of technology himself, the other as a psychologist), but rather as an advancement of existing approaches to the history of technology – but it is still a rather harsh critique of the theoretical and conceptual capabilities of existing historical accounts. Roland Giere, in his obituary of Thomas Kuhn, praises the work of historians for dealing “with real scientists in

real time” (Giere, *SSS* 1997:3, p.497), but also diagnoses them with a reluctance to theorise about science in general. On a related note, the sociologist Randall Collins draws a rather clear hierarchy between sociology and history when he reflects upon the “dual relationship between theory and history” (Collins, *STHV* 1989:2, p.109): “On the one side, historical analyses provide essential material for developing a sociological theory. On the other hand, theory is also being used to illuminate these histories.” (ibid.) While it should be remarked that Collins isn’t explicitly talking about history as a *discipline* here, but rather about the status of historical accounts, it is still an interesting touch to talk of history and theory as two entirely different phenomena. History here seems to be little more than an ancillary science for sociology.

Perhaps it should come as no surprise, then, that Alex Roland begins a commentary in *STHV* with the following words: “Like many historians of technology, I experience a certain amount of culture shock when I read articles such as those collected in this issue of *Science, Technology, & Human Values*.” (Roland, *STHV* 1992:1, p.79) Interestingly enough, one of the things that throw him off, besides the rather different vocabulary, is indeed the fact that these accounts are “more theoretical and abstract” (ibid.) than what he is used to – thus perhaps confirming that the style of analysis employed in a journal such as *STHV* is indeed more theoretical than ‘mainstream’ historical accounts. Of course, for Roland, this isn’t necessarily an advantage: “The danger in concentrating on theoretical constructs, as the authors in this collection of articles do, is that it can lead to distortion of fact.” (ibid., p.81) Still, Roland concludes, “the differences between those theories and many of the models found in the historical literature are more semantic than substantive. Students of technological change seem to have more in common than their disciplinary vocabularies would suggest.” (ibid., p.93) Whether the differences are indeed semantic or substantive, the preceding paragraph has shown that there sometimes rests an uneasy tension between historical approaches and the mainstream of analysis covered by the two journals. The authors in *SSS* and *STHV* frequently recur on historical work, but are also quick to point out that their own research offers some surplus value over traditional historical approaches, in that it is more theoretically oriented, more conceptual and more ready to provide conclusions that go beyond the scope of the particular historical case that is being presented.

Some of this ambivalence in regard to historical work might become more palpable when considering the reception of Thomas Kuhn, who is by far and away the theorist who gets name-checked the most in papers following or discussing a historical approach in both journals. It is interesting to note here that one of the very first discussions of Kuhn's work starts with the observation that Kuhn's writings, while very influential in the philosophy of science and sociology of science, "have had only minor application to history of science" (Reingold, *SSS* 1980:4, p.477) and concludes that this is the case because it simply isn't very applicable. If we take this observation of a reluctance on the part of historians of science to take up Kuhn's views at face value³⁸, this might also hint at a certain divergence between the kind of historical accounts published in these two journals, where Kuhn's work is eulogised as "the single most influential force in creating the intersection of history, philosophy and sociology of science that became identified as 'science studies'" (Giere, *SSS* 1997:3, p.497), and the field of history of science on the whole. According to this diagnosis, some of the divergence between the mainstream of history of science and the academic approaches adopted in *SSS* and *STHV* might be due to the debt owed to Kuhn's legacy in the historical papers published in these journals, while history of science at large may have been far more apprehensive about taking up notions from Kuhn's writings.

The estrangement of science and technology studies and history of science is also diagnosed in a speech held by Sheila Jasanoff at a meeting of the History of Science Society (HSS) in 1999 and printed in *SSS* in 2000. She describes the relationship between the two fields (and their respective societies, *4S* and *HSS*) as a "somewhat one-sided love affair" (Jasanoff, *SSS* 2000:4, p.623): "4S has recognized from its very foundation that history of science has to be part and parcel of any meaningful attempt to study science and technology as human, social institutions. Accordingly, we have not only welcomed historians to our meetings but have showered on them most of the prizes and honours at our Society's disposal. [...] On the HSS side, the attitude toward 4S's intellectual

³⁸ Judging the merits of this diagnosis that Kuhn's writings left only little traces in the history of science and was far more influential in the philosophy and sociology of science is, clearly, not possible on the basis of an analysis of the journals *SSS* and *STHV*. That Kuhn has indeed been a major influence on theoretical developments in sociology and philosophy probably goes without saying; a detailed account of the reception of Kuhn's work within sociology of knowledge (identified mostly with the 'strong programme' of SSK), contemporary philosophy and the natural sciences, but not of his reception in the history of science, was written by Uwe Rose (2004).

programme bespeaks, in my view, considerably greater wariness. [...] A historian colleague given to pithy statements puts it this way: ‘Historians like to get prizes from 4S, but they don’t want to go there’.” (ibid., p.622) Still, Jasanoff expresses the hope that “Science and Technology Studies (S&TS), the field that 4S stands for, has reached a point at which we can begin to engage (some would say *reengage*) with the History of Science as a mature intellectual partner” (ibid.; italics in the original), and shows several points on which the two fields have already reached some convergence.

Jasanoff is not alone in at least detecting some signs of convergence. Trevor Pinch, not a historian but a sociologist himself, suggests that case studies in what he calls the sociology of scientific knowledge display “the kind of sensitivity to the technical content of the science studied which is familiar from the history of science” (Pinch, *SSS* 1985:1, p.3) and goes on to quote the historians Rudwick and Shapin as saying that some of this work is history of science ‘in all but name’. While Pinch clearly identifies ‘his’ style of scholarship as a sociological approach, he also sees a close affinity to historical approaches. Indeed, the historian Peter Dear holds that “much recent work in the history of science dealing most effectively with the culture of science is directly and deeply indebted to the work of SSK” (Dear, *STHV* 1995:2, p.156). In summary, historical approaches seem to play an important role in much of the research covered by the two journals and points of mutuality with historical research are often sought out, but nonetheless, some troubles affect the relationship with history as a discipline. In particular, historical approaches are frequently lauded for their richness of detail and concern for an in-depth understanding of science and technology, but also scathed for their lack of willingness to look beyond the specificities of each case, to draw upon theoretical notions and to work on conceptual conclusions. Related to this ‘conceptual lacuna’ may or may not be an unwillingness of historians to take up stimuli from the work of Thomas Kuhn, whose writings have been influential for the more historically-oriented research published in *STHV* and *SSS*.

V.1.3 Philosophy

Philosophy is the third traditional discipline that is very frequently invoked in the pages of the two journals. Both in *STHV* and in *SSS*, references to philosophical approaches in combination with sociology, history and sometimes other disciplinary approaches are frequently found. However, references to philosophy as a stand-alone discipline are more rare, and few authors explicitly and exclusively present themselves as philosophers. There are, of course, exceptions – such as an article by Langdon Winner in *STHV* wondering what philosophers need to know about technology (Winner, *STHV* 1993:3) or one by Gale and Pinnick, published in *SSS*, in which they criticise the laboratory studies approach from the standpoint of epistemology (Gale/Pinnick, *SSS* 1997:1). Indeed, a series of commentaries and papers taken from a symposium on ‘The Potentials and Limits of Applied History, Philosophy, and Sociology of Science and Technology’ and published in *STHV* in 1980, with a number of follow-up commentaries published in 1981, focuses mostly on the possibilities of an applied philosophy of science – as the historian Loren Graham has remarked, during this debate, “only Philosophy made it to center stage” (Graham, *STHV* 1981:1, p.3). Yet philosophy does not leave the stage without having to face some criticism first, as Michael Mulkey, in his comment on the same debate, suggests that “an effective applied philosophy of science would have to become more sociological” (Mulkey, *STHV* 1981:1, p.15).

Mulkey is not alone with this suggestion, as several articles are framed in terms of a rivalry between sociology and philosophy. I have already hinted at this tendency in the section about sociology in this chapter, as well as in chapter II.3, where I have quoted Sal Restivo’s (1995) description of STS as characterised by a contest between philosophy and sociology. Indeed, a number of articles, especially ones published in *SSS* during the 1980s, display a tendency of sociologists seemingly wanting to ‘emancipate’ the study of science from being the exclusive domain of philosophers, who are accused of a focus on the discussion of general principles of science, at the expense of a concern with the specifics of scientific practice. For example, there’s Steve Yearley’s triumphant observation that “sociologists of science have very successfully prised the study of scientific development from the hands of philosophers and internalist historians” (Yearley, *SSS* 1990:2, p.313) in recent decades; there’s Steve Woolgar’s more polite, but still rather aloof comment that

“philosophers adhering to traditional tenets about the scope of their investigation have been disturbed by the potential intrusion of sociologists into their domain” (Woolgar, SSS 1981:3, p.365); there’s Elisabeth Clemens’ off-hand comment that “even philosophers have devoted increasing attention to specific disciplines and research networks” (Clemens, SSS 1986:3, p.421). The last example, at least, points to a convergence between sociological and philosophical approaches (although it is not difficult to detect an air of condescension in the admittance that *even* philosophers are beginning to see the light) – a point that is also made by Denise Russell (Russell, SSS 1983:3), who points out several affinities between Paul Feyerabend’s philosophy of science and relativist approaches in the sociology of science.

The picture outlined so far is one of philosophical approaches frequently invoked as one of several influential strands for the study of science, but also often confronted with criticism for its lack of empirical orientation and exaggerated concern with general principles of science. Probably the climax of this debate about the merits and pitfalls of philosophical approaches to the study of science came in a 1990 special issue of SSS, based on a series of papers and comments from a symposium and published under the title of ‘Deconstructing Quarks’. The leading article, written by two philosophers, calls into question if recent attempts of explaining scientific theory changes with sociological, rather than philosophical, analyses have been “no more than the dream of empire-building social scientists” (Roth/Barrett, SSS 1990:4, p. 580). As they argue on the basis of a discussion of several works in the sociology of science (most prominently, Andrew Pickering’s (1984) book *Constructing Quarks*), such a case for the sociological analysis of scientific theory change has yet to be convincingly made. Now, this article would be interesting in its own right, as it is an attack on sociology launched by two philosophers not unlike the kind of stabs directed at philosophers by various sociologists quoted in the paragraphs above, but it is especially interesting for the replies it has provoked. One comment simply replies to some substantial points contained in the paper by Roth and Barrett, without getting involved into the quarrel between different disciplinary approaches (Oldroyd, SSS 1990:4), while others do respond to the disciplinary turf war, but express the hope to move beyond it (Nickles, SSS 1990:4; Pickering, SSS 1990:4) and another challenges sociologists to stop paying attention to the objections and debates of philosophers altogether (Fuller, SSS

1990:4). Steve Fuller contends that “there has never been a successful philosophical explanation of an episode in the history of science” (ibid., p. 666) and that sociologists would be better off to drop notions stemming from the philosophy of science (and specifically, Quine’s formulation of the underdetermination of scientific theory by evidence) altogether, pointing out their origins in “a technical philosophical context, little of which ought to be to the sociologists’ liking” (ibid.). However, most of the other sociologists responding to the paper are less keen on the separation of sociological approaches from philosophical thinking, pointing out that neither sociology nor philosophy are uniform and that some versions of the sociology of science are very much compatible with some versions of the philosophy of science – just not with the kind of philosophy of science advocated by Roth and Barrett (Pickering, SSS 1990:4). Trevor Pinch, too, sees the paper by Roth and Barrett as a “good example why so many of us working within the sociology of scientific knowledge (SSK) despair of constructive dialogue with some philosophers of science” (Pinch, SSS 1990:4, p. 658), but is quick to point out that this by no means applies to all philosophers of science, and that common ground can be found with some philosophers. One of those philosophers he mentions is Thomas Nickles, who also replies to Roth and Barrett’s paper, expressing his disappointment about its arrival just when it seemed that “the ‘mutual contempt’ phase of relations between sociologists and philosophers was coming to an end, and just when sociologists thought it was finally safe to talk to philosophers” (Nickles, SSS 1990:4, p. 633). He does concede that “Roth and Barrett are zealously responding to the rhetoric of earlier sociological tracts, but we can largely understand *that* as a combination of self-motivating manifestos and territorial display by eager scholars trying to carve out a new specialty” (ibid.; italics in the original).

Nickles’ account seems to be to the point: By 1990, the ‘mutual contempt phase’ between sociologists and philosophers seems to have been largely over and done with, at least in the pages of *SSS* (in *STHV*, it has never left much of a trace to begin with, Mulkay’s paper quoted above notwithstanding), with the *Deconstructing Quarks* debate serving as a last example of the quarrels between philosophers and sociologists³⁹. His characterisation of

³⁹ A counter-example would be Steve Fuller, who several years later, in an obituary of Thomas Kuhn, points out that Kuhn epitomised “much of what science studies has stood *against*, especially in its confrontations with philosophers” (Fuller, SSS 1997:3, p.493), thus once again reminding the readers of the old antagonism between science studies and philosophy of science.

many of the snarky comments about philosophy quoted above as ‘territorial display by eager scholars trying to carve out a new speciality’, too seems apt: At least this would explain their concentration in the early years of the period under study, and the fact that few authors seem to see much of a need to explicitly distinguish their own approaches from a philosophical tradition in the later issues. In fact, most of the later references to philosophical work are of a rather casual nature, with the exception of a few examples mentioned at the beginning of this section, as well as a paper by David Bloor (SSS, 1996:4) in which he tries to clear up some misunderstandings about the sociology of science often invoked by many philosophers. For the most part, it appears that the debate on philosophy of science was one of the early period, during which many authors doing work in the social study of science saw the need to establish their own approaches in the light of traditional philosophical discourses from which they set themselves apart. Once these approaches had become established, philosophical approaches more or less shifted out of view. Indeed, when Keulartz, Schermer, Korthals and Swierstra (Keulartz et al., STHV 2004:1) discuss the merits and blind spots of philosophy and STS, these are conceptualised as two entirely different modes of scholarship.

V.1.4 Anthropology

Sociology, history and philosophy are the most common ones, but not the only disciplinary approaches that authors in *STHV* and *SSS* sometimes identify with or at least refer to as a reference point. One additional discipline that is referred to in more than a handful of articles in both journals is anthropology. For the most part, these articles refer to anthropology as one of several disciplinary approaches that are relevant to the study of science, technology and medicine, usually in combination with sociology and history, sometimes also philosophy and political science (Richards, SSS 1988:4; Hart/Victor, SSS 1993:4; Callon, STHV 1994:4; Hogle, STHV 1995:4; Chompalov/Shrum, STHV 1999:3; Mialet, SSS 1999:4). However, several authors also self-identify as anthropologists (Orr, STHV 1998:4) or position their studies as anthropological analyses (Forsythe, SSS 1993:3; Rapp, STHV 1998:1; Livingston, SSS 1999:6). One author describes the ‘anthropology of science and technology’ as a “developing” (Forsythe, STHV 1993:4, p.460) field and suggests that anthropological approaches could help to identify embedded values in technological tools, thus objecting to the traditional view of technology as value-free.

Another author argues that “[c]ultural anthropology can help elucidate some of the underrecognized cultural aspects of environmental issues” (Fitchen, *STHV* 1987:2, p.1). Anthropology is also said to be making a contribution to the field of STS with its notion of ‘culture’. Linda Layne writes: “I introduce a new concept for the STS theoretical tool kit – the cultural fix – which I argue represents a distinctive anthropological contribution to the field.” (Layne, *STHV* 2000:4, p.493) In her opinion, the “broader understanding of ‘culture’ in STS is transforming some of the field’s fundamental understandings of where and how technoscience is produced” (*ibid.*, p.509), so the anthropological notion of culture is leaving its imprints on the field of STS as a whole.

This view implies that anthropological approaches do not ‘naturally’ or ‘self-evidently’ fall within the scope of STS – it presupposes a view of STS that is more or less free from anthropological approaches to start with, and to which some pinches of anthropology are added after the fact. This impression is furthered by an introduction to a special issue on anthropological approaches in STS, in which Layne reports that the *4S* meeting in New Orleans had been “a memorable meeting because of the visible anthropological presence” (Layne, *STHV* 1998:1, p.4); evidently, then, a visible anthropological presence was not something to be taken for granted at a *4S* meeting. An article published by Emily Martin in the same special issue highlights the tensions between STS and anthropology; in it, the author presents herself as something of an outsider to the field of STS, although she also identifies as “a recent participant in science studies” (Martin, *STHV* 1998:1, p.25). As such, she concedes to feel humiliated by some of the recent criticisms of anthropological approaches by various STS scholars, and gets defensive about the contributions of anthropology to the field, which she feels have been met with reservations, if not downright hostility, by some STS scholars. In an obituary for Diana Forsythe, David Hess (*SSS* 1998:1) points out that some of this criticism might be due to a confusion about what anthropology is precisely – after all, argues Hess, the term of an anthropology of science had first been appropriated by non-anthropologists, in the context of the laboratory studies of the late 1970s and early 1980s. According to Hess, this has led to a curious situation of non-peer review when attempting to publish in STS journals, as anthropologists now have to “face the oddity of non-anthropologists telling anthropologists that they are not doing proper anthropology” (*ibid.*, p.178). Yet he also describes the community of anthropologists within STS as one that has stabilised and grown more coherent over time.

Finally, it should be noted that not all sub-specialties of anthropology are seen as integrated into the STS field to the same extent. Michael Geselowitz, who self-identifies as a prehistoric archaeologist (a field which, at least in the USA, is generally organised as a sub-discipline of anthropology), seems to have a much more positive view of the integration of anthropology on the whole into the STS community, but says that his own specialty of prehistoric archaeology has “despite early calls for an inclusion in the field of science, technology, and society [...] not, by and large, been integrated into this new discipline” (Geselowitz, STHV 1993:2, p.231). According to Geselowitz, this is both due to a lack of demand and desire to draw upon data and approaches from prehistoric archaeology on the side of STS scholars, and to a failure on the supply side, as archaeologists have failed to present their work in a way that offers explicit points of contact with STS research.

In summary, then, the relationship between anthropology and STS seems to be seen as troubled by many anthropologists: While a considerable number of anthropologists contribute to the journals and engage with the STS community, many of them remark upon tensions between the fields. While there is disagreement about the extent to which anthropological approaches have been integrated into the STS community and about the cohesion of a community of anthropologists within STS, it seems to be generally agreed upon that anthropology is not, for the most part, one of the disciplines that STS has emerged out of, so that anthropology is not seen as a self-evident ingredient of the STS mix. Concerns have also been voiced about the ‘appropriation’ of anthropological approaches by non-anthropologists, and that this obfuscation of anthropology now makes the position of ‘proper’ anthropologists within STS more difficult. In this light, then, the section above can be read as an account of the attempts of anthropologists to get their foot in the door of STS and establish themselves as part of the (inter)disciplinary mix that STS is arguably made up of.

V.1.5 Psychology

Psychological approaches can also sometimes be found within the pages of the two journals, although it is interesting to note that psychology appears as an object of analysis almost as often as it appears as a disciplinary approach (see preceding chapter, ‘Searching for an Object’). Like in the case of anthropology, a number of articles mention psychology as one of several disciplines that bear on the debate or subject at hand (Star, SSS 1983:2; Hartland, SSS 1996:1) or that are devoted to the study of psychological along with social factors (Tweney/Yachanin, SSS 1985:1; Lightman/Miller, SSS 1989:1). A few others are explicitly positioned as psychological studies (Turkle, SSS 1982:2; Farris/Revlin, SSS 1989:3; Schulze, SSS 1990:1; Golden, SSS 1994:3). Gorman and Carlson argue that the historical study of innovation is dealing with a “conceptual lacuna” (Gorman/Carlson, STHV 1990:2, p.133) and that concepts from cognitive psychology could contribute to “develop a more rigorous framework for analyzing invention” (ibid., p.134) as an intellectual or cognitive process; in this view, cognitive psychology is called upon to make good on some of the conceptual weaknesses of the history of technology. Another article observes that, while Kuhn and Bloor had drawn much inspiration from psychological studies, “sociology of science since Kuhn and Bloor has frequently seemed at odds with cognitive psychology” (Gorman/Mehalik, STHV 2002:4, p.499). The authors identify social constructivism’s notion of ‘technological frames’ as one concept that could be elucidated by insights from cognitive psychology, and show how such an integration of mental models and technological frames could be brought about. Again, the argument here is that psychology can make an important contribution to STS, but the starting point for this observation is that the fields of STS and cognitive psychology are currently at odds with each other. On a related note, an obituary of Donald Campbell describes his work in experimental and social psychology as rather unconnected to his existence as figure in the science studies community, pointing out that “[f]ew of the scholars in science studies with whom he interacted fully appreciated how important and influential his psychological work was” (Rosenwein, SSS 1996:3, p.717). Campbell’s own interest, then, is described as trying to bring the two fields closer together, hoping for “a more psychological approach to science studies” (ibid., p.720).

It is curious, then, that apparently psychology seems at least as likely to be considered as

an object of analysis in the study of science (see chapter IV) as it is as a disciplinary approach to be employed to the study of science, whereas none of the other disciplinary approaches discussed above have been an object of STS scholarship to the same extent. The explanation seems to lie in the focus of most studies on the natural sciences, and the fact that, unlike in the other disciplinary approaches discussed here, there is a strong tradition within psychology of considering the discipline as a natural science (Benetka 2002; Metzger 1976). Heike Wiesner (2002) offers this as an explanation for the lack of systematic psychological analyses of science: “In my opinion, this deficit is no coincidence. The traditional design of psychology finds its justification in the paradigm of the natural sciences. The affinity of psychology to the natural sciences thus results from the (historical) understanding of science and from the concrete practice of research. The mechanisation of psychology bears on the contents of work as a positive ‘bias’.”⁴⁰ (Wiesner 2002, p.41, translated by AS) The cautious appearance of psychological approaches within the journals *SSS* and *STHV* seems to support this interpretation.

Interlude: Gender Studies

Surely, it wouldn't seem right to treat gender studies indiscriminately as one of the traditional disciplinary approaches that STS research is (or is not) made up of that I have outlined in the previous section of this chapter. Yet at the same time, this should not be a reason to refrain from reflecting upon the relationship between STS and gender studies; on the contrary, such an analysis might provide some insights into how two different interdisciplinary fields or very young disciplines interact with each other.⁴¹ Unlike the disciplinary approaches discussed so far, all of which can look back upon at least several decades of a more or less stable disciplinary identity and a relatively successful process of institutionalisation, gender studies seems to be in a similar stage of institutionalisation as STS itself. As discussed in chapter II.2, this is not to say that the criteria for stable

⁴⁰ Original text: “Dieses Defizit ist m.E. kein Zufall. Das klassische Design der Psychologie findet ihre Begründung im Paradigma der Naturwissenschaften. Die Affinität der Psychologie zu den Naturwissenschaften ergibt sich somit aus dem (historischen) Wissenschaftsverständnis und aus der konkreten Forschungstätigkeit. Die Apparatisierung und Technisierung der Psychologie wirkt sich als positiver ‚bias‘ auf die Arbeitsinhalte aus.”

⁴¹ Another example could be the case of postcolonial studies; however, whereas references to gender studies are spread out over both journals and one and a half decades, articles reflecting upon postcolonial studies are essentially concentrated in one special issue published in *SSS* in 2002 (*SSS* 2002:5-6).

academic disciplines are self-evident, either; but no matter which particular criteria are applied, it is fairly clear that sociology, history, philosophy, anthropology and psychology meet disciplinary standards such as more-or-less-well-defined international communities, internationally accredited institutions and departments, and indeed, a shared name. As the second section of this chapter will show, things are not quite as clear for STS; nor are they for gender studies. The trouble starts with the name: So far, I have adopted the label of gender studies; however, similar (but not quite identical) conceptions, such as ‘feminist studies’ or ‘women’s studies’, also exist. It is not my goal here to trace the exact ways in which these terms are employed in the two journals, as only a relatively small number of texts dealing with either of these conceptions have been published there anyway. Yet a short glance at the secondary literature suggests that the different denominations are not interchangeable: For example, Friederike Hassauer (1994, p.12) argues that the term ‘women’s studies’ simply connotes looking at the role of women in addition to previous academic analyses, whereas ‘gender studies’ suggests a new category of research, that of gender, a term which can symmetrically refer to both genders. As an entry to *Metzlers Lexikon Gender Studies Geschlechterforschung* specifies, the term ‘gender studies’, unlike ‘women’s studies’, does not postulate “communalities of women which could be attributed to their specific physicalness or to the male reaction to this physicalness [...]. Thus, the central category of analysis of G.St. is not ‘the woman’ or ‘femininity’, but gender as a genus, i.e. as a historically mutable, societal-cultural phenomenon.”⁴² (*Metzlers Lexikon Gender Studies Geschlechterforschung* 2002, p.143, translated by AS) The term ‘feminist studies’, on the other hand, seems to point towards the origins of the academic field in a movement of political feminism, and indeed, the extent to which gender/feminist/women’s studies is a discipline just like any other or rooted in a political and social movement is also an important issue of contention (Hark 2005). In the following paragraphs, however, I will not distinguish between these different terms, instead opting to reflect upon the relationship of gender/ feminist/ women’s studies⁴³ and STS.

⁴² Original text: “Gemeinsamkeiten von Frauen, die auf ihre spezifische Körperlichkeit bzw. die männliche Reaktion auf diese Körperlichkeit [...] zurückzuführen wären. So ist die zentrale Analysekategorie der G.St. nicht ‘die Frau’ oder ‘Weiblichkeit’, sondern Geschlechtlichkeit als Genus, d.h. als historisch wandelbares, gesellschaftlich-kulturelles Phänomen.”

⁴³ Much like I have adopted the name of STS without repeating the different permutations over and over, I will also, for reasons of readability, refrain from reiterating the different denominations from now on.

As has become clear in the previous chapter, ‘Searching for an Object’, the consideration of gender issues has not been a topic of concern in the two journals from the outset. It will therefore not come as a big surprise that gender studies cannot be taken to be one of the ‘founding disciplines’ of the STS field, and that the relationship between STS and gender studies only came to be reflected upon in the late 1980s and early 1990s. As Evelyn Fox Keller complains in a Keynote Address at a *4S* meeting, printed in *STHV* in 1988, work on gender issues in science “has attracted a considerable amount of interest among feminist scholars in all disciplines, [but] our invitations to historians and sociologists of science not automatically identified with feminism has, at least until now, gone largely unaccepted, for the most part, even unacknowledged” (Keller, *STHV* 1988:3-4, p.236). Evelleen Richards and John Schuster, in an *SSS* article published the following year, also agree with Keller that “the two disciplines of feminist studies and social studies of science have only very recently begun to intersect” (Richards/Schuster, *SSS* 1989:4, p.697f.). Richards and Schuster, however, analyse the situation differently, arguing not that science studies scholars are unwilling to engage with work on gender issues, but rather, that many feminist studies of science tend to work with essentialist assumptions about the gendered character of scientific method that are “increasingly at odds with results emerging from the social studies of science” (ibid., p.698). After these accusations, Richards and Schuster conclude on a more conciliatory note, calling for a “critical engagement and mutual instruction of feminist studies and social studies of science” (ibid., p.715). In a reply to this article, Evelyn Fox Keller points out that some of the tension between feminist theory and STS might be due to the confusion of the categories of sex (a biological category) and gender (a cultural category) by many STS scholars: a distinction that marks an important contribution of feminist theory, but that has proven difficult to communicate “beyond the realm of feminist scholarship, perhaps especially so in the social studies of science” (Keller, *SSS* 1989:4, p.721).

The goal of these paragraphs is not to determine who is to blame for the tensions between gender studies and STS, which apparently do or did exist; rather, it is to highlight these tensions in order to reflect on the relationship between the two fields of research. One central point of tension, already mentioned in the preceding paragraph, is the question of essentialism and determinism. This topic is brought up by a number of contributors in both

journals, often in relation to particular types of feminism or gender studies research, especially regarding ‘eco-feminist’ views, which presuppose that science and technology are strongly gendered in a masculinist fashion, and that an alternative system, more oriented towards caring and nurturing values and thus more ‘feminine’, should be fostered. This kind of approach is also incompatible with other versions of feminism, for example with more liberal conceptions (Cowan, SSS 1995:2), but it might be partially responsible for an uneasy tension between certain strands of feminism and STS approaches. This tension has also been taken as a challenge, where the goal is to “find a ‘third way,’ where speaking about gender is meaningful without relying on essentialist assumptions” (Berg/Lie, *STHV* 1995:3, p.342) – something that the authors think can be accomplished if constructivist STS and feminism learn from each other and make use of existing meeting points, such as the understanding of gender and technology as social constructs or the focus on microanalysis. In another article in the same special issue of *STHV*, which was published under the heading of ‘Feminist and constructivist perspectives on new technology’, Baukje Prins (*STHV* 1995:3) also expresses the concern that feminism and constructivism should learn from each other, pointing out that feminism might do well to adopt constructivism’s scepticism towards grand theories and focus on details, while constructivism might learn to be more politically relevant, to leave room for utopian ideals, and to take into account the perspectives of outsiders. Also reflecting upon the relationship of feminism and constructivism, Keith Grint and Steve Woolgar (*STHV* 1995:3) diagnose the situation differently, arguing that not only feminism, but also constructivism is guilty of implicitly containing essentialist assumptions.

Yet most authors deliberating about the relations between gender studies and STS in the two journals stress the need for dialogue and point out ways in which the two strands of research can cooperate or learn from each other. Several authors (Berg/Lie, *STHV* 1995:3; Wajcman, SSS 2000:3; Lohan, SSS 2000:6) agree that the difficulty of bringing gender issues into STS analyses lies with the focus in the STS tradition on the design phase of technologies or the focus on scientific laboratories; that is, with the move upstream, away from the users. Since “most scholars are habituated to consider gender issues only when their subjects are women” (Wajcman, SSS 2000:3, p.454), and women are often not present in these stages of research or technology development, gender is hardly recognised

as a relevant category either. Another point of tension would be the STS prescription against a priori assumptions, which is difficult to reconcile with the a priori decision to focus on the analysis of gender issues harboured by gender studies, argues Lohan (SSS 2000:6). Yet Lohan also talks of a “developing theory-coalition in feminist technology studies between feminist studies and science and technology studies” (ibid., p.895), where these “formerly separate schools” (ibid., p.900) are starting to converse and even converge, despite some remaining tensions. Indeed, Judy Wajcman (SSS 2000:3) mentions a growing (if still inadequate) awareness for gender issues within STS, while feminist studies of technology have moved away from the previously held essentialist and technologically determinist approaches, towards the examination of “the very processes by which technology is developed and used, and those by which gender is constituted” (ibid., p.450). These developments give room to cross-fertilization, argues Wajcman, but there is some serious catching-up to do, as the feminist writing on technology has for the most part been “carried out at some conceptual distance from the sociology of science and technology which developed during this same period. This writing is explicitly informed by a combination of cultural studies, anthropology and postmodern philosophy.” (ibid., p.457)

In summary, while numerous points of tension between STS and gender studies have been pointed out in the pages of the two journals, and the authors tend to agree that during the early years, these two research traditions have developed in some distance from each other and have often been explicitly at odds with each other, there seems to be widespread agreement that they do share enough common concerns to make a cooperation fruitful, and the tendency in the last few years seems to have been a convergence of the two fields. This finding also seems to be in line with publications at the intersection of the two fields outside of *SSS* and *STHV*; for example, Heike Wiesner’s dissertation aims to “make the category of gender fertile for the discourse of science studies”⁴⁴ (Wiesner 2002, p.14, translated by AS), a goal which presumes that so far, gender was not a prolific element of science studies discourse. Indeed, Wiesner writes that, in order to meet this goal, she has “not only spotted, but also systematically *established* the interface between specific STS

⁴⁴ Original text: “die Kategorie gender für den Diskurs der Wissenschaftsforschung fruchtbar zu machen”

and feminist discourses”⁴⁵ (ibid., p.292, translated by AS; italics in the original). Wiesner’s dissertation also includes a comparative perspective that my own thesis cannot reappraise: In comparing the situation at the interface of STS and gender studies in the German and Anglo-American contexts, she found that STS and gender studies have found and established some common ground in the Anglo-American countries for some time already, while in the German-speaking countries, such a development has not yet taken place.

V.2 Beyond Traditional Disciplinary Perspectives

For all the references to and identifications with various (more or less) traditional disciplinary approaches found in the two journals and outlined in the previous section, there are also many references that refer not to any particular traditional discipline but rather to an inter- or transdisciplinary research field referred to under a more general moniker (such as ‘social studies of science’ or ‘science and technology studies’). To be sure, there is no diametric opposition between the two – one can refer to an interdisciplinary field and at the same time mention the different disciplines that make up this interdisciplinary mix. Some authors are explicit about the relationship they see between the disciplinary inputs and the larger interdisciplinary field, as the example of Trevor Pinch shows, who writes about the “sociology of science – my own specialty – but also [...] the field of Science, Technology and Society (STS) at large” (Pinch, SSS 1992:3, p.487). Brian Wynne also sees the sociology of scientific knowledge as part of a broader STS field: “Here I use the term ‘sociology of scientific knowledge’, SSK, but imply no precise demarcation from the wider field of science and technology studies, S&TS. Whatever differences now cleave SSK, it shares a basic commitment to the idea that sociological investigation is relevant to the question of how scientific knowledge is constructed, and that this question is interesting and important. S&TS as a whole does not share these minimum commitments.” (Wynne, SSS 1996:2, p.382)

Others describe the field as historically developing away from its disciplinary roots: Wiebe Bijker reports about the “multidisciplinarity and broadness of the field of science, technology, and society studies (STS) as it has developed from its early stage as a specialty of sociology” (Bijker, STHV 2004:2, p.133), which resulted in the broadening of the

⁴⁵ Original text: “die Schnittstelle zwischen spezifischen STS- und feministischen Diskursen nicht nur ausgemacht, sondern auch gezielt *hergestellt*”

“social studies of science, technology, and medicine beyond the confines of sociology” (ibid.).

Between accounts sticking to the traditional disciplinary identities and descriptions of an interdisciplinary research field, there exists a grey area of terms such as ‘sociology and history of science’ or ‘philosophy and anthropology of science’ – while literally, these could be interpreted to mean two separate disciplines, looking at the way in which they are employed, it turns out that they are in fact often used as a token for a shared field devoted to the study of science in which people from various disciplinary backgrounds get together and exchange ideas, concepts and findings. Indeed, it is possible for one person to identify as a ‘historian *and* philosopher of science’ (Ruse, STHV 1986:2), or for authors to explicitly identify the ‘sociology and philosophy of science’ as *one* research field (Brante/Halberg, SSS 1991:3).

On the whole, however, this use of the names of two or three traditional disciplinary approaches as a shortcut for describing an interdisciplinary research field has waned since the early 1990s, as other terms that do not specify the disciplinary inputs have become more established, including phrases such as the ‘social studies of science’, ‘science, technology and society studies’ or ‘science and technology studies’. To be sure, none of these terms are an invention of the 1990s; they have been around since the 1980s, if not longer, as a few instances of use during that period show:

- ‘Science, Technology, and Society’ (e.g. Hargreaves/Hargreaves, SSS 1983:4; Mayo, STHV 1985:4)
- ‘science studies’ (e.g. Yearley, SSS 1985:1; Restivo, STHV 1987:2)
- ‘social studies of science’ (e.g. Shepard/Hamlin, STHV 1987:2; Latour, SSS 1988:1)

Specifically, such labels have always been around in formats such as the ‘country report’, an erroneously published article type in *SSS* to describe the research field in a particular country. These country reports have discussed fields such as ‘science studies’ (Elzinga, SSS 1980:2; Levin, SSS 1984:3; Freudenthal, SSS 1990:2), ‘history and social studies of science’ (Pancaldi, SSS 1980:3) and ‘social studies of science’ (Bowker/Latour, SSS

1987:4; Low, SSS 1989:2) – on the other hand, none of them have been devoted the (exclusive) discussion of the sociology or philosophy of science in any particular country. While no devoted format of this sort exists in *STHV*, reports have been published about the ‘science of science in China’ (Weingart, *STHV* 1981:2; Elzinga, *STHV* 1981:2) and about the development of STS curricula (Slaughter, *STHV* 1980:2).

While it is thus true that these terms have been around probably since the inception of the two journals (after all, none of the journals was founded under the banner of one particular disciplinary approach), they have definitely become more common as descriptors of the academic field covered by the two journals during the 1990s and 2000s.

Which of these terms is adopted in particular is to some extent a random choice – or at least it is difficult to detect any particular pattern, and the terms are sometimes used seemingly interchangeably within one article. Nonetheless, it pays to look at the different variations. The term ‘social studies of science’ reached its peak in *STHV* during the early/mid-1990s, when many articles made reference to this name⁴⁶, but has come more or less out of fashion since then. In *SSS*, presumably because it resonates well with the name of the journal, the term has displayed more staying power and continues to be invoked quite frequently in the 2000s.⁴⁷

Probably the group of terms most commonly used to describe the academic field covered by the journals are ‘science studies’, ‘technology studies’ and ‘science and technology studies’. As I have already remarked in chapter IV, the occupation with technology as a research object has increased over time, especially in *SSS*, where it had not been much of a topic for research or discussion in the early/mid-1980s. Early mentions of technology studies, then, tended to conceive of it as a separate entity from science studies, in need of integration with the study of science (Pinch/Bijker, *SSS* 1984:3). While references to both ‘science studies’⁴⁸ and ‘technology studies’ or ‘social studies of technology’⁴⁹ are made,

⁴⁶ A few authors using this term during the peek period include Woolgar (*STHV* 1991:1), Felt and Nowotny (*STHV* 1992:4), Wright (*STHV* 1993:1) and Nutch (*STHV* 1996:2).

⁴⁷ Examples include Rees (*SSS* 2001:4), Shapin (*SSS* 2001:5), Levidow (*SSS* 2001:6), Hercock (*SSS* 2003:1) and MacKenzie (*SSS* 2003:6).

⁴⁸ A few examples include Yearley (*SSS* 1985:1), Fuhrman and Oehler (*SSS* 1986:2) and Restivo (*STHV* 1987:2).

⁴⁹ An early example of the use of the term ‘technology studies’ is an article by Boelie Elzen (*SSS* 1986:4). Instances of the use of the term ‘social studies of technology’ include articles by Kranakis (*SSS* 1989:1),

the integrated term of ‘science and technology studies’, now very common, did not catch on until the early 1990s in *STHV* and the mid-1990s in *SSS*.⁵⁰ A similar point can be made about the term ‘social studies of science and technology’.⁵¹

If ‘technology’ was mentioned alongside ‘science’ as part of a name for a discipline or interdisciplinary field during the 1980s, it was generally not in the context of terms such as ‘science and technology studies’ or ‘social studies of science and technology’, then, but rather in ones that were more concerned with applications or external relations of science, such as ‘science and technology policy studies’ (Hamlett, *STHV* 1983:2) or ‘science, technology and society’ (e.g. Hargreaves/Hargreaves, *SSS* 1983:4; Mayo, *STHV* 1985:4; Beckwith, *STHV* 1989:4). Perhaps this goes to show that science and technology were more likely to be conceived of as *one* unit of analysis in the context of its societal or policy application. Incidentally, this kind of focus is far more likely to be found in *STHV* than in *SSS*, where to this day, hardly any mentions have been made to ‘science, technology and society’ as a field of research (some exceptions would be, as previously mentioned, Hargreaves/Hargreaves, *SSS* 1983:4 and Pinch, *SSS* 1992:3). Indeed, it has been argued that the more activist-oriented ‘science, technology and society’ movement is separate from the more academically-minded ‘science and technology studies’ (Edge, *SSS* 1996:4), and that it might be time for “present (science and) technology studies [...] to break away from a too academic, internalistic perspective and return to the politically relevant ‘Science, Technology & Society’ issues that informed much of this work more than a decade ago” (Bijker, *STHV* 1993:1, p. 113). This hints at the fact that sometimes, a name is more than just a name: Whether the term ‘science and technology studies’ or the alternative ‘science, technology and society’ is employed might, at least in some cases, be related to the perspective chosen and to the political agenda of an author – a topic that will be discussed in more detail in the following chapter, ‘Searching for an Agenda’. Of course, some disambiguation is caused by the fact that both terms can be, and frequently are,

Carlson and Gorman (*SSS* 1990:3), Vincenti (*SSS* 1991:4), Kling (*STHV* 1992:3) and Grint/Woolgar (*STHV* 1992:3).

⁵⁰ Early uses in *STHV* include those by Pfaffenberger (*STHV* 1992:3), Giere (*STHV* 1993:1), Winner (*STHV* 1993:3), Gieryn (*STHV* 1996:1) and Frickel (*STHV* 1996:1). In *SSS*, the term caught on a little later, e.g. in articles by Myers (*SSS* 1995:1), Balász et al. (*SSS* 1995:4), Richards/Ashmore (*SSS* 1996:2) and Velho/Pessoa (*SSS* 1998:2).

⁵¹ See, for example, Coward and Franklin (*STHV* 1989:1), Sørensen and Levold (*STHV* 1992:1), Jasanoff (*SSS* 1996:2), Pels (*SSS* 1996:2) and Hecht (*SSS* 1996:3).

abbreviated to ‘STS’, which means that many authors (be it on purpose or not) actually evade the decision to adopt one or the other term and just use the abbreviation without specifying what it should be taken to stand for. As the preceding paragraph has shown, this is probably more than a random decision, nor does it seem to be a case of ‘science and technology studies’ being the “newer guise” (Jasanoff et al. 1995, p.XI), while ‘science, technology and society’ was the older incarnation of the same thing.

Whether the field or discipline in question is referred to as STS, social studies of science, or something else, it is commonly described as a primarily Western, and in particular Anglo-American, affair. This point is brought up in an *SSS*-comment in 1985 written by a theoretical astrophysicist from India, who introduces himself as an outsider to the field of STS. This conception as an outsider is partly due to the fact that he is not a trained social scientist and that his “interest in history, philosophy and sociology of science is only that of an outsider” (Choudhuri, *SSS* 1985:3), but partly also due to geographical factors: “Most philosophers and sociologists of science, who have studied the nature of the scientific enterprise, grew up in Western society and were trained in Western universities. [...] I think that we, who have seen Western science both from the outside and from the inside, develop an unusual perspective, and become aware of certain aspects of the scientific enterprise which a Western philosopher or sociologist very naturally overlooks.” (ibid., p.476f.)

Choudhuri is not alone with the opinion that STS is for the most part a thoroughly Western academic enterprise – indeed, others would go as far as saying that it is thoroughly Anglo-American. It is striking that, among the series of ‘country reports’ published in *SSS* during the 1980s, there is a common tendency to regard the state of the art of STS or social studies of science in the United States and/or Great Britain as a reference point for the situation in the countries being reported upon. The general tendency is not just to compare any given country to the US and the UK, but also to point out that the phenomenon of STS, as it exists in those countries, is unheard of in the country the article reports about. This is true for a report on the ‘science of science’ in China written by Peter Weingart for *STHV* in the early 1980s, in which Weingart remarks that “Chinese science of science differs from our understanding of the social studies of science” (Weingart, *STHV* 1981:2, p.17), as well as

for the aforementioned country reports published in *SSS*. For example, the situation in Sweden is said to be marked by an absence of both “‘American-style’ sociology of science [and] ‘British-style’ social studies of science” (Elzinga, *SSS* 1980:2, p.200); the field of social studies of science in Italy by a low degree of institutionalisation and by intellectual fragmentation (Pancaldi, *SSS* 1980:3); Soviet science studies hardly exist at all, and “little attention is paid to modern Western perspectives in the social study of science” (Levin, *SSS* 1984:3, p.462); in France, “if we look for a well-defined field of scholarship in some vague way akin to what English or American academics call ‘social studies of science’ or ‘Science, Technology and Society’ (STS), [...] there are very few professionals, very few groups, few libraries, very few journals, no curricula” (Bowker/Latour, *SSS* 1987:4, p.715) – in other words, “nothing comparable to what on the other side of the Channel and the Atlantic is called ‘science studies’, particularly ‘social studies of science’, has emerged” (Freudenthal, *SSS* 1990:3, p.354) in France; and in Japan, “social studies of science remain in a chrysalis, waiting to be awakened” (Low, *SSS* 1989:2, p.332). In summary, *none* of these country reports reporting on state of the art in the field of STS or social studies of science in their own country report back about the existence of a well-defined research field along those lines; they *all* remark upon the lack of such structures. Each and every one of them presents this lack of a clearly-defined research field as a specificity of their own country, usually invoking the USA and Great Britain as examples of countries where such a research field does exist. Curiously, reports upon the state of the art in these countries have not been published in either journal⁵², so that it is open to interpretation whether an author attempting to sketch out the situation of STS in the USA or the UK would also remark upon the lack of clear structures or thematic coherence there. The general diversity of approaches described in this chapter would suggest that they probably would; a unified, sharply defined and to-be-taken-for-granted field of STS does not appear to exist in anywhere, so that the lack of such structures and patterns is probably not a specificity of the countries of Sweden, France, Italy, Japan or any of the others that have been reported about above, but rather a characteristic of STS in general.

⁵² Arguably, the lack of country reports about the UK and the USA is no coincidence, but rather a further clue to the dominance of Anglo-American scholarship: A possible explanation would be that the journals on the whole provide sufficient insights into the state of the art of British and US-American STS, so that there was little need for a dedicated country report; in this interpretation, the country reports are understood as providing an overview over work in countries that are not usually covered (extensively) by the journals.

VI. Searching for an Agenda

After the preceding analysis of the way authors in *STHV* and *SSS* conceptualise the object as well as the discipline of their academic activities, I will now concentrate on the agenda behind their work. As Pierre Bourdieu (1975) has pointed out, the imposition of a definition of science (or, one could add, of an academic discipline or specialty) is always, in some way, driven by an agenda of sorts, as the struggle for academic recognition is about establishing the very definition of science that best fits one's own approach and products. Indeed, this notion is one of the underlying thoughts of this thesis on the whole. Yet my purpose in this chapter is more specific than to show a general agenda underlying the different conceptions of a scientific field. Rather, it is concerned with the kind of social or political agenda that authors or editors may or may not envision as constitutive of the STS field on the whole. By extension, this also means to look at the agenda stated for individual articles.

We can coarsely distinguish two main approaches here: While some authors position their research as driven by a purely academic curiosity, others instead begin by showing the political or social relevance and implications of their work. My aim here is decidedly *not* to uncover hidden agendas and masked intentions behind these articles, only to discuss those that have been made explicit. This focus is not based on the naïve assumption that scholars are always open about their 'true' agenda and are thus going to accurately point out their intentions behind writing a particular paper – rather, the idea behind this analysis is that the 'real' intentions of an author are less interesting than the manifest practice of conceding to (or ignoring/denying) a social or political agenda. The question here is not whether the work of an individual *is* driven by political or social concerns and interests, but whether the author deems it appropriate or even necessary to position their work in relation to such concerns, whether they decide to embed their work into a political discourse or refrain from doing so – a decision which is as much symptomatic about the rules of conduct and discourse in a journal or field as it is indicative for the individual motivation of an author. In order to answer this question, it is necessary to trace the explicit theoretical debates about a political or social agenda of STS, as well as the individual practice of (not) taking a stand on a particular issue.

For the most part, my account will be structured (vaguely) chronologically, documenting the development of debates about the present and potential role of social and political engagement in STS research. Some parts will discuss the two journals separately, whereas others, where the convergence appears quite big, will discuss overarching developments in both journals. However, before giving my account of these developments, I will first have a look at how the various authors in both journals and at various times have taken to describe the historical origins of STS. My concern here is related to that of Sabine Hark in her undertaking of a discursive history of (predominantly German) feminism, where she points out that the past and the future of an academic project are always intertwined: “Because for one thing, the past is never just given, but is constantly re-made in the ways of its tradition; for another thing, the future is always already configured by the ways in which the past is reported upon.”⁵³ (Hark 2005, p.17, translated by AS) Hark’s concern here is for the cultural memory of feminism, which can be remembered either as rooting in a social movement or as an academic discipline like any other. This question can be posed in very similar terms for the historiography and cultural memory of STS: While some authors attribute a big role to social and political movements in the history of the field, others deny that this played any role at all, instead tracing the origins of STS to a purely academic development. These different historiographical accounts do not directly, unidirectionally bear on the discussion of normative stances in state-of-the-art and future STS research, but nonetheless shape the way in which scholars look upon their field.

VI.1 Social Movement or Academic Field?

Broadly speaking, there are two divergent views of the origins of the STS field: One stresses the importance of a critical ‘science and society’ movement for the development of the field, whereas the other conceptualises STS as an academic community of disinterested scholars with little or no overlap with social movements bearing upon questions of science and technology. It is important to note that these different historical views of the development of STS stand in no linear relationship with particular opinions on what

⁵³ Original text: “Denn zum einen ist Vergangenheit nicht einfach gegeben, sondern wird in den Weisen ihrer Überlieferung beständig neu gemacht, zum anderen ist Zukunft immer schon davon konfiguriert, wie von jener berichtet wird.”

normative or political role STS *should* play. On the side of those who urge STS researchers to be more openly political in their actions and reflections, there are both those who bemoan the fact that STS has lost sight of its roots in a political movement (e.g. Shepard/Hamlin, *STHV* 1987:2; Martin, *STHV* 1993:2; Bijker, *STHV* 1993:1, 2003:4) and those who do not appear to see any historical affinity between STS and a critical science movement, but who think that STS should overcome its current (and past) normative irrelevance (Restivo, *STHV* 1987:2). On the other hand, I could find no cases of a scholar who argues that STS should break free from its roots as a social movement, although such a position certainly seems conceivable – then again, there are very few articles to be found stating that STS should not flat out play any normative role at all. The alternative view, that STS has never had any ties worthy of mention with a critical science movement and should keep it this way, has indeed been expressed (Collins, *SSS* 1996:2).

Both journals have published their share of papers that point to a formerly lively, but recently neglected tradition of tackling normative issues in STS research and make a case for a return to these normative roots of STS. Among the scholars supporting this account are Hans Radder (*SSS* 1992:1), who argues that STS has lost its sight in the process of its institutional establishment starting in 1975, as well as Edward Woodhouse, who makes a similar claim, accusing the field of having “moved away from engagement with real human problems” (Woodhouse, *STHV* 1991:3, p.390) since the 1980s. Brian Wynne phrases a similar point quite differently, arguing that “the issue of its political relations has never been far from the explicit agenda of *SSK*” (Wynne, *SSS* 1996:2, p.357), but also acknowledging that it has been marked by apparent extinction during the 1980s. For Wynne, this had only been a temporary loss of sight, while others suspect a more systematic blind spot. In both cases, the message is clear: STS research has always had a normative role to play, even if it has sometimes lost sight of this fact, and should not betray its roots by sinking into normative irrelevance.

What’s more, several authors argue that STS has not only traditionally been concerned with normative questions, but that its roots can in fact be traced back to a social movement of the late 1960s and early 1970s – a view that, as the following citations will show, seems to enjoy greater popularity in *STHV* than in *SSS*. Philip Shepard and Christopher Hamlin

report: “Many of the social studiers of science joined up in the 1960s and 1970s because they were concerned about the immense powers that western societies had invested in science.” (Shepard/Hamlin, STHV 1987:2, p.19) Among these scholars who first became interested in issues of science and society through their own involvement were, according to their accounts, both Brian Martin (STHV 1993:2) and Wiebe Bijker (STHV 1993:1, 2003:4). Bijker thinks that this particular background might in fact be specific to the Netherlands: “Science and technology studies in the Netherlands did not so much emerge from the academic disciplines of mainstream sociology, history, or philosophy. Rather, their origin lies in the Dutch ‘science and society’ movement that resulted in the early 1970s in the establishment of critical STS programs in most science and engineering faculties.” (Bijker, STHV 1993:1, p.116) Bijker’s account goes on as follows: “That *science-and-society* movement fruitfully merged with the *sociological studies of science in society*, roughly at the same time that the crucial impulse from the *strong program* and the *sociology of scientific knowledge* (SSK) came.” (Bijker, STHV 2003:4, p.445; italics in the original) However, Bijker argues, much of the resulting academic work has lost sight of its normative relevance, and he proposes that STS researchers should tackle these issues again. Martin’s story, while taking place in Australia rather than the Netherlands, is quite similar: Acquainted with the anti-capitalist radical science movement as a Ph.D. student of physics in the 1970s, where “a critique of science was seen as part of a critique of society” (Martin, STHV 1993:2, p.247), Martin soon began to read literature from the sociology of scientific knowledge, but grew disappointed with the fact that, as the years rolled on, these seemed “to become more insular, more disconnected from those early concerns about the human impact of science” (ibid., p.248). Also, Martin had to witness during what he refers to as the academisation of SSK, that the influence of the critical science movement and of authors with an openly critical agenda have been “given little acknowledgement” (ibid.) in the official canons of SSK.

Indeed, some authors do not acknowledge any particular influence of such movements for the development of STS. Harry Collins is most explicit in his rejection of this view, stating that “contrary to Martin, SSK owes no direct debts to the science protesters” (Collins, SSS 1996:2, p.230), as its real roots lie “in academic questions about the universality of knowledge, in Wittgenstein’s (and Winch’s) ideas about forms of life and, perhaps, in Kuhn’s

version of the history of science” (ibid.). He insists that SSK and the science protest movements, whom he accuses of “never doubting the power of scientific authority in matters of science knowledge” (ibid.), have very little in common. Even if he admits that “one or two practitioners of SSK” (ibid.) think that the most important mission of SSK is to level the ground between science and other cultural endeavours, he insists that “others would prefer SSK to have left science exactly as it found it” (ibid.). As an aside it can be noted that a few years later, in a paper co-written with Robert Evans, Collins already seems less sure of the absence of all ties between science studies and a social movement, wondering: “Has it become a political movement rather than an academic discipline?” (Collins/Evans, SSS 2003:3, p.436).

While Collins’ article is exceptional in its explicit rejection of the influence of a science protest movement in the origins of the STS field, plenty of articles exist that never make any mention of such a movement at all and instead limit themselves to the discussion of theoretical developments.

Curiously, Sal Restivo, who certainly could not be accused of – as Collins put it – preferring SSK ‘to have left science exactly as it found it’, seems to be with Collins where the influence of the critical science movement is concerned. While Collins seems content, even determined, to keep STS and the science protest movement completely separate, Restivo’s goal appears to be having STS become more like the science protest movement. Nonetheless, like Collins and unlike the likes of Martin or Bijker, Restivo’s (STHV 1987:2) historical account of the development of STS mentions only scholars and no protesters, except when he points out that the radical critics of science have not left any influence on contemporary science studies at all. For him, the time before the 1970s have been dominated by uncritical accounts of science, and the emergence of the new STS field has made no fundamental difference in this respect: “With the emergence of science studies as a hybrid discipline in the 1970s, modern science came under more critical scrutiny, and *scientific knowledge* itself became an object of social inquiry. But there is a significant degree of continuity across the 1970s watershed. [...] So, just as on the other side of the 1970s watershed science studies exemplified and helped to preserve the worldview of modern science, science studies on this side of the watershed continue to a great extent (albeit in an arena of greater intellectual diversity and deeper critical insights)

to exemplify and preserve that same worldview.” (ibid., p.13f.; italics in the original)

As I have already mentioned in chapter V, ‘Searching for a Discipline’, whether STS is conceived of as a social movement or as a purely academic discipline often goes hand in hand with the choice of a particular name for the field. Taking STS as an abbreviation for ‘science, technology, and society’ means to emphasise the interaction with society, whereas STS as ‘science and technology studies’ clearly focuses on the scholarly aspects of the STS undertaking. An *SSS* editorial (Edge, *SSS* 1996:4) mentions that Richard Sclove has remarked upon how the loss of political relevance in the STS field links up with a shift of meaning of its name from ‘science, technology, and society’ to ‘science and technology studies’. David Edge disagrees with this appraisal, but seems to concur with the meanings associated with the two terms: “Our experience in Britain is that ‘S&TS’ has had a parallel ‘life’ [to ‘ST&S’]: the two communities have been quasi-independent, yet constantly interacting. The acronym ‘STS’ has happily covered both, with no sense of any impeding ‘take-over’ or ‘shift of meaning’ – let alone any intention of ‘depoliticizing’ STS.” (ibid., p.728)

My aim here, obviously, is not to uncover to what extent STS *really* is, or emerged out of, a social movement or a scholarly discipline – rather, I am interested in the boundary work that takes place when these accounts are expressed and contested. Clearly, for the authors publishing in the two journals, STS is both a movement and an academic field, and which aspect prevails is bound to be shaped by the way the particular scholars entered the field themselves, which country they live in, the type of research they make, and, indeed, which normative role they want to see for the STS field. It is this issue that I now turn to on the following pages.

VI.2 Modern Ballet or Experimental Dance?

The following section traces the debate about normativity and political or social engagement in the two journals up until the late 1980s. Since very little common ground is to be found between the two journals regarding this issue during that particular time period, I will proceed by outlining the developments for both journals separately.

VI.2.1 The Debate in SSS

In *SSS*, the issue of normative and political relevance of STS did not become an explicit topic of discussion at all until the early 1990s. Throughout the 1980s, the idea of being politically or socially relevant largely appeared to be a non-issue; even articles dealing with topics of clear societal relevance, e.g. with social movements against particular technologies (Leahy/Mazur, *SSS* 1980:3; Del Sesto, *SSS* 1983:3) were generally linked to scholarly discussion of the same issue rather than to societal debates and were careful not to take any apparent partisan stands on the issue in question. Papers in which authors explicitly take a stand on an issue were far and few between, and generally – as I have already discussed in chapter III – took place not in the ‘articles’ section of the journal, but rather under different headings, such as ‘comments’ or ‘discussion papers’. One such example would be a comment on current educational reforms in Britain, which, on the basis of historical evidence, warns of these plans of reform and their notions of ‘flexibility’ (Fox/Guagnini, *SSS* 1986:3). While a discussion paper on the fluoridation controversy two years later (Martin, *SSS* 1988:2) doesn’t explicitly take sides in the controversy, it does criticise most previous studies of the subject for implicitly assuming a pro-fluoridation consensus among scientists and thus treating the issue asymmetrically (support of fluoridation was taken for granted as adhering to the scientific truth, while opposition to it had to be explained through social factors). The article also points out that the pro-fluoridation side was in a position of power, which it used to control the extent to which its opponents were able to express their views, whereas the other side was not influential enough to do the same. While Martin never explicitly declares his solidarity with the ‘underdog’, the impression that he does so between the lines seems justified, especially considering his later contributions to more explicit debates about the neutrality or commitment of social science researchers⁵⁴, which I will discuss later. Another discussion paper by Stephen Hilgartner (*SSS* 1990:3) two years later makes a similar point on a completely different topic: Where Martin criticises positivist accounts of controversy for siding with the powerful, Hilgartner takes issue with the dominant view of popularisation of scientific knowledge as not only being theoretically and empirically inadequate, but also being a political tool in

⁵⁴ It is curious to note that Martin’s partisanship still remains implicit in the *SSS* article discussed here, while these later, more explicitly political contributions were primarily deemed appropriate – by Martin himself or by the editors of the journals? – for publication in *STHV* (Scott et al., *STHV* 1990:4; Martin et al., *STHV* 1991:2; Martin, *STHV* 1993:2), whereas they are taken up in *SSS* only much later (Martin, *SSS* 1996:2).

the hands of scientific experts who want to establish a hierarchy between their own knowledge and that of laypeople. At least from a retrospective angle, these articles thus already hint at, but do not quite make explicit, some of the issues that would become a topic for much theoretical reflection in the early 1990s.

VI.2.2 The Debate in STHV

While debates about normativity and the question of whether or not STS researchers should take a stand on the issues they write about only became an issue in *SSS* in the early 1990s, it has been one in *STHV* all along – or, more accurately, has been an occasional topic for reflection from the beginnings. Overall, *STHV* is characterised by much stronger activist leanings than *SSS* during the 1980s – whereas *SSS* mostly seems to be content with the role of a disengaged scholarly journal studying science (and, sometimes, technology or even medicine) and hardly any reference is made to the potential societal or political relevance of this kind of research, societal relevance seems to be one of the central goals of the *STHV* enterprise. As I have already hinted at in chapter III, this is especially obvious when comparing the editorials published in the two journals during this time period – while nearly every editorial in *STHV* during this period picked up on and discussed some issue of societal relevance, editorials in *SSS* were few and far between, usually reporting upon changes in the editorship or format of the journal. It is also apparent, however, when comparing the articles themselves. Not only was the issue of whether, to what extent, and how STS research should strive for normative relevance or political application already a topic for explicit reflection in the early 1980s – a subject I will return to shortly – but many articles that do not tackle such issues on an abstract level nonetheless find the author clearly taking a stand in a debate. For example, many – although not all – of the articles dealing with environmental or anti-nuclear movements in the early 1980s are clearly sympathetic towards these movements, and a special issue from 1982 on the debate about nuclear weapons not only reprints excerpts from a pastoral letter by the U.S. National Conference of Catholic Bishops (in which the bishops demand nuclear disarmament), but also lists the contact details and activities of various religious groups (mostly of Christian persuasions) active against nuclear weapons and for world peace (Anonymous, *SSS* 1983:3b). Even without the inclusion of an introductory paragraph by the editor referring to the pastoral letter as “[m]ore than simply a dogmatic statement of one religious organization,

the pastoral letter represents a vigorous new current in moral discretion and responsibility” (Anonymous, *STHV* 1983:3a, p.14), it would still be hard to miss that the editor was siding with the anti-nuclear weapons side of this societal debate. In a similar fashion, a commentary on the movement against nuclear war in 1981 also lists “some key groups in the movement against the nuclear war” (Casper/Krauss, *STHV* 1981:4, p. 26) in a box next to the article. A similar story can be told about a special section on creationism published in 1982 (*STHV* 1982:3), which quite clearly supports the proponents of evolutionary theory – among some legal documents and comments by members of the STS community, some of the attorneys who supported the ACLU (American Civil Liberties Union) in their struggle against the teaching of creationist science in schools in Arkansas are also given space to explain their position and reflections on the lawsuits, as is Michael Ruse, the philosopher and historian of science who served as an expert witness for the attorneys of the ACLU. While his article (Ruse, *STHV* 1982:3) is met with some criticism from another philosopher and historian of science, the point of criticism is not that he had sided with the wrong side, nor that he should not have sided with any side at all, but is quite explicitly criticism coming from someone who supports the same cause. Specifically, the charge against Ruse (who had entitled his article ‘Creation Science Is Not Science’ and had employed rather traditional demarcation criteria of logical empiricism to argue his point) is of “perpetuating and canonizing a false stereotype of what science is and how it works” (Laudan, *STHV* 1982:4, p.19), with the result that the “victory in the Arkansas case was hollow” (ibid.). Nonetheless, there appears to be no disagreement within the pages of *STHV* over which side should win in the creationism trials, nor over the fact that this is a fight worth fighting. To pick a third example, a special section on ‘Technology and the Arms Race’ in 1990 is led off by an introductory statement written by the guest editor, Patrick Hamlett, in which he is almost apologetic about the fact that “[t]hese articles [...] reflect a *realpolitik* perspective common in the national security field. These authors assume that security must ineluctably be sought in a world of independent and largely anarchic nation-states, each pursuing national self-interests as each sees those interests.” (Hamlett, *STHV* 1990:4, p.381; italics in the original) In combination with his earlier emphasis on the fact that the articles contained in the special section were all written by political scientists, it is difficult to shake off the impression that Hamlett feels the need to apologise to his audience of *STHV* readers for the fact that the articles (all of which are, to varying degrees, in favour of

control of the qualitative arms race) are not more radically pacifist. Again, it seems to be suggested that the most obvious role for an STS researcher would be to support the peace movement.

While plenty of explicit as well as between-the-lines partisanship can be found in the 1980s issues of *STHV*, some more theoretical discussion about possible societal applications of STS research and about normativity is also to be found. A fair amount of it is grouped around a plenary discussion sponsored by *STHV* at the joint gathering of the *History of Science Society* (HSS), the *Philosophy of Science Association* (PSA), the *Society for the History of Technology* (SHOT), and the *Society for Social Studies of Science* (4S) in Toronto 1980. The session was held under the title ‘The Potential and Limits of Applied History, Philosophy, and Social Studies of Science’, and several papers and comments that were either originally presented at this session or referred to it were printed in *STHV* during the years 1980 to 1982. Unlike the debates that were characteristic for the 1990s, which I will discuss later, the focus of discussion here is not so much about the possibility or impossibility of remaining neutral as a researcher or about the desirability of being politically relevant (although these issues are touched upon, too), but rather on the possibility and limits of providing useful knowledge for policy purposes. This topic is discussed quite controversially. While one author sees the possibilities of applied philosophers of science mostly in their contributions to technology assessment (Shrader-Frechette, *STHV* 1980:4), another one thinks that history of science could be “brought to bear on current socio-political problems involving science and technology” (Graham, *STHV* 1981:1, p.5), e.g. through the inclusion of historians of science in science advisory boards. Another author, discussing the possibilities of an applied ethics, is rather cautious about the idea of a “mechanical application of available theories to the nitty-gritty of medical practice” (Caplan, *STHV* 1980:4, p.26) and would rather see ethicists take on a role other than that of an expert technician who provides such applied policy advice: “Theoretician, diagnostician, educator, coach, conceptual policeman, and skeptic are all supplemental or alternative roles to that of technician.” (*ibid.*, p.30)

Dorothy Nelkin is even more critical of the idea of an applied ethics, voicing her “own uneasiness with the field of applied ethics, especially with the idea that skill in philosophical analysis and command of ethical arguments provides special competence to

evaluate public policy” (Nelkin, STHV 1981:1, p.16). In her opinion, “the role of social and philosophical studies of science and technology is neither to manipulate behavior, nor to reveal the rights and wrongs of policy choice” (ibid., p.17), but rather, “to draw a coherent picture of what is in fact going on” (ibid.).

Marx Wartofsky also argues against narrow applications of social scientific knowledge to policy making, but unlike Nelkin, who – without actually using that buzzword – seems to propose a more neutral role for social scientists, remains in favour of a deliberately critical role for what he refers to as ‘metascientists’: “What I am proposing, in effect, is that the metasciences are in no special way fitted to advise or formulate in the domain of applied social questions of science – no more than movie actors or sports heroes are fitted to make pronouncements upon, or to give leadership in, matters of public policy. What they *are* fitted for is that second-order critique of the forms and practices of applied rationality, and of that distillation of pure rationality which carries this history and practice of application into its universalized and abstract form.” (Wartofsky, STHV 1980:4, p.21; italics in the original)

Nicholas Steneck argues the exact contrary, because, after all, “[m]ost policy-makers do not need (or want) yet another conscience looking over their shoulders while they make decisions” (Steneck, STHV 1982:3, p.106); what they need instead is “workable models and useful facts” (ibid.): “Policy-making is a mundane, down-to-earth activity. As a consequence, the degree to which history can succeed in aiding policy-making will depend on the degree to which it can function as a fact-gathering skill and not as a provocative intellectual discipline. [...] History provides complete, accurate, and unbiased reconstructions of the past, whether that past is centuries old or yesterday. On this basis, applied history can and should be justified.” (ibid., p.111)

What Steneck does not appear to be interested in arguing here is *why* such an applied history should be justified – “improving the human condition” (Graham, STHV 1981:1, p.6), which most of the other commentators in this debate present as their noble goal, does not appear to rank very highly on his agenda. The reader is thus left with the impression that career opportunities for historians and societal recognition of the fact that “history must be done by historians” (Steneck, STHV 1982:3, p.105) are the focus of his concern – a proposition that is only logical once one accepts that history has exclusive access to

‘complete, accurate, and unbiased reconstructions of the past’. That such a view is in harsh contradiction with much STS work of the last few decades probably goes without saying – it is interesting to note, then, that it was still deemed fit for (uncommented) publication in *STHV*.

To recapitulate, the debate of the early 1980s rather controversially deals with the possibilities and limits of policy involvement of philosophers and historians of science. The issues of disagreement are whether it is desirable for a social scientist to get involved in policy questions at all, as well as which role they should adopt if they get involved – that of an expert who brings facts or concrete policy advice to the table, or that of someone who assumes a sceptical, critical position vis-à-vis science and policy. What both of these perspectives have in common, if to varying degrees, is an interest in becoming a voice *within* the existing science and technology policy structures, rather than expressing a desire to radically change these structures or looking for opportunities of social or political involvement outside of the traditional policy-making realm. This fact is criticised by Sal Restivo, who accuses the participants of the ‘Potential and Limits of Applied History, Philosophy, and Social Studies of Science’ debate of upholding scientific orthodoxy and acting as “apologists and ideologists for science-as-it-is and technology for the higher circles” (Restivo, *STHV* 1981:1, p.20). Picking up the metaphor – coined by Loren Graham and employed by several of the commentators in the debate – that historians should learn “to dance modern ballet” (Graham, *STHV* 1981:1, p.6), Restivo proposes they learn “experimental dance” (Restivo, *STHV* 1981:1, p.22) instead. With that, he intends to move away from the “liberal-reformist politics that dominated the discussion of applied science and technology studies” (*ibid.*), and shows concern about his colleagues’ “failure to acknowledge the difference between seeking to become a viable voice within the existing power structure, and opposing that power structure because it is at the roots of the problems they are trying to solve” (*ibid.*).⁵⁵ In a follow-up paper, Restivo outlines the

⁵⁵ Curiously, Restivo is not the only one who associates the contributors to the debate with liberal political values, as unexpected support comes from Dorothy Nelkin. While Nelkin argues the exact contrary from Restivo – where Restivo is in favour of more radical social involvement, Nelkin recommends a disinterested academic perspective – she also argues that her colleagues who claim to provide objective factual advice for policy-makers are in fact “advocates of liberal ideology” (Nelkin, *STHV* 1981:1, p. 17). Unlike Restivo, Nelkin takes no issue with these liberal values, but does ask for them to be made explicit: “While most of us academics agree with their liberal values, let’s call a spade a spade and not mask advocacy in the guise of objective conceptual analysis.” (Nelkin, *STHV* 1981:1, p.17)

‘weak programme’ in the sociology of science, developed by him and Daryl Chubin in contrast to Bloor’s ‘strong programme’, which he accuses of failing to “entertain the possibility of alternative sciences – or of alternatives *to* science” (Restivo, *STHV* 1981:2, p.26; italics in the original). The weak programme, on the other hand, moves its focus away from science and to the broader concept of inquiry, and thus doesn’t give modern science any a priori privilege over other systems of inquiry. A “concern for ethical and moral issues and problems and a commitment to liberatory values” (ibid., p.28) is central in the weak programme. Throughout the 1980s (apart from the two aforementioned papers, two additional articles – Restivo, *STHV* 1986:1 and *STHV* 1987:2 – also accuse his fellow STS scholars of failing to question the ideology of modern science), Restivo seems to take the role of resident anarchist at *STHV*, continually urging his colleagues to become more socially aware and more politically radical. These calls to action are met with very little resonance – for the most part, the orientation of scholars publishing in *STHV* during the 1980s who aim to leave their footprints outside of the academic community is one of providing policy-relevant insights and comments, rather than one of radically altering the ways in which science and politics are made.

One commentary from 1985 shares neither Restivo’s revolutionary aspirations, nor the concern of many of the aforementioned articles of making STS knowledge applicable for policy-making, but rather seems to anticipate some of the points of discussion that would come to the fore in the early 1990s. Under the heading ‘It’s Good Enough for Science, But Is It Good Enough for Social Action?’, Ditta Bartels sets out to “show how the insights of the constructivist view of scientific knowledge [...] can throw light on how science is used in the political domain” (Bartels, *STHV* 1985:4, p.69). She complains that, so far, “the sociologists in this field have not examined extensively the social usefulness of their particular type of social science” (ibid., p.70). This usefulness lies in contributing to public recognition of the fact that “scientific knowledge is not objective, and hence cannot serve as the undisputed, rock-hard base upon which potentially discriminatory political decisions may be erected” (ibid., p.73). In this view, the social responsibility of STS researchers is not to contribute specific policy advice, nor to radically change the political and scientific institutions of our society, but rather, to deconstruct knowledge claims and de-mystify allegedly objective scientific knowledge.

With this line of argument, Bartels explicitly derives the social relevance of STS researchers not from their ability to contribute to policy-making, nor (exclusively) from the relevance of the subject matter they research, but rather from the particular scientific approach taken. While the approaches discussed above – with the exception of Restivo’s weak programme – tend to either presuppose a positivistic view of (social-)scientific knowledge (where historians or philosophers are called upon to provide accurate knowledge for policy-makers), or work regardless of positivistic or constructivist views of science (where social scientists are called upon to provide more abstract critique of science and policy questions), Bartels explicitly links the normative role of STS to a constructivist view of knowledge and wonders what the political implications of constructivism are. In this sense, she discusses an issue that would later become a more central concern in both of the journals under investigation.

VI. 3 A Normative Turn?

From this point onwards, I will not discuss the debates in *SSS* and *STHV* separately, as they share many of the same focal points and frequently relate to each other. Unlike in the 1980s, the 1990s are not marked by fundamental differences in the way the two journals treat issues of normativity, social involvement and political relevance. It is indicative that the same questions Ditta Bartels discussed in *STHV* in 1985 are picked up on in 1990 by authors in both journals: In an off-hand comment in an article in *SSS*, David Turnbull (*SSS* 1990:2) discusses the normative relevance of a constructivist approach to the study of technology, while in *STHV*, Scott, Richards and Martin (*STHV* 1990:4) raise similar questions about the use of the ‘symmetry principle’ in controversy studies of science. Both of these articles, despite their differences, raise a similar question: what it means, politically, to adopt one particular academic approach over another. This question has been given much room in both journals during the period of 1990 until about 1998, and I will attempt to give an overview over the developments in both journals for this time period in the following section. For reasons of clarity and readability, I have decided to split it into separate sections outlining different thematic strands.

VI.3.1 Critical Potential or Normative Deficit? The Political Relevance of Constructivism

This first sub-section about the debate in the early/mid-1990s focuses on the strand of discussion concerned with the normative relevance or normative deficit of STS, in particular of constructivist approaches. In a comment on malaria vaccines, David Turnbull is the first to remark on this issue, arguing that the task of an STS researcher is “not merely to understand the world but to change it” (Turnbull, SSS 1990:2, p.383). Furthermore, Turnbull remarks that “this can best be achieved by working within a constructivist framework, rather than a technologically determinist one, where the difficulties of asking the question ‘in whose interests is it?’ all too often conceal the answer that it is in the interest of those who control the means of production of the technoscientific knowledge/practice at issue.” (ibid.)

However, while Turnbull sees the adoption of a constructivist approach to the study of technology as a prerequisite for a politically relevant STS, others are less enthusiastic about the critical potential (or at least the critical practice) of constructivism. The most damning critique of the critical potential of social constructivist approaches is voiced by Langdon Winner in 1993. After acknowledging several positive features of a social constructivist approach, Winner pauses “to ask whether or not their approach does amount to an improvement over other approaches. Before we forget our Marx or our Mumford, Ellul, or Heidegger, it is important to notice what one gives up as well as what one gains in choosing this intellectual path to the study of technology and human affairs.” (Winner, STHV 1993:3, p.367) For Winner, adopting a constructivist approach has “a significant cost: a willingness to disregard important questions about technology and human experience” (ibid., p.368), as questions about the political impact of technologies are not part of the social constructivist approach. However, according to Anne-Jorunn Berg and Merete Lie, this amounts to a confusion of “a critique of theory with a critique of practice” (Berg/Lie, STHV 1995:3, p.333), as the fact that most constructivist scholars have failed to deal with questions of normative relevance – in their case, feminist questions – does “not necessarily imply that constructivist concepts are useless or that constructivism cannot serve as an analytical instrument for gender studies of technology” (ibid., p.345). Indeed, the authors conclude that there are several important meeting points between feminism and constructivism, and that dialogues between the two “offer the possibility of learning more

about gender and technology relations” (ibid., p.347).

In an article published in 1992, Hans Radder (SSS 1992:1) also finds that normative reflections have been largely neglected in recent STS work, especially in constructivist approaches, and proposes more reflection upon the normative implications of STS research by paying attention to non-local values and broader patterns in the development of science and technology. In the same year, Christopher Hamlin suggests quite the contrary when he states that STS scholars “need not spend so much time in front of the reflexive mirror” (Hamlin, SSS 1992:3, p.537) and should instead contribute by “intervening intentionally into the enterprises of science and technology to produce better products, whether these be better policies, more capable college graduates, or more creative and more socially responsible scientists and technologists” (ibid., p.533). Despite the seeming contradiction of these two arguments, they do both share a common concern that STS is not realising its full normative and political potential. This is not to obscure the fact, of course, that the alternative strategies the two authors propose – reflecting more on normative implications vs. stopping to reflect already and going out to change things – could not be any more different.

Meanwhile, in *STHV*, Ronald Giere worries about the lack of policy-relevant insights that can be gained from constructivist approaches to the study of science: “Social constructivists have had precious little to say about science or technology policy. I suspect this silence is not an accident. If one takes seriously the position that science and technology are social constructs, the only policy advice one can give is to improve one’s use of the rhetoric of science and technology to persuade others of one’s point of view and to build cohesive social networks.” (Giere, *STHV* 1993:1, p.109)

Wiebe Bijker’s article in the same issue of *STHV* couldn’t possibly express a more different view. Bijker argues that it is precisely constructivism that opens up new possibilities of intervention into technical change. A determinist view of technology leaves little room for interventions into technological development, because “either it is too early to foresee the implications of a new technology, or it is too late to intervene because the technology has become so entrenched in society and culture that it cannot be changed anymore” (Bijker, *STHV* 1993:1, p.129), while a more constructivist view opens up new

possibilities of technological choices and “the possibility of continually shaping and reshaping a technology, during all its stages of development, is recognized” (ibid.). While Bijker and Giere thus disagree on the political potential of constructivism, they apparently do agree that constructivists have not done much concrete policy- or politics-relevant research (yet, Bijker would add). Bijker would probably also agree with Giere that this has not been ‘an accident’, but he proposes an altogether different explanation, presenting “the science and technology studies of the 1980s as an academic detour to collect ammunition for the struggles with political, scientific, and technological authorities” (ibid., p.116). According to Bijker, the detour has probably been long enough by now and it might be time to return to the main route by starting to “relate present findings in science and technology studies to political issues of democratic control of science and technology” (ibid., p.117). Unlike for Giere, the main concern for Bijker is clearly not so much a contribution to policy decision-making, but rather one to an ongoing democratisation of society. As I have already hinted at in the account of the debate around applied STS in the 1980s, these two different types of ‘engagement’ are both discussed under the banner of normative relevance, but their implications are quite different. In the view of Brian Martin, who, like Bijker, sees his roots in the radical science movement of the 1970s, activist leanings tend to be frowned upon in STS, but the same is not true for involvement in policy-making. According to Martin, this might be because involvement in policy-making tends to bring “large contracts and consultancies from government and industry, and lots of status as being the people dealing with the ‘real world’” (Martin, STHV 1993:2, p.251), whereas involvement with community activists “smacks of making a social commitment [...]. Apparently, commitment to one’s career or serving powerful groups is honored, but commitment where there is no money is suspicious.” (ibid., p.252) Unsurprisingly, Martin’s proposal is to strengthen ties with activists, asking them for their opinions on relevant research topics as well as writing in a more accessible style in order to reach non-academic audiences, and indeed, also joining social action groups.

One question that ranks highly on the agenda of normative debates is whether an application of STS knowledge and methods to issues of social and political relevance is already due or still premature. As mentioned above, this question has been addressed by Wiebe Bijker in his deliberations about whether or not it is already time to end the

‘academic detour’ of STS and return to issues of normative relevance, with the implication that, yes, it is about time. On the other hand, in his reply to the discussion paper by William Lynch and Ellsworth Fuhrmann (STHV 1991:2) in which they want to resurrect Marxist teachings, Michael Lynch comes to a different conclusion, arguing that “the demand for a normative SSK seems premature, or worse, regressive, because it ignores the failure of positive social science to achieve agreement on the most basic policies of theory and method” (Lynch, STHV 1992:2, p.232). In this view, STS would better not strive for normative relevance until every last theoretical and methodological issue has been resolved consensually and theoretical maturity has been accomplished. Brian Martin advocates the opposite view: “Well, yes, I agree that it would be desirable to develop a critique that is both epistemologically sophisticated and socially relevant, and also self-critical about its method and social location. I look forward to analyses that fulfill all these specifications. But for those of us who are not superhuman, I suspect it is more appropriate to set less exalted goals.” (Martin, STHV 1993:2, p.257) Martin leaves no doubt that his priority lies on tackling issues of normative relevance, even if it means that some theoretical issues remain unresolved for the time being.

VI.3.2 Symmetry and Neutrality

In 1990, Pam Scott, Evelleen Richards and Brian Martin (STHV 1990:4) would publish a paper in *STHV* that would dominate the debate on normativity in both journals for years to come. Under the title ‘Captives of Controversy’, the three authors present three cases of controversy studies in which the adoption of a principle of symmetry (one of David Bloor’s (1991) tenets of the ‘strong programme’, as outlined in chapter IV) has led to the ‘capturing’ of the controversy analyst by one of the sides in the controversy – specifically, by the side of the ‘underdog’. This should come as no surprise, Scott, Richards and Martin argue, because “an epistemologically symmetrical analysis of a controversy analysis is almost always more useful to the side with less scientific credibility or cognitive authority. In other words, epistemological symmetry often leads to social asymmetry or nonneutrality.” (ibid., p.490). As the authors argue in this paper as well as in a follow-up the next year (Martin et al., STHV 1991:2), this is not a problem in itself – rather, it is something that every researcher should be aware of when (not) choosing a symmetrical approach to controversy analysis. The methodological implication is that the researcher

will usually be more likely to gain access to information from the side with less scientific credibility and will be treated reluctantly by the other side. The political implication, according to Martin, Richards and Scott, is that the researcher will not be able to maintain a neutral, disengaged position in the controversy, but will be drawn into the debate as a participant. In a reply to this paper, Harry Collins agrees with the dilemma that Scott, Richards and Martin (SRM) are describing: “In particular, they are right in saying that the ‘neutral’ analyst will be thought of by participants as being on the side of the underdog. Thus however neutral the analyst intends to be, the work will always be drawn into the debate. Only SRM’s conclusion – that attempting to be neutral is pointless – is wrong.” (Collins, *STHV* 1991:2, p.249)

Collins argues in favour of maintaining neutrality as a methodological prescription, arguing that it is possible and desirable to “try to be methodologically neutral while accepting that one’s work will have an asymmetrical impact on the world” (*ibid.*, p.250). If the researcher doesn’t want to bear the responsibility for the impacts of a particular research project, they can always make the private decision to pull out of this research, but not even striving for neutrality in research means to undermine one’s own work, says Collins. Scott, Richards and Martin do not see this danger, and instead warn that Collins’ approach falls danger of “becoming only an intellectual exercise for armchair philosophers” (Martin et al., *STHV* 1991:2, p.225).

The principle of symmetry has also given rise to concerns about the normative relevance of STS outside of the ‘Captives of Controversy’ debate: In 1991, William Lynch and Ellsworth Fuhrmann single out the symmetry principle, with its prescription for neutrality, as an indicator for a “retreat from normative reflections” (Lynch/Fuhrmann, *STHV* 1991:2, p.235) in current sociology of scientific knowledge and suggest returning to the early writings of Karl Marx to recover the normative relevance of the sociology of scientific knowledge. Yet the main strand of the debate about symmetry is undoubtedly the one aroused by Scott, Martin and Richards, which was revisited in a 1996 special issue of *SSS* on neutrality and commitment, based on a symposium dedicated to the ‘Captives of Controversy’ debate and involving essentially the same participants.

This *SSS* special issue carries on some of the themes that were discussed in *STHV* at the

beginning of the 1990s; however, the issues have also transformed somewhat during those years. While Harry Collins had previously agreed with Scott, Richards and Martin's description of the neutrality dilemma – that a researcher attempting a symmetrical analysis of a controversy will usually be drawn into the controversy as a proponent of the underdog's point of view – and had only disagreed with their conclusions – that researchers should therefore give up striving for neutrality and embrace their partisan role – he now no longer thinks that this is necessarily the case. He accuses Martin, Richards and Scott of practicing a 'commitment to commitment', which "saves the trouble of arguing for the cause [of commitment] itself" (Collins, SSS 1996:2, p.231). Collins now stresses that he is not opposed to all commitment, only to commitment for the sake of it. Nonetheless, Collins also argues in favour of neutrality – not because he thinks that full neutrality can ever be reached, but because striving for more neutrality makes sense as a methodological prescription.

Brian Martin also seems to back-pedal a little in his contribution, stressing that he does not wish to "argue for intervention as an inherently superior approach" (Martin, SSS 1996:2, p.267), but rather as one possible course of action, useful in particular contexts, less so in others: "Intervention should be recognized and used as part of the repertoire of social scientists studying science. [...] The implication is that the science studies community should support a variety of noninvolvements and involvements." (ibid.) Martin also goes to great lengths to show that a prescription for neutrality does not, in fact, follow from the tenets of symmetry and impartiality, as these tenets refer only to the style of explanation chosen by the researcher, but do not imply that the researcher cannot hold any beliefs of his or her own. Martin seems to be on a similar wavelength here as Dick Pels (SSS 1996:2), whose comment also discusses the SSK tenets and who criticises Scott/Richards/Martin as well as Collins of casting the debate in false dichotomies of involvement/detachment or partisanship/neutrality. Pels wants to abandon these dichotomies as well as the tenets of SSK, arguing in favour of 'distance' and 'autonomy' rather than 'symmetry' and 'neutrality'. He wants to "differentiate between different *forms* of partiality and different *fields* of politics, and to acknowledge the fruitful (indeed classical) intuition about *critical distance* which the protocols of symmetry and impartiality inadequately encode." (ibid., p.292; italics in the original)

Meanwhile, several authors question the notion of ‘taking sides’ in a controversy: Malcolm Ashmore points out several scenarios where STS might end up on the ‘wrong side’, because “the question of which side remains or becomes the under- or overdog is a function of the outcome of the process” (Ashmore, SSS 1996:2, p.315) and not known in advance. Moreover, scientific authority is not always on the ‘side’ of the economically or politically powerful, so if STS always sides with the scientific underdog, it might end up on the side of the socio-economic overdog. If STS wants to avoid ending up on a side it is not comfortable on, Ashmore proposes the strategy of reflexivity, as “a reflexive study will seem *self*-undermining to all concerned and therefore of no use to anyone” (ibid., p.307; italics in the original). Ashmore also calls into question the grouping of epistemological radicalism with politico-moral neutrality on the one hand and of politico-moral partisanship with epistemological asymmetry on the other hand: “Politico-moral radicalism should be conceived, not as partisan, not as on a side, not as endorsement, but as a critique of dominance, a nay-saying refusal to accept the ruling relations. Intellectual (epistemological) radicalism, as the effort to extend scepticism as far as possible (and further), to doubt the indubitable, to unsettle the certainties of science, common-sense and self-evidence” (ibid., p.316).

Both Brian Wynne and one of the original authors of the ‘Captives of Controversy’ paper, Evelleen Richards, also call into question the notion of ‘sides’. Richards points out that her intention has never been to champion one side, but rather to use her “analysis of the dispute and its larger institutional and social context to inform a social critique of scientific methodology” (Richards, SSS 1996:2, p.341). Her intention was thus neither to remain neutral, nor to side with one of the ‘sides’ in the controversy, “but rather to stake out *another* position, a position informed by my SSK analysis” (ibid., p.340; italics in the original). With this approach, the criticism voiced by Brian Wynne of the original article published by herself, Scott and Martin as well as of Collins’ counter-position seems to evade her: “Both parties assume that public issues involving science and technology are adequately conceptualized in terms of the ubiquitous model of controversy, with its typical sharp polarization and congealment of ‘sides’. In addition, and perhaps most important, they display an underlying commitment to an exclusively decisionist model of society” (Wynne, SSS 1996:2, p.359).

Like Richards, Wynne neither sees the point of “taking (nor reifying) sides, nor also pretending to a spurious neutralism” (ibid., p.360); instead, he sees the position of STS as a ‘critically engaged’ one, as it can “constructively call into question taken-for-granted definitions of the identities and interests of actors involved in real public issues such as global environmental risks, and thus to call into question taken-for-granted definitions of what is at issue” (ibid.). A year after the publication of the special issue, Brian Martin publishes an article in which he also proposes a type of political commitment that does not consist of simply ‘taking sides’ in a controversy: He makes a plea for including a utopian dimension to science and technology studies (his own example being non-violent, community-oriented alternatives to the military), something which he finds generally missing in constructivist as well as technological determinist studies, thus resulting in a “conservative orientation, in that they affirm that which exists and offer no analytical means for focussing attention on what might exist in a different society” (Martin, SSS 1997:3, p.456).

VI.3.3 Truth as a Touchstone of Political Action

A third strand of debate about the normative relevance of STS, next to discussions about constructivism and symmetry, relates to the issue of relativism and the possibility of normative action in the absence of truth. The point is made most bluntly by Nils Roll-Hansen in an article in which he expresses concern about communication failures in efforts to protect the environment, arguing that mistaken policy decisions are made because “arbitrary and superficial impressions and opinions become the basis of action because knowledgeable experts are not properly consulted” (Roll-Hansen, STHV 1994:3, p.325). While relativism (or indeed, STS perspectives) are not at the centre of his reflections, Roll-Hansen mentions in passing that “sceptical and relativist views of science can promote an intellectual climate favourable to wishful thinking. Such attitudes do not directly support specific prejudices and biases, but they favor them indirectly by undermining the motivation and efficiency of criticism.” (ibid., p.336) Kate Soper makes a similar point in a paper about the relations between feminism and ecology, stating that a realist position is “the only responsible basis from which to argue for any kind of political change” (Soper, STHV 1995:3, p.312). On the other hand, Soper also sees the dangers of such a realist approach, and thus advocates that both realism and relativism should be more conscious of

what their discourses ignore or repress. Vicky Singleton faces a similar dilemma in trying to combine postmodern theory and its rejection of essentialism with a political and moral commitment to feminist values. She is disappointed with the fact that “feminist approaches to science presently facing this postmodern dilemma frequently seek a resolution by adopting an epistemologically conservative standpoint that, for example, asserts the political impotence of approaches (such as actor-network theory in the sociology of scientific knowledge [SSK]) characterized by a rejection of dualism and universalism.” (Singleton, SSS 1996:2, p.446) After a lengthy discussion of the problems she faced when trying to translate her own research on cervical smear tests into normative recommendations for women confronted with the decision of taking or refusing these tests, she concludes that the answer has to be given not in terms of what women *should* do – as this “seems to be based in a discourse of oppression and domination” (ibid., p.462) – but rather in terms of what they *could* do, which leaves room for ambivalence and negotiation and is thus compatible with a rejection of essentialism, while still making a normative point. This approach is later criticised by Hans Radder (SSS 1998:2), who finds that Singleton is sidestepping the real question here and could have tackled this issue by giving prescriptive answers, but making them context-dependent (thus, answering not what *all* women should do, but nonetheless giving recommendations what women *under certain circumstances* should do). The fact that she was unable to give such an answer marks a logical outcome of an application of an actor-network theory (ANT) approach to him. Singleton agrees that ANT cannot tell us what to do, but sees this as a strength, rather than a weakness, of the approach, because “it exposes how difficult it is to say what to do” (Singleton, SSS 1998:2, p.336). Brian Wynne also chimes in, arguing that one “does not have to reach substantive normative conclusions in order to make a normative contribution” (Wynne, SSS 1998:2, p.339), and that calling into question things that are usually taken for granted is also part of a normative STS agenda.

The charge of relativism being doomed to political irrelevance is also discussed by Steve Woolgar and Keith Grint, who reject the idea that “political action requires a touchstone of analytic realism” (Grint/Woolgar, STHV 1995:3, p.301), insisting that relativism is just as capable of political application as realism. Langdon Winner, one of the spearheads in the critique of social constructivism as politically void, appears to agree with the position that

political action need not be rooted in a notion of ‘truth’: “As regards technological decision making, I believe that the challenge is not that of how to impose universal standards of judgement clarified by ‘liberal metaphysics,’ but how to expand the social and political spaces where ordinary citizens can play a role in making choices early on about technologies that will affect them.” (Winner, STHV 1994:1, p.107)

In a 1998 article on participatory design, Marc Berg also stresses that “[n]ormative positions do not need essentialist grounds” (Berg, STHV 1998:4, p.483). Quite on the contrary, he argues that abandoning dichotomies such as ‘authoritarian’ and ‘democratic’ technologies or ‘technology’ and ‘human work’ “might actually strengthen the political leverage of these approaches in new ways” (ibid., p.477), as it does not limit the political issues at stake along one predetermined axis (e.g. democratic/authoritarian). For example, it allows the researcher to ask not whether the internet decreases or increases democracy, but rather how it might transform what we mean by ‘democracy’.

The question of how “a sceptical and reflexive stance in relation to scientific knowledge [can] be reconciled with making authoritative recommendations for social policy” (Jasanoff, SSS 1996:2, p.393) also concerns Sheila Jasanoff. In her experience, many science studies scholars find the idea that their discipline might be able to contribute to politics highly suspect and that “even a bounded relativism about particular forms of scientific knowledge [...] strikes many as being incompatible with normative analysis, let alone with prescriptions for social change” (ibid., p.395). This need not be so, argues Jasanoff, if STS becomes more reflexive in its choice of research sites, style of explanation, and methods of expressing normative positions and prescriptions.

Recapitulation: A Widening Gap?

In summary, the early and mid/late 1990s have been characterised by much systematic and programmatic discussion of the normative role and position of STS. In some ways, the debate resembled that of the early 1980s (which had been mostly dormant throughout the rest of the 1980s), when several scholars were also concerned with ways of applying STS to practice and normative action. However, unlike in most of the articles published in the 1980s, the link between particular theoretical approaches and normative implications of research has now become an explicit topic of reflection. The focus was less strongly on finding ways to apply STS knowledge and advice to policy making, and more on making

contributions to a thorough democratisation of society – although of course both of these concerns existed (as they did in the 1980s, with a different relative strength), and are in no way mutually exclusive anyway. Several issues that have arguably been looming over the field for some time have become topic of explicit theoretical discussion, spread out over many articles, discussion papers and indeed even special issues in the two journals. The debate has sometimes become quite heated, and in a 1993 editorial of *STHV*, Susan Cozzens diagnoses a “widening gap that separates the scholarly pursuit of science and technology studies from the more activist stances of teachers and political organizers who consider themselves part of the science, technology, and society movement” (Cozzens, *STHV* 1993:3, p.275).

A year later, also in a *STHV* editorial, Olga Amsterdamska lists the dominating debates in the field of social studies of science and technology during the last few decades, identifying the 1970s with a debate of the relative merits of Kuhn and Popper, of the Mertonian sociology and the strong programme, of internalist and externalist accounts of the history of science; the 1980s, with the discussion of constructivism and technological determinism. In this sense, the 1990s belong to the discussion of normativity and political relevance: “Today, the debate focuses on the social and political relevance of the various approaches to the study of science and technology, and questions are being raised about the responsibilities of those of us who wish to understand, criticize, and perhaps influence the manner in which science and technology exert their power over the contemporary world. On the one side are those who accuse constructivists of academicism and political indifference [...] On the other side, we hear protests that political commitment cannot replace epistemological sophistication, and that ideological or moral indignation is no substitute for analysis.” (Amsterdamska, *STHV* 1994:1, p.3)

On the other hand, especially in the middle of the 1990s, several articles were published that question these dichotomies, indicating that perhaps the gap isn't insurmountable after all.

VI.4 Bridging the Gap?

After the intensive programmatic discussion about political engagement and normativity in the early and mid 1990s, the debate seemed to ebb away somewhat – perhaps a little earlier in *STHV*, where it had also reached its climax earlier (whereas the debate in *SSS* was at its most intensive in a 1996 special issue, most of the programmatic discussion had taken place around 1991-1993 in *STHV*). However, this is neither to say that the issue dropped from the agenda altogether, nor that the discussion had not left any traces.

Perhaps most immediately striking is the observation of a downright boom in the late 1990s of articles dealing with possibilities of citizen participation, both on an empirical level (often linked to developments in the biomedical and biotechnological field and to health issues⁵⁶) and on a more programmatic, theoretical level. On the programmatic level, Patrick Hamlett argues that, in order to “bridge the apparent gap between the program’s descriptive richness and its normative irrelevance” (Hamlett, *STHV* 2003:1, p.134) and to engage in intentional interventions to increase public participation in technological policy-making, constructivists would be well suited to look to two theoretical movements inside political science (deliberative democracy and participatory public policy analysis) for inspiration. While Hamlett’s starting point is the observation of a normative irrelevance of constructivist approaches (although he concedes that several constructivist scholars have already made steps in the right direction), he actually seems to belong to what Joseph Wachelder calls a general “trend to embrace a ‘strong’ model of democracy” (Wachelder, *STHV* 2003:2, p.248) and to advocate citizen participation in the STS field. Many of the papers embracing this ‘trend’ either take the need for democratisation for granted or argue it on political grounds; however, a paper by Robert Evans, Simon Guy and Simon Marvin dealing with different management approaches to urban infrastructure management favours

⁵⁶ A non-exhaustive list of examples include the development of a framework for evaluation of public participation methods (Rowe/Frewer, *STHV* 2000:1) as well as an evaluation of a deliberative conference on radiation dose assessment following such a framework (Rowe et al., *STHV* 2004:1), a report about experiments fostering public discourse on developments in genetics (Barns et al., *STHV* 2000:3), a theoretical discussion of public participation initiatives regarding bioethical questions (Kelly, *STHV* 2003:3), an article on public participation regarding tobacco regulation (Roth et al., *SSS* 2003:1), and a comparison of public discourse on humane genome research in Germany and Ireland, where one dimension is a comparison of the extent of societal participation in discourse production (O’Mahony/Schäfer, *SSS* 2005:1).

participatory projects for their compatibility with a constructivist view of knowledge, while predictive demand strategies go hand in hand with a traditional view of science. For them, “SSK is not about saving the planet [...]. Rather, the commitments that follow from SSK are to understand how science and technology are produced, used, and developed and to promote this understanding.” (Evans et al., STHV 1999:1, p.109) Nonetheless, the form of SSK they advocate is a non-neutral one, which acknowledges that “the view of knowledge that SSK promotes has, potentially, profound implications for policy making” (ibid., p.126).

This ‘trend’ towards participatory approaches has also given rise to some concern, expressed most clearly in the ‘Third Wave’ debate started by Harry Collins and Robert Evans in a 2002 discussion paper in *SSS* and carried on in a series of papers, taken from a symposium spawned by the paper by Collins and Evans, published in the same journal in 2003. Collins and Evans argue that science studies can be distinguished into three phases or waves: The First Wave was marked by an unquestioned authority of scientific knowledge in which scientific experts were believed to hold the truth, while the Second Wave, initiated by a “sociological turn” (Collins/Evans, *SSS* 2002:2, p.236) within science studies in the 1970s, pointed out the problems of legitimacy such a view entailed and showed that “technical decision-making can and should be widened beyond the core of certified experts” (ibid., p.237). However, while the Second Wave thus saved the problem of legitimacy, it now faces a problem of extension, exemplified by the question of how far participation in technical and scientific decision-making should be extended. It is now up to the emerging Third Wave, argue Collins and Evans, to tackle this problem by accepting that technical decision-making should not be the exclusive domain of scientific experts, but nonetheless drawing “a boundary around the body of ‘technically-qualified-by-experience’ contributors to technical decision-making” (ibid., p.238) and thus preventing that the “distinction between expertise and democracy” (ibid., p.269) is dissolved altogether. The Third Wave, then, is about finding “a rationale which is not inconsistent with the last three decades of work in science studies” (ibid., p.236); more importantly, it is about finding a normative theory of expertise and experience. The first step in this undertaking is to distinguish different types of expertise. A similar attempt was made by Stephen Turner in a discussion paper a year earlier, in which Turner posed the question, “What Is the Problem

with Experts?” and concluded that there are different types of experts, only some of whom are problematic in a democratic society, and that it is possible to grant cognitive authority to experts without “granting them some sort of absolute and unquestionable power over us” (Turner, SSS 2001:1, p.144), as what counts as an expert is “conventional, mutable and shifting” (ibid., p.145). Both Turner and Collins and Evans are thus concerned with finding a way to salvage the notion of expertise without subscribing to outdated and authoritarian ideas of what constitutes an ‘expert’; yet it was the paper by Collins and Evans that would lead to an extensive debate of these notions.

The ‘Third Wave’ paper was met with much criticism. According to their critics, Collins and Evans have misread the literature of what they deem to belong to Wave Two (Jasanoff, SSS 2003:3; Wynne, SSS 2003:3), attempted a misguided translation of concepts from laboratory-based controversies to issues of public decision-making (Jasanoff, SSS 2003:3; Rip, SSS 2003:3; Wynne, SSS 2003:3), and based their approach on a mistaken “assumption that public issues in which scientific knowledge is involved are only about propositional questions [...] and not also about public meanings” (Wynne, SSS 2003:3, p.402). Brian Wynne accuses them of misunderstanding the nature of the legitimacy crisis of science, arguing that it is “not caused only by the exclusion of underrecognized but legitimate forms of expertise from propositional negotiations about consequences. It is a legitimacy problem caused more by the undemocratic imposition of assumed meanings on the issue, and on the public, through an institutional scientific culture” (ibid., p.412). By failing to call this into question, Collins and Evans “reinforce western scientific society’s scientific cultural hegemony. [...] Yet it seems an essential responsibility for SSK to explore and put into question just these given boundaries, and presumptively imposed meanings, for society at large to address and learn to negotiate.” (ibid., p.412f.). Sheila Jasanoff also sees the role of STS not in creating boundaries between science and politics, but rather in showing “what is at stake in the making of such boundaries” (Jasanoff, SSS 2003:3, p.398). Of the three scholars replying to Collins and Evans, Arie Rip is the only one who concedes that “more ‘extension’, i.e. more participation by non-specialists, is not always better” (Rip, SSS 2003:3, p.419) and who agrees with their notion of experience-based expertise which should be recognised in the technical decision-making process; however, he finds their treatment of these issues “curiously disappointing” (ibid.) and their

proposals for recognising experience-based expertise hopelessly unclear.

While the debate about the 'Third Wave' is limited to a few issues of *SSS* in 2002/2003, an *STHV* special issue published in 2005 appears to follow a similar train of thought. In the introduction to the special issue, Robert Evans writes: "Despite the tendency amongst science studies scholars to urge, and even celebrate, the 'dethroning' and 'democratization' of science, the consequences of this should at least cause us to pause and ask if, despite what we know, there is a need to retain a delimited but still privileged role for science in society." (Evans, *STHV* 2005:1, p.13) The articles contained in the special issue do not presume to answer this question, but rather to provide a basis from which the discussion can begin, writes Evans. Unlike the papers on the 'Third Wave' in *SSS*, these articles are predominantly of an empirical, rather than a programmatic nature, but they nonetheless share the same type of concern.

What is curious to note about the 'Third Wave' debate is that, unlike in the 'Captives of Controversy' debate that marked the early and mid 1990s (and which, in Harry Collins, shares a key participant with the 'Third Wave' discourse), this debate finds both sides arguing in favour of an explicitly normative STS field. Although Collins and Evans do see preventing STS from becoming "a political discourse" (Collins/Evans, *SSS* 2003:3, p.449) as one of the goals of their contribution, they nonetheless make it very clear that they strive for a normative approach to STS, a concern that their counterparts in the debate certainly share.

Perhaps it is not altogether overstated, then, when Michael Lynch and Simon Cole assert that so many "calls have been made for research that *intervenes* in public controversies about science and technology [...] that it is fair to say that the field has undergone a *normative turn*" (Lynch/Cole, *SSS* 2005:2, p.269; italics in the original). Even the most urgent plea for a restructuring of the STS field and academia in general in order to "allow a more strongly normative strand of STS to flourish" (Woodhouse et al., *SSS* 2002:2, p.311) concludes that "there has been considerable shrinking of what once seemed a very substantial gap between those doing 'committed' scholarship and those doing avowedly 'neutral' or relatively nonpartisan scholarship" (*ibid.*, p.312). Indeed, the discussion now no longer seems to centre around the desirability of partisanship or social commitment, but

rather around the ways of a policy-relevant strand of STS research – a notion that had shaped the debate about normativity in *STHV* during the early 1980s, but that had then taken a back seat compared to more conceptually-oriented discussions and calls for socio-critical (rather than policy-oriented) relevance in the 1990s. The ‘Third Wave’ debate seems to indicate a new strand of discourse⁵⁷ focused on the possibility and desirability of providing policy-relevant insights on the basis of STS scholarship. The coexistence of socio-critical and policy-relevant STS research is also discussed by Wiebe Bijker, who writes that “STS now has three distinct routes to the future” (Bijker, *STHV* 2003:4, p.449): an ‘Academic Highway’, with its journals and curricula, a ‘Policy Street’, which provides research that is directly useful to the public and private sectors, and a ‘Democratization Boulevard’, which combines “long-term academic agendas with clear political and societal engagement” (ibid.). Bijker stresses the need for a peaceful cohabitation of these three routes, arguing that “all three routes need to be maintained at the institutional level in the field of STS” (ibid.).

There appears to be widespread agreement on the desirability of such a cohabitation of different styles of research: While Woodhouse and his co-authors acknowledge that “explicit partisanship is not a prerequisite for STS scholarship to be relevant to activists” (Woodhouse et al., *SSS* 2002:2, p.309), Collins and Evans point out in reply to Arie Rip that involving oneself as a researcher “is one way, perhaps the most direct way, to make a difference, but it is not the only way. Karl Marx, one of the most effective difference makers in history, just wrote books.” (Collins/Evans, *SSS* 2003:3, p.443) While there is thus certainly no consensus on *what* the political and normative agenda of STS should be, nor on the best strategies for implementing it, there is little evidence in the issues of *SSS* and *STHV* between 1999 and 2005 of any contestation that STS should have *some* kind of political or normative agenda, or at least be able and willing to accommodate approaches with such an agenda. While the first section of this chapter, which outlined different

⁵⁷ While it falls outside of the time-span covered by my analysis, an article published by Andrew Webster (2007a) in *STHV* in July 2007, in which he calls for a more ‘serviceable’ strand of STS that directly engages with science policy, and replies by Nowotny (2007), Wynne (2007) and Webster (2007b) continue this debate about the potentials of policy-relevant STS. Brian Wynne’s reply, in particular, touches upon the issue of a distinction between “the radical-critical and the pragmatic-accommodating” (Wynne 2007, p.494) line of STS scholarship, thus suggesting that the parting line no longer runs between those who aspire to be politically relevant and those who don’t. Yet Wynne also rejects the establishment of a dichotomy between the pragmatic and critical, as the “pragmatic should not be entirely separated from, even counter-posed to the radical” (ibid., p.500).

historiographical accounts of the origins of STS, implied the existence of two very different conceptions of the field (one closely linked to the science-critical movements of the 1960s and 1970s, and one oriented towards a purely academic discourse – in Steve Fuller’s (1993) distinction, a Low and a High Church of STS), the second section suggested that *SSS* was closely associated with the High Church, while *STHV* was rooted more strongly in the tradition of the Low Church. But already by the early 1990s, this distinction started to crumble, as the similarities of the debate in both journals described in the third section of this chapter show. And by the late 1990s, the authors in both journals seemed to agree on the political and social relevance of STS research and on the need to include normative reflections in STS scholarship. As Sergio Sismondo (forthcoming) has recently argued, the two Churches possess numerous bridges and are interlinked in multiple ways. Indeed, Gary Bowden’s verdict that the transformation of *STHV* into the official journal of *4S* illustrated a beginning convergence of two “communities, which had existed in relative scholarly autonomy” (Bowden 1995, p.71), one more concerned with the political and social implications of science and technology and the other more academic and theoretical, seems apt.

VII. Conclusions

Perhaps the most pressing issue to be commented upon in these concluding remarks to my thesis has to do with its most glaring omission: For all the talk about the boundary work within the journals *SSS* and *STHV*, for all the remarks about the discipline, object and agenda of STS, precious little has been said about the methods of STS, little attention been given to the method talk that took place in the two journals during the period under investigation. The fact that STS work is generally taken to rely strongly on its use of case studies of scientific or technological episodes, and that both journals pride themselves in being places for the publication of original empirical research, makes this omission even more significant. Yet it also cannot simply be blamed on negligence on the part of the researcher: While it was fairly clear early on in my analysis where to search for elucidations about the object, discipline and agenda of STS (namely, in the abstracts and opening paragraphs of the articles), no amount of familiarity with the journals would indicate a single good place to look for insights into the methods employed to get to the results of the empirically-oriented articles. Due to the only rare occurrence of methodological notes in the introductory statements or abstracts and the lack of a dedicated method section in most articles, the only way of gaining insights into the methodological set-up of the research would have been to carefully analyse each article in full, a procedure that would have gone well beyond the scope of this thesis.

Systematic insight into the methods of STS research is thus something that this thesis was not able to provide; nor did it do much to link the analysis of articles in the two journals to developments on the level of institutional structures, curricula or other journals in relevant fields. Fortunately, it does offer some findings on other issues to make up for these shortcomings. After sketching out the general development and character of the two journals under investigation (chapter III), the thesis moved on to analysing how a research object was constructed in the two journals (chapter IV). In this chapter, I showed the negotiations about what it means to set out to study ‘science’ – negotiations about whether the social sciences can and should be included under this banner, about whether emphasis should be put on the context of the production of scientific knowledge or on its application in society or policy, about whether to focus on the contents or the institutions of scientific work, etc. I have also traced the study of technology in the two journals, showing that in

STHV, both science and technology have been considered legitimate objects of analysis all along, but that the idea of treating both science and technology with the same conceptual tools and theoretical notions was only introduced later and controversially discussed until well into the 1990s. In *SSS*, there was much less discussion of technological themes before this move to treat science and technology as one unit of analysis – a move that was discussed less controversially here than in *STHV*. In both journals, a development from focusing on ‘large technologies’ towards also considering ‘smaller’ technologies, and doing so not only in their context of use and application, but also in the design and testing stages, took place during the time period under investigation. Finally, chapter IV also showed how gender issues, slowly and reluctantly, were taken up as objects of analysis in the two journals, sometimes brought to the table by self-described (semi-)outsiders of the STS field. While during the early years of the two journals, gender issues (if they were brought up at all) were usually framed in terms of ‘women in science’, the focus gradually changed to include considerations about constructions and depictions of masculinity and femininity in various scientific and technological contexts. It should be noted here that this development runs parallel to a trend within gender studies, or rather to a development from women’s studies to gender studies, in which a transformation of the object of analysis took place: the object of interest was no longer ‘women’, but ‘gender as a historically mutable, societal-cultural phenomenon’ (Metzlers Lexikon Gender Studies Geschlechterforschung 2002, p.143).

Chapter V, then, was dedicated to an analysis of the disciplinary boundary work that took place in *SSS* and *STHV* between 1980 and 2005. The chapter consists of two sections, with an interlude connecting the two: In the first section, the various traditional disciplinary approaches that have informed work in the two journals were discussed, working out the extent to which publications in the journals draw upon certain disciplinary traditions and what characteristics are ascribed to these approaches. Especially during the 1980s, many articles published in *SSS* distinguished themselves from philosophical (which are frequently accused of being too concerned with general, essentialist principles of science and of not being sufficiently empirically grounded) and historical (which are lauded for the richness of detail, but scathed for failing to look beyond the specifics of each case and for their lack of attention to theory) approaches to the study of science and instead proposed to

study scientific knowledge sociologically. Yet these approaches often also distanced themselves from traditional sociological perspectives, which they rejected due to their insistence on universalistic principles and their conception of studying science exclusively in terms of a study of scientific actors and institutions while failing to take into account the contents of scientific knowledge. In *STHV*, there was far less evidence of such manifestos for a new approach to a sociology of science – but then, this should come as no surprise, as *STHV* during the 1980s was generally less prone to participate in purely academic debates, as far more attention was given to the treatment of policy-relevant issues or topics of societal concern, and indeed, articles that did not meet the usual format of academic papers prevailed over purely academic writings; by the time that *STHV* morphed into a more traditional academic journal in the late 1980s, these ‘boundary wars’ between sociology, history and philosophy had largely been fought out already. Indeed, the custom of positioning approaches by distancing oneself from (supposedly) more mainstream, more established traditions, is one that seems stronger during the early phase of the period studied in this thesis and is later superseded by an increasing number of positive references to particular academic (usually STS) approaches and concepts. This pattern is certainly not singular for the journals under consideration, nor is it for the field of STS; for example, both psychology (Benetka 2002) and sociology (Karady 1981) have, during their process of institutionalisation and becoming a recognised and established academic discipline, differentiated themselves from philosophical thinking in a similar manner.

By contrast, anthropology was not at the core of the disciplines that authors frequently referred to and distinguished themselves from during the early years of the analysed material, and consequently, discussions about the status of anthropological approaches in the two journals did not catch on until later. Especially in *STHV*, a number of articles by anthropologists were published during the 1990s and early 2000s in which the authors complained about the lack of recognition their work has gained in STS circles, and pointed out neglected anthropological contributions to STS. On the other hand, psychological approaches have not been brought to bear upon STS research to the same extent; indeed, psychology is the only discipline discussed here that can be found as an object of analysis more so than as a disciplinary approach to the study of a particular object. This exceptional status of psychology is probably related to the fact that, more than any of the other disciplines under consideration, psychological approaches have frequently succeeded in

positioning themselves among the ‘hard’, natural sciences, which, as chapter IV had shown, are a more common object of STS analysis than the ‘soft’ social sciences or humanities.

In an interlude in chapter V, the relationship of STS and gender studies – a field that enjoys a roughly similar status of institutionalisation and establishment as STS – was reflected upon, showing some of the points of tension, but also signs of convergence of the two fields. The second section of chapter V then moved on to discussing conceptions of the field covered by the two journals that move beyond the boundaries of traditional disciplinary perspectives. Here, I have shown that, as time moved on, multidisciplinary conceptions such as ‘philosophy and sociology of science’ or ‘history and anthropology of science’ have gone out of fashion somewhat in both journals, while denominations that stress the interdisciplinary character of research (while de-emphasising the predominance of individual disciplines), such as ‘science and technology studies’ or ‘social studies of science’ (the latter, perhaps unsurprisingly, enjoying more popularity in the journal that shares its name) have come into vogue. Among these, terms that mention science and technology in the same breath (‘science and technology studies’, ‘science, technology and society’, ‘science and technology policy studies’) have largely emerged in combination with the development of a common object of analysis described in chapter IV. However, in *STHV*, these terms caught on a little earlier than in *SSS*, generally in instances where science and technology were discussed in contexts of application; especially the term ‘science, technology and society’ was used much more commonly in *STHV* than in *SSS*, a phenomenon that may well be related to a stronger interest in understanding science and technology in a societal context, rather than studying it as a socio-cognitive activity.

Finally, in chapter VI, I have described the debates about a political or social agenda for STS. Since STS is an academic field that, at least partially, emerged out of a tradition of social movements (although not all STS scholars agree on the extent of this historical affinity between STS and a critical science movement, as I have shown in the first section of the chapter), this question about the agenda of STS is perhaps more pressing than it is for most academic disciplines. However, gender studies face a very similar discussion about the ‘feminist turn of science’ and the ‘academic turn of feminism’ (Hark 2005), and thus about the relationship between academic projects and feminist movements.

As my analysis shows, during the 1980s, many articles published in *STHV* explicitly displayed a partisan character and showed concern for the political and social implications of science and technology, while debates tended to centre around possible science policy applications of the knowledge produced in the journal and its adjacent fields; in *SSS* during the same period, an ostensibly disinterested style of scholarship prevailed. During the 1990s, however, both journals dedicated considerable space to debates about the normative relevance of STS research, where much conceptual discussions of the political implications (more so than the political applications) of STS research took place. Topics of discussion included the political potential and implications of constructivist approaches to the study of science and technology, the feasibility and desirability of studying scientific controversies symmetrically and in a neutral fashion, and the possibility of advocating political and social agendas while rejecting notions of absolute truth. While these debates tended to commence in rather antithetic terms – where one side posed that constructivism and relativism blocked up potential for political action, while the other side argued that it opened them up because it allowed more people to participate in the construction of scientific knowledge and technologies; or where one side argued for a disinterested approach to the study of scientific controversies while the other side argued for partisan intervention – several subsequent comments undermined such a formation of opposed fronts, questioning these dichotomies and emphasising the need to accommodate different approaches and styles of research within an STS tradition. The late 1990s and early 2000s, then, marked a growing concern for participatory democracy and citizen involvement in scientific and technological decision-making, and with such, also a return to a focus on the political application of STS research, rather than the socio-political implications of the adoption of a particular style of research. With these new developments, there is still room for disagreement over the desired extent of democratic participation or over the strategies of implementation for policy-relevant insights from STS research, but the basic notion that STS has a social and political role to play no longer (or at least not currently) seems to be a point of contestation.

While it may be tempting at times to read this account, or at least parts of it, as a narrative of progress, displaying the continuing refinement of analytic categories and normative positions within STS, I would like to resist this temptation – and not just because, in some

aspects, the debate can actually be seen to run in circles (e.g. concerning the discussion about the policy-relevance of STS work, which had taken some space in *STHV* during the early 1980s, shifted into the background for a decade and a half, and celebrated a comeback during the last few years in both journals). Rather, I think this way of reading the thesis – ‘here is how STS has reached its current level of sophistication’ – is not just self-satisfied, but also not particularly interesting. Instead, I propose to read these developments not (necessarily) as conceptual progress, but rather as an indication that, within STS as much as within other academic disciplines or fields, very few things can be taken for granted or are clear from the outset; a scientific field does not just pop up out of nowhere one day, but emerges out of persistent debates about its character, is shaped by sustained and meticulous negotiations about its properties. Neither STS, nor any other academic field, just suddenly appears out of thin air; its boundaries have to be drawn and redrawn, its focus carved out. This is accomplished in many arenas – the design of university curricula is one of them, the writing of research proposals and reports another, etc. As this thesis has shown, scientific journals can also be understood as one of these arenas, and the writing, editing and publishing of journal articles can be understood as an instance of boundary work. Within *SSS* and *STHV*, countless negotiations about the character (and denomination) of the STS field have taken place, and the (sometimes blurry) silhouettes of the field can be perceived by surveying these two journals. By focusing on the object, discipline, and agenda of STS, I have traced three aspects of the boundaries of the field, providing a glance at the negotiations and debates that sometimes sharpen, sometimes blur them.

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