

## Paradigms, Cultures and Translations: Seven Ways of Studying the Discipline of International Relations

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**Draft paper, comments more than welcome!**

### 1. Situation Knowledge Production: Why a Sociology of IR?<sup>1</sup>

"The way the profession remains strangely quiet, almost silenced, [...], makes this a particularly relevant time to enquire into the links between theory and practice"  
(Steve Smith 2002: 233)

"The predominant, essential character of the university is generally considered to reside in its 'self-governance': this shall be preserved. But have we also fully considered what this claim to the right of self-governance demands of us? Self governance means: to set ourselves the task and to determine ourselves the ways and means of realizing that task in order to be what we ourselves ought to be. But do we know who we ourselves are [...]? Can we know that at all, without the most constant and most uncompromising and harshest self-examination?"  
(Martin Heidegger 1991 [1933]: 29)

Similar to other social science disciplines, International Relations (IR) is facing these days a growing range of critics accusing it for being a useless discipline. Whether these critics come from the inside or from the outside, they attack the very heart of the discipline. If an autonomous discipline of international relations is a useless project, why should it persist? The cynic might argue that the problems addressed by IR, such as international cooperation, war and peace are persistent to a degree that also in future all sorts of social knowledge that can be made available will be needed. This is however an argument that drives IR into arbitrariness and does not justify the resources the members of the project have been granted, or the existence of a master programme in IR (or even IR theory). The positivist scholar might argue for the superiority of the formalized knowledge that an academic discipline can provide. Given that it is a conventional wisdom also among politicians these days, that academics rarely speak in the name of truth and scholastic knowledge has offered little problem solutions, how to justify the existence of an

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<sup>1</sup> For comments on an earlier version of this paper I like to thank Peter Wagner and the participants of his seminar "Whither the European social science? The sociology of forms of social knowledge applied to Europe", European University Institute, Winter 2006. Further I like to thank Frank Gadinger, as much of what is presented here draws on our earlier discussions.

autonomous discipline of international relations instead? Heidegger's quote is suggestive in this context. Firstly, he reminds us that the contemporary situation, or crisis as we might call it, is not a novel situation. Secondly, he identifies a strategy of coping with the situation and defines a concrete task: Defending self-governance requires the “constant and most uncompromising and harshest self-examinations” by which scholars define their tasks and ways and means to fulfil them.<sup>2</sup>

If we take this perspective, how has the project of IR examined itself?<sup>3</sup> The majority of scholars have opted for the path of examining the project by means of epistemological debates. Can we consider these debates and examinations as the harsh and uncompromising kinds Heidegger calls for? I suggest this is not the case. A focus on ideal types of producing knowledge and how it represents reality, sidelines, undermines, and neglects decisive components of a disciplinary project. Science is first and foremost a social practice. Science is materially and socially situated; it requires material, financial and human resources; it is structured by socialization and disciplinarization; it requires knowing subjects, who are gendered, marginalized or authorized; it is negotiations about relevance, significance, instruments and methods; it requires a range of institutions and techniques, and it is also a political practice involving ethical considerations of all sorts.<sup>4</sup> These are some of the forces and dimensions that have been identified by the sociology of science. Hence if we attempt to follow Heidegger's path, what is required is a systematic alternative to the so far dominant epistemological reflections. This alternative consists in the sociology of science perspective.

And indeed, if something has flourished in recent years, then it is a growing interest in studying the project of IR in a sociological fashion. The past, presence and future of the discipline as one way of studying ‘the international’ and problems of world politics has been of growing interest. A nascent number of studies have re-told the early history of the discipline, provided different readings of its birth and evolution. Scholars have become increasingly concerned how the structure, mechanisms and practices of the discipline have shaped the way the international is thought. Although the majority of these studies focus on methodological or pedagogical

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<sup>2</sup> The other alternatives might be to either leave the examination of IR to these bureaucratic evaluation programmes that are currently already permeating the sciences globally, or to refrain from any justification and give up the idea of a scientific study of IR.

<sup>3</sup> My question is not referring to examination techniques such as peer review. While this is the standard technique used to judge (examine) about the quality of scientific work, with its own problems (see Hellmann and Müller (2004)), it refers to the examination of individual or smaller groups work. I refer to collective endeavours.

<sup>4</sup> Problems of scientific practice arise, for instance, when a librarian has not managed to organize literature needed in time, which was the case for me, and led to the exclusion of some major works from my discussion.

practices, in recent years, scholars have stressed the significance of other practices, institutions and structural and environmental factors. Taken together, these studies present a new wave of self-examinations: Self-examinations of how the discipline studies its issues, what it has achieved and how it limits and enables its members. Disciplinary sociology as a significant field of inquiry and as alternative to epistemological reflections has emerged.<sup>5</sup> It would however be an exaggeration to claim that disciplinary sociology has reached the core of the discipline.

As for the case of epistemological reflections<sup>6</sup> there is a certain danger inherent to these reflections, the danger of becoming an esoteric disciplinary island, where as David Newsom has stressed it in a different context “even the humour is for members only”. As Pierre Bourdieu puts this “Sociologists must avoid the temptation of indulging in the type of reflexivity that could be called *narcissistic*, not only because it is very often limited to a complacent looking-back by the researcher on his own experience, but also because it is its own end and leads to no practical effect” (Bourdieu 2004:89, emphasis in original). Disciplinary sociology would be rather useless, if it only focuses on the discipline in a closer sense and does not address the broader disciplinary contexts such as the relation of the discipline to politics and society.

Jürgen Habermas (1978:13) has argued that reflections on science should be at least composed of three elements: 1) empirical research on the organisation of scientific and technological progress; 2) reflexive analysis of the social situations (*gesellschaftlichen Zusammenhang*) in which science is institutionally and methodological embedded and which decides the same time about the usage of scientific created information's; 3) the preparation of the practical usage of knowledge (*Erkenntnis*) to be translated into technology and strategies as well as into communicative praxis. Hence according to Habermas studies of science should reflect on the constitutive conditions of knowledge production, on the organisation and practices of knowledge production and on institutions and practices by which the knowledge is used, disseminated and put into praxis. Bourdieu argues in the same spirit, when he claims (in a more wordy way):

"Practical reflexivity can take on its full force only if the analysis of the implications and presuppositions of the routine operations of scientific practice is taken further into a genuine critique (in Kant's sense) of the social conditions of possibility and the limits of the forms of thought that the scientist ignorant of those conditions unwittingly engages in his research and which, unknown to him, that is to say, in his place, perform the most specifically scientific operations, such as the construction of the object of science." (Bourdieu 2004: 90)

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<sup>5</sup> I outline my understanding of the terms ‘disciplinary sociology’ and ‘sociology of IR’, I use interchangeable, below.

<sup>6</sup> Peter Mayer (2003) provides a nice example, when he argues ironically that one of the reasons of why epistemological debates continue might lay in the fact that scholars engaged in it have invested so many resources that debates need to continue for a sufficient pay-off

These interrelated tasks are what a disciplinary sociology of IR needs to address. This does not mean necessarily to follow the path outlined by Habermas (in *Theorie und Praxis* and *Erkenntnis und Interesse*), or by Bourdieu (in *Homo Academicus* and *Science of Science and Reflexivity*).<sup>7</sup> Rather the field of science studies invites us to make use of their different, concepts, theories and results.

If these are the tasks, what needs to be done, in what way has the project of international relations tackled these? If sociology of science is the path to go how far did IR researchers walked on it so far? Or to phrase it with Heidegger's words, what is the quality of the self-examinations conducted by disciplinary sociology, are they as harsh and uncompromising Heidegger calls for?

#### *Overview of the paper*

In this paper I attempt to address these questions in examining the self-examinations of the disciplinary project of international relations. The objective of this paper is to read IR's disciplinary sociology in the context of the broader sociology of (social) science discussions. Such a strategy helps to identify what kind of analytical choices scholars have made, what their implicit, silent assumptions are and where the strength and weaknesses of the contributions are. The imperative behind such a discussion is to foster a needed dialogue between IR and sociology of science (Wæver 1998) in order to improve the current discussion and future research. Although disciplinary sociology in IR has meanwhile reached a quantitative level that is unique if compared to reflections on political science in general and to other fields studying political phenomena, many of these reflections have demonstrated an astonishing resistance to engage thoroughly with the sociology of science. The primary tasks of this paper are hence: First, to identify and systematize disciplinary self examinations; second, to suggest paths by which the examinations can become as harsh and uncompromising as needed.

[As side-product, and a secondary task, taking disciplinary reflections as an object of analysis might lead us to interpretations of what this thing called IR is, as the narratives produced by disciplinary sociology are not only descriptive but also prescriptive. They are accounts of what IR is, and what it is not, of what it should be and what it shouldn't, of where it comes from and where it should go. To use Heidegger's words, they are attempts to determine what "we ourselves ought to be". Such a discussion might thus contribute to understand what IR is, what its place in the world is and how it relates to other cultural spheres. On purpose I am using here the word

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<sup>7</sup> I would rather argue that both Habermas and Bourdieu have failed in their own standards. This is, however, a different discussion.

might, as my discussion builds up on a selective reading of the literature and is limited to studying texts, rather than practices.]

The following section two, firstly, provides an overview of science studies traditions as a background. Secondly, I shall conduct an inventory of IR's disciplinary sociology, identify seven ways of studying IR, thirdly, criticize the achievements of the seven ways in the light of the described challenges and conclude in sketching persistent *problematiques* in IR's disciplinary self-examinations. Section three is a sketch on how to proceed with disciplinary self-examinations from a *Cultural Studies of Science* perspective and section four a summary combined with a note on self-reflexivity.

## 2. Examining self-examinations: Sociology of Science and IR's disciplinary sociology

If we start from a wide understanding of the term 'self-examination' we might include a wide array of studies. To some degree any study summarizing the state-of-the-art of a disciplinary subfield or critically judging about existing literature is a self-examination. This would be however a too broad and rather un-operative, useless understanding for the purpose of this paper. A more narrow notion is used here. I consider as self-examinations, those studies that are a) written with the objective to conduct self-examinations, b) relate themselves in some way to the project of disciplinary sociology, signified by a degree of reflexivity on the issue and are c) build to some degree on empirical observations, hence are not limited to the discussion of metaphysical problems.<sup>8</sup> Further, I assume that contributions to disciplinary sociology set up a relation to the discussions in the sociology of science. Such a relation can be composed of either a quoting strategy or the direct usage of concept, models and results of the sociology of science.

### *Sociology of Science: Four traditions*

As a background for my following discussion, and given that most IR researchers are not familiar with sociology of science, let me explain what I understand by it. Sociology of science is conventionally split into four traditions.<sup>9</sup>

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<sup>8</sup> Hence prominent state-of-the-art articles and edited volumes, some would expect from an appraisal of self-examinations, are excluded from my discussion.

<sup>9</sup> For such a separation and general introductions see Bourdieu (2004: 4-31), Felt, Nowotny and Taschwer (1995), Lynch (1993:39-116), Rouse (1992), and Law (2004) on methods. Despite otherwise noted, these are the hinterland of the following discussion. For an exceptionally concise and short summary of myths about science and science studies findings see Traweek (1996).

1) A functionalist wing in the footsteps of Robert Merton, primarily interested in the institutions of science and the identification of social norms that should govern science. Mertonians work in the frame of a separation of labour in which the past and the ‘reconstruction of progress’ is the meal for the historians, a philosophy of science is responsible for scientific content and concepts, and what is left, or cannot be explained otherwise, falls in the realm of the sociologist.

2) With the publication of Thomas Kuhn’s seminal *Structure of Scientific Revolution* such a separation was successfully challenged and research developed historically after Kuhn builds up on the assumption that research on the sciences needs to integrate time, content and any social dimension of the sciences. I am hence speaking in the following of post-Kuhnian thought when research does reject an implicit or explicit frame of a separation of labour. In marking this difference, scholars prefer the term of ‘science studies’<sup>10</sup> over the term of ‘sociology of science’ to highlight that any studies of scientific practice is a *transdisciplinary* project (Weingart 2003) and not a ‘sociology of’. Accordingly I will use this label, science studies, in the following.<sup>11</sup>

Kuhn’s major contribution (and those following in his lines) is twofold. First he successfully challenges the idea that the development of science is a continuous process. In introducing the concepts of normal science and revolutionary shifts, he demonstrated the discontinuities and breaks that mark the history of science. Second, Kuhn re-introduced the idea of a scientific community<sup>12</sup>. He argued that scientists form a closed community whose research draws on a well-defined range of problems and who use methods adapted to this work. Instead of determined by the rules as set out by philosophers of science, or the norms suggested by Mertonians, scientific practices are attempts to solve concrete problems, regarded as ‘puzzles’. For Kuhn the problems stem from what he called ‘paradigm’ or ‘disciplinary matrix’: A set of scientific achievements, theories and methods that are taken for granted. The actions of scientists are hence determined by paradigms, and the community becomes indistinguishable from this paradigm. Kuhn was not only criticized for his under-specification of the concept of paradigm, but also for cutting of the scientific group from any external world. Although it is Kuhn’s merit to have brought back the idea of scientific community governed by a central norm (paradigm)

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<sup>10</sup> Other conventionally used terms are ‘social studies of science’ or ‘science and technology studies’. Both highlight distinct features, while the former wants to preserve the notion of explicit social explanations of science, the other stresses that science and technology cannot be separated. However, I came to prefer the term ‘science studies’ as the most neutral one, while ‘sociology of science’ is the most popular term.

<sup>11</sup> Consequently I also would prefer to speak about ‘IR studies’, instead of ‘disciplinary sociology’ or ‘sociology of IR’, however I am afraid this would cause even more confusion.

<sup>12</sup> I am using here “re-introduced”, given that Kuhn largely copied his concept of scientific communities, although rephrased under the label “paradigm”, from the work of Ludwik Fleck, that was largely forgotten. See Felt, Nowotny, Taschwer (1995: 127-128) and the historical reconstruction in Schäfer and Schnelle (1980).

and drawing attention to the discontinuities of scientific history, with this problems Kuhn fell back behind earlier more sophisticated thought.<sup>13</sup>

3) A third tradition, the ‘strong programme’, sets up on such a critique of Kuhn’s work. This tradition is associated with the work of scholars conventionally referred to as the *Edinburgh school* following the writings of David Bloor and Barry Barnes and the *Bath group* following Harry Collins. The seminal work in this context is Bloor’s *Knowledge and Social Imagery*, first published in 1976. To construct a conclusive theory of scientific knowledge, Bloor argued for four major principles that should be followed: 1) causality: any proposed explanation must be causal; 2) impartiality: the researcher must be impartial towards the ‘truth’ or ‘falsehood’ of any assertions made by the actors studied; 3) symmetry: the same means must be used to explain both beliefs considered to be true by the actors and those judged to be ‘false’; 4) self-reflexivity: in principle science studies must be open to the same treatment as those sciences studied. Given the notion of causality is interpreted even by Bloor himself widely as also including ‘understanding’, these principles form the core principles of any contemporary science studies.<sup>14</sup>

Besides outlining these core principles for science studies, the merits of Bloor, Barnes, Collins and followers lay in clearly showing empirically the underdetermination of any theory by data. As case studies exemplify, belief preferences, tactics of persuasion, opportunistic strategies and local conditions such as equipment and procedures define to a considerable degree the outcome of scientific practices. Further, controversies are rarely solved by evidence or by rational means. Controversies come to be settled by diverse strategies of boundary drawing and of persuasion or even by dishonest means. Hence in this tradition scientific achievements are better explained by social ‘factors’.

The strong programme has however, been criticized for failing to address the wider (external) environmental conditions shaping scientific actions. Much the same as Kuhn did, a given autonomy of science was assumed, the attention was centred on the immediate local environment of scientists and a wider context largely ignored. The main criticism is however that researchers have overstretched the “social” explanation in a way in which the object of analysis does not matter anymore.<sup>15</sup>

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<sup>13</sup> For instance the related work of the sociology of knowledge by Karl Mannheim or, as mentioned already, the work of Ludwik Fleck.

<sup>14</sup> Nonetheless there is considerable disagreement about the meaning of these principles, see the discussions in Pels (1996) and Latour (2005).

<sup>15</sup> Such an interpretation led to what has to become known as the *science wars*. See Latour (1999, chapter one) and Topper (2005) for provoking discussions of the *science wars*. While Bruno Latour offers a ‘truce’, Keith Topper shows how the *science wars* find their parallels in political science’ *Perestroika Movement*.

4) A fourth tradition initially took the perspective of the strong programme as starting point and used it to conduct ethnographic studies of scientists in action. Researchers moved closer to the sites where scientific knowledge was produced and followed scientists minutely. The key finding was that

“we cannot start from a particular division between systems like the one between the scientific community and the rest of the social context and then determine their interaction. As direct observation studies of scientific work indicate, the social context of scientific work is both more restricted and extended than the notion of a speciality community suggests. It is more restricted (more local) in that researchers draw upon a variety of variables that are situationally contingent. For example, their decisions may be influenced by measurement instruments which happen to stand around in the laboratory, or by arguments which come up in a technical discussion. The context of scientific work must be defined more broadly than previously suggested in that these variables and arguments are not, in principle, limited to (bounded by) the scientific community. Direct observation of scientific work suggests that laboratory operations are embedded within transscientific fields of interaction and discourse. Transscientific fields are not logical classes whose members share specific characteristics. They are constituted by what is transmitted between agents; they come through actual or potential (discursive) interaction and communication relevant to what happens in the laboratory. “ (Knorr Cetina 2005 [1983]: 191)

The local is translated to the global and the other way around in specific sites such as the laboratory.<sup>16</sup> To give an illustration what and how these researchers studied scientist actions, consider the box below, which is part of an excerpt of an interview John Law and Michael Williams (1982) conducted with two groups of scientists (studying the capacity of polymers to stimulate the uptake of substrate by cells):

**Watt:** What was interesting to you in our last discussion?  
**Williams:** What would be publishable, how that affects what you do.  
**Watt:** But I think it's the same question as what is valid and meaningful. I don't think it's a different question.  
**Morse:** I think they both occur when you reach a set of conclusions.  
**Gladstone:** They're a combination of results and . . . getting more money for the next research! (Interview, 25 May 1978)

*Box 1: Interview excerpt, reproduced, without permission, from Law and Williams (1982: 535)*

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<sup>16</sup> I shall discuss shortly if political science has laboratories or not below (7).

Both groups under study were attempting to write a publication together. What Law and Williams identify is how scientists attempt to produce a paper that has the maximum impact and is perceived by their colleagues as both reliable and relevant. Law and Williams conclude that scientists in many ways behave like entrepreneurs: they conduct market research, evaluate the relevance of their study to this group or that and then “are trying to array people, events, findings and facts in such a way that this array is interpretable by readers as true, useful, good work, and the rest.” (Law and Williams 1982:537). In sum these scientists arrange a network (array or transscientific field) of resources (events, findings and facts) and people (earlier researcher, the readers and themselves, the authors). That those networks are not necessarily composed of scientific resources and people, becomes clear already from the above interview excerpt, as the scientists have already in mind future funding. While funding is an obvious link, others have shown how wide the repertoire of resources and people decisive for scientific research can be. For instance, Bruno Latour gives over his writings vast evidence of how economic interests, concerns of politicians (such as national security concerns) and scientific interests merge and become assembled in one network. For instance, the work of Frederic Joliot trying to reduce the absorption speed of neutrons to make use of nuclear power, is a good case for such a network. A network of Zairian miners (providing the uraniumoxid), French import companies (importing the material), German scientific research, leading to the threat for the French war ministry that the Nazi Regime could use nuclear energy earlier, spies identifying this threat, and, of course, a research team at the *Collège de France*, etc. These are only parts of the networks Latour (1999) identifies. To understand what is going on in this network, protagonists of this wing of science studies, made use of the notion of translation as developed in the sociological writings of Michael Serres. According to Callon (1986:197), translation in this sense can be understood in the following way<sup>17</sup>:

“translation postulates the existence of a shared field of meanings, preoccupations, and interests. [...] If it concedes the existence of divergences and irreconcilable differences, it nevertheless affirms the underlying unity of distinct elements. To translate it to create convergences and homologies out of particularities”

What came to be known initially as *laboratory studies* or *laboratory constructivism* is today much more difficult to grasp by a unifying label. The protagonists of this ethnographic way of studying science, just discussed, have developed their thoughts into what has become known as *Actor-*

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<sup>17</sup> See further on the notion of translation Callon (1986), Callon and Latour (1981), Latour (1999, 2005),

*Network Theory*.<sup>18</sup> Others advocate for an ‘ethnomethodology’ of science (Lynch 1993). In the U.S. context the more integrative denomination of “cultural studies of science” (Rouse 1992) has been established for describing the ongoing work in this tradition. Rouses stresses with this signifier that the study of sciences should be understood as part of other attempts that focus on the emergence of meaning within human practices.<sup>19</sup>

### *Summary and back to IR*

To take Bloor’s principle of self-reflexivity serious, my short introduction of science studies should not be understood as a narrative of intellectual progress. In the footsteps of all four traditions sketched, contemporary science studies research is produced. I have conducted this discussion to make clear what the relations of IR’s disciplinary sociology to science studies in principle can be. I suggest that the four traditions are the major resources disciplinary sociology can draw on. This does not mean that these are the only resources available from science studies. Science studies has developed as such a lively field of research that it is nearly impossible for an informed dilettante to provide any comprehensive overview. Consequently if IR has picked up any other line, than discussed, we should be happy about it. Nonetheless the discussed traditions are, in some way, the minimum we would expect IR has made use of, if the idea of a disciplinary sociology is taken serious.

What becomes, however, already visible from my short science studies discussion is that if the tasks I identified with Habermas and Bourdieu – to reflect on the organisation of knowledge production, the environmental conditions shaping knowledge production and usage *and* the translation of knowledge into praxis as well as the interactions between politics and science – are to be followed, we need to pay close attention to the last tradition of a ‘cultural studies of science’. That there is a need to turn to the perspective outlined by this programme I will (hopefully) develop in section three.

So far I argued in following Heidegger that there is a need for self-examinations to cope with the current pressures put on IR. I argued that self-examinations which devote themselves exclusively to the discussion of epistemological problems are not sufficient in this regard. Instead, I stressed that self examinations are best conducted in the spirit of reflexivity induced by a sociology of science. Such a reflexivity should however avoid becoming narcissistic, through paying attention

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<sup>18</sup> See Latour (1999, 2005: 9-12) and Law (1999) for a discussion of these terms.

<sup>19</sup> Latour (2005) has made a similar move when he speaks of that the social interpretation of the sciences might have failed, but has led to a complete new understanding of what constitutes the social. Latour (e.g. in his *Parliament of Things*) and many other have consequently meanwhile moved far beyond “only” studying science.

to the organisation and conditions of knowledge production as well as the translation of knowledge into praxis. Finally, I outlined shortly on which major resources from science studies a disciplinary sociology of IR can draw upon. Now let us move to the question of how IR has conducted self-examinations by disciplinary sociology.

*Seven ways of studying the discipline*

In 1998 Ole Wæver has argued quite strongly that “the relationship between IR and sociology of science is virtually nonexistent” (Wæver 1998:692). In focussing primarily on histories of the discipline, he stressed that those “are usually not based on systematic research or clear methods. They are, at best, elegant restatements of 'common knowledge' of our past, implicitly assuming that any good practitioner can tell the history of the discipline” (Wæver 1998:691). Five (six?) years later, the situation did not look much different for him (Wæver 2003). Wæver’s pessimistic assessment needs to be however understood as an argument for strengthening the link between IR and Sociology of Science. While this warmly welcomed, I would suggest more caution in making such a claim. First, IR’s constructivists have drawn in many ways on science studies, (maybe most prominently on Berger and Luckmann’s *The Social Construction of Reality* and Ian Hacking’s *The Social Construction of What?*), this however has been done in an epistemological or ontological way<sup>20</sup>, rather than in an self-examination way, as understood here. Second, Thomas Kuhn’s *Structure of Scientific Revolution* has considerably shaped IR discussions. One is rather tempted to say that Wæver’s claim is right in the sense that the signifier ‘sociology of science’ and ‘science studies’ do not exist in IR – it seems that in IR everything that ever has been written on science, runs under ‘philosophy of science’. In many ways IR scholars heavily trained in epistemology debates, tend, whenever they are faced with the sociology of science, to see only philosophy of science, as this is what they are familiar with.

Nonetheless, Wæver’s claim can be used to ask in what way IR’s disciplinary sociology has actually made use of sociology of science. If we follow broadly the tasks set out by Habermas and Bourdieu, this means primarily how IR has used post-Kuhnian (science studies) thoughts, as these are the resources addressing these issues. I shall question the achievements of the disciplinary literature has made in settling the three tasks and in providing the necessary means of reflexivity in the Heideggerian sense. [Further if I follow my secondary target in gathering some knowledge about the self-image of the discipline by studying self-examinations, attention needs to be paid on how IR scholars construct their object of study.] In sum to reflect on the self-examinations of IR’s disciplinary sociology, I suggest to address the following questions:

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<sup>20</sup> The epistemic community approach, for instance, claims to make use of Kuhn and Fleck.

- 1) How has IR mobilized the resources of the sociology of sciences so far? Is Ole Wæver diagnosis adequate?
- 2) In what way has IR coped with the three tasks set out by Habermas and Bourdieu?
- 3) What were the reasons for doing sociology of IR? Autonomy preservation in a Heideggerian sense?
- 4) [What subjects of objectification have been studied? What are the ‘ourselves’ in the study? What is the underlying model of science?]

I shall start with an inventory of disciplinary sociologies.<sup>21</sup> Rather than discussing every single self-examination, I conduct a clustering. The categories are constituted by the networks of articles, scholars citing each other and similar problems being addressed by similar means. My list is not meant to suggest any hierarchical order or a historical narrative. The main purpose of the list is to demonstrate what a lively field disciplinary sociology is and to provide an initial assessment in the light of the above questions. Such an approach has, of course, its own shortcomings. Given that my perspective is wider than what is usually understood as disciplinary sociology, this comes at the cost of depth. My reading is selective; I will not be able to do justice to all the works I refer to; etc.

I suggest that disciplinary sociology can be usefully ordered in seven categories – seven ways of studying the discipline. However, categories overlap and the list is open ended.<sup>22</sup>

#### 1) *The textbook and aesthetic genres*

The most conventional way of examining the discipline is to be found in the format of textbooks. Textbooks attempt to provide overviews for newly arriving students and external (academic or non-academic) consumers. In their objective textbooks differ from other genres of literature.

In his classic 1935 study the *Genesis and Development of a Scientific Fact*, Ludwik Fleck differentiated between three types of scientific practice that manifest themselves in three different genres of scientific publications. Fleck argued that the three types differ over the *Erkenntnisziele* (epistemic goals) they follow and how they relate to non-community members – what Fleck called *exoteric communities* in contrast to *esoteric communities* of scholars.

Textbook science – Fleck spoke of *Populärwissenschaft* (popular science) – forms one type. Textbook sciences are constituted of these research practices which are the closest related to exoteric communities. Their key objective is *Anschaulichkeit* (a term difficult to translate: maybe imaginary clearness?), to develop images and symbols of a research object to make it

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<sup>21</sup> I use the plural on purpose here. As the perspectives I discuss are, although related, very different.

<sup>22</sup> Several works did not find their space here, but might in a future version of the paper. Most notably those are studies from feminism, and subfields of IR. (see also Fn. 4).

comprehensive for exoteric communities. Rather than being interested in specific evidence of detailed components, the goal is to provide knowledge of an object by providing imaginaries.

Fleck contrasted this type of research practice with two others, *Fachwissenschaft in Handbuchform* (specialized science in handbook format) and *Zeitschriftenwissenschaft* (journal science). The latter types focus on evidence and detail, but yet differ. The objective of handbook science is to provide critical summaries into an ordered system. Journal science is of a personal and provisional character. According to Fleck journal science follows individual standpoints and personal working methods in a way that an addition of articles do not make up a unified organic whole. Articles are incongruent and contradictory. However, research articles are always related to handbook science, as they usually relate themselves to a unified whole, in providing a state of the art section or in ‘making a contribution to the literature’. This kind of science is none the less always tied to an individual and his work, which becomes obvious in the usage of terms such as “I am” and a defensive style of reasoning such as “I attempt to show”, etc. In contrast handbook science is de-personalized, detached from personal opinions and works. Therefore terms such as “it is”, etc. are used.<sup>23</sup>

Fleck’s highlights the different objectives of research practices and that they are related to different audiences. His distinction is useful as it, first, stresses that textbooks are a very different genre than other forms of science and usually not address the esoteric community. Hence we shall not expect a high degree of reflexivity or a sophisticated usage of science studies from IR textbooks. Nonetheless, also textbooks are in need of an ordering device, which in principle can be drawn from science studies resource. Second, some of the publications in my inventory come in the format of handbook science (Schmidt 2002?), the majority is, however, journal science.

a) IR textbooks usually start either with a certain definition of the object of analysis of IR (what is international relations?), with an introductory discussion of what theory is or can be, or with a short historical narrative of what has been, in the view of the authors, the important steps in the development of the discipline.<sup>24</sup> These introductions also provide the ordering device of the books. Either, the device is the history of world politics, and IR developments are described as a (causal) reaction to world political events. Or, some interpretation of Kuhn’s ‘paradigm’ is used as a more sophisticated version of speaking about theory. Then, or, if the ordering device is more

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<sup>23</sup> In *Science in action* Latour (1987) makes a quite similar argument, he speaks of the construction of facts and *blackboxing*. For an argument of the importance of handbooks and state-of-the-art sections to understand scientific practice, see my discussion of Rouse in section three. Rouse stresses that these are means by which the narrative structure constitutive of science is reconfigured.

<sup>24</sup> See the discussion in Wæver (2004) and in Enterline (2004). Notable exceptions I ran across are: Cynthia Weber’s recent introduction that uses the concept of ‘myths’. Greg Fry and Jacinta O’Hagans’s edited volume (2000) which uses the notion of “images of world politics”. Ole Wæver and Iver Neumann’s volume on IR’s *Masters in the Making*.

explicit historically, we get the well known stories of great debates and phases of hegemony (Smith 1995). In contrast to single authored, edited textbooks usually start with a short introductory essay in which a thesis about the development of the discipline is presented and the following contributions are vaguely sorted into that. Peter Wagner (2001:3) gives us a well reasoned critique of such approaches when he argues that these

"adopt a perspective, in more or less concealed guise, in which all prior debates and disputes gradually and possibly unevenly, but equally unfailingly, lead to the state of conceptual and empirical accomplishment that has been reached in the present. The authors of such accounts are often active practitioners of the social science rather than historians of ideas or sociologists of knowledge and the sciences. As such, they find it – understandably – difficult to imagine a higher state of knowledge being attained at times other than their own, be it in the past or in the future.(3)

As we will see in the following, this is however not only a problem that arises in textbooks. Rather it is a common device to construct a state-of-the art as a trajectory of events, paradigm dominance and great debates.

b) Disciplinary self examinations also come in a format not considered by Fleck. Thus we might add a fourth genre of more sublime character, a genre that can be called a ‘commemorating’ or ‘aesthetic’ account. Such a type includes speeches given at anniversaries celebrations of an academic association or, plenary speeches by newly elected presidents of associations, for instance the ISA presidential speech. This genre is textbook science oriented, as highly imaginary tools are used, but it differs as primarily the esoteric community is addressed. Similar to handbook science it attempts to order and organize scientific developments, but yet it is most often highly personal in character. In difference to article science it is less oriented at evidence and engages in more aesthetic or artful reasoning. Beside speeches, examples of this kind are to be found in the *Pieces of our Craft* section of *International Studies Perspectives*. Although these accounts are telling, especially if made into an object of analysis in its own, we will not expect that they engage with science studies, although some of them might implicitly.

## 2) *Progress Assessments and ‘Paradigm Battles’*

Thomas Kuhn’s work has not only heavily influenced the practice of writing textbooks, but led to a real cottage industry of attempts to describe and to judge about the intellectual progress of IR.<sup>25</sup>

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<sup>25</sup> See the related contribution of Elman and Elman, Keohane, Vasquez, Hellmann (ed.), which I do not (yet) explore in full detail here. See also Guzzini (1998: 1-12).

While some scholars attempt to apply Kuhn's approach to IR, contemporaries have been more interested in the Kuhn-Lakatos exchange and discovered the toolbox of Lakatos' *Methodology of Scientific Research Programmes*. If we are tempted to search for a reason, why IR has been more interested in philosophy of science rather than science studies, we find one of the crossroads here: In contrast to the Kuhn-Lakatos exchange, the Kuhn-Bloor-exchange, so important for the development of science studies, was never introduced to IR.

Kuhn's role in IR starts to get problematic with the inflationary use of the term paradigm in which it has gradually lost its Kuhnian meaning (e.g. Legro and Moravcik 1999, Smith 1995 ) and the discovery of Kuhn's concept of incommensurability to mark boundaries between different IR 'theories'<sup>26</sup>. Rather than understanding IR as a scientific community governed by the paradigm of studying global politics and trans- or interstate relations as "international relations" – and not as macrosociology or international law– the term paradigm was now understood as referring to theories. While it was difficult to argue for revolutionary shifts in IR given the continuing multiplicity of research approaches, when 'realism' lost the cold war, the 'demise' of 'realism', provided the opportunity to do so. Nonetheless realists are fortunately still with us. Although it is doubtful if the Kuhnian concept of scientific communities ever reached IR, the move to Lakatos triggered by Kuhn, even worsened the situation. Lakatos' *Methodology* was initially introduced as a more reflective, methodological way to appraise if the "work is getting any better" (Elman and Elmann 2002:1) As Frank Gadinger (2002) has shown in his examination of applications of Lakatos in IR, *Methodology* transformed into a "discursive weapon". Instead of gathering knowledge of the social processes and practice constitutive of IR and 'causing' its development, studies were primarily interested in claiming this or that 'research programme' 'degenerative' or 'progressive'. Whether the critics of this "paradigmism" (Hellmann) might have meanwhile triumphed or not, several protagonists in this debate have developed their thoughts further and provided key contributions in other ways of studying IR.<sup>27</sup>

### 3) *Emancipation of Non-American IR: Leaving Stanley Hoffmann's shadow.*

A third way of studying IR is centred on the quest if the discipline is structured by a 'hegemony' of U.S. IR. Scholars either criticize the discipline for being too American, or demonstrate, with obvious emancipative intentions, that IR is practiced in Non-American sites very differently. The standard reference is Stanley Hoffmann's (1977) initial diagnosis of an 'Americanness' of IR. Hoffmann's contribution is today seen as innovatively (e.g. Jørgensen 2000, Smith 2002, Wæver

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<sup>26</sup> When I here and in the following refer to the term 'theory' I mean a system of statements (a narrative) that is intelligible and considered to be significant, see my following discussion in section three.

<sup>27</sup> This is quite obvious if we trace the writings of Steve Smith and Ole Wæver: Paradigmatism+Hoffmann=recent disciplinary sociology?

1998) as he related the emergence of IR to specific interests of the U.S. policy elite (leaders looking for some intellectual compass) and identified three institutional ‘factors’, of why the IR developed in the U.S. and not in Europe: 1) the link between the scholarly community and government, which meant that academics and policy-makers moved back and forth between universities and think-tanks, and government; 2) the existence of wealthy foundations which linked the "kitchens of power" with the "academic salons", and thus could create a "seamless pluralism to link policy concerns of government to the academic research community"; 3) the fact that the universities were flexible and operated in a mass education market which allowed them to innovate and specialize in their research activities, they were able to respond to the demands of government in a way that was impossible in the European University sector of the time. Such an understanding is seen as innovative as Hoffmann acknowledged the interrelation between politics and IR in an elite network, funding as an important device and the structure of an educational system. Hoffman’s thesis was largely rediscovered in the late 1980s and 1990s (Smith 1987, Krippendorff 1987)<sup>28</sup>. From a contemporary perspective this led to three ways of responding to Hoffmann’s argument.

a) With only minor changes in the argument, Steve Smith has discussed a diagnosed ethnocentrism of IR over several articles (1987, 2000, 2002, 2004, etc.). For Smith IR is still tied to the interests of American foreign policy elites, much the same way as Hoffmann described it. This is considered to be problematic as it favours a distinct view of what international relations and IR is, and what and how it should be studied. In his most recent contribution (2004), he investigates how IR treats questions of violence and concludes “that the discipline’s definition of violence looks very closely linked to the concerns of the white, rich, male world of the power elite” (Smith 2004:510). Smith claims to make use of a sort of genealogy à la early Foucault. His method is describing the development of IR theorizing and then linking it to other discourses. A range of other authors have meanwhile used different data and methods. Nosal (2001), for instance, has focussed on 14 U.S. textbooks to identify what visions of the world shape IR. He concludes that these texts “portray the world to their readers from a uniquely American point of view” (Nosal 2001:l.p.). Others also focussed on IR publications but did so by statistical means (Wæver 1998, Friedrichs 2004, and earlier Holsti 1985). These statistical analyses claimed that there is indeed an American hegemony, but that there is an evolving European counter-hegemony (a drifting apart). Statistical analyses however face a not easy solved methodological problem of nationalism, implied through the memberships of scholars and institutions in multiple

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<sup>28</sup> See Smith 1987, fn.2 for related studies re-discovering Hoffmann.

communities.<sup>29</sup> For instance, is a scholar who has made his career in the U.S academia, but is German in citizenship and mother-tongue, a European, a German or an U.S. scholar? Is a scholar with U.S. citizenship, based at a Scandinavian University, publishing mainly in European outlets an American scholar?

b) Others have responded to the Hoffman quest by mobilizing the concept of schools (see Wæver's (2004) discussion of this concept). Most visible advocates of an 'English school' have stressed that such a perspective must be returned to the discipline (e.g. Buzan 2001, Dunne 1995, 1998). In contrast to the above discussed texts, these authors were less interested in interpreting the discipline but to do something about the American hegemony, what Buzan (2001) called interestingly "self referential reflection". While Smith, Dunne and others attempt to secure that the British IR tradition is preserved and the US academics will continue to listen to British voices, a range of case studies has sought to show that there is even other than Anglo-Saxon IR scholarship.

*c) National Community Studies*

Numerous studies have meanwhile demonstrated at which sites IR has grown academically and how.<sup>30</sup> The primary perspective has been on 'national communities', usually referred to as those academics based in a nation state or by the language they publish. The findings were that there is an astonishing variety of national IR communities, who differ over how they dependent they are or relate to the U.S., that many interesting achievements have been reached, which stay often inaccessible for wider audiences as they are not published in English language. These studies face in many ways the methodological nationalism problem, discussed above. Many of these national case studies cumulated into more eclectic descriptions of the data the scholar could find or in narratives in which the scholars describe their own experiences in a national system. To encounter such a tendency scholars mobilize sociology of science to study IR more systematically. If we feel comfortable with a national perspective, these have been Danish contributions in the first place (the Danish way of doing disciplinary sociology?). Let me discuss two contributions which have reconfigured the study of IR in a more systematic way.

1) Wæver's 1998 article published in the anniversary issue of *International Organizations*, was maybe the first to speak explicitly of a 'sociology of IR'. Wæver conducted numerous works in this article: He conducted an analysis of publishing pattern, which let him to the conclusion that

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<sup>29</sup> Problems of this kind are discussed in Holden (2004), Wæver (2003).

<sup>30</sup> Too numerous to be cited for now, see Jørgensen (2000:12), Wæver (2003:2) and Holden (2006: 232) for collections. Studies meanwhile come from all over the world, Asia, Latin America, Europe, Africa?.

European and US IR are drifting apart. He developed Hoffmann's institutional factors further in mobilizing the 'discourse coalition model' developed by Peter Wagner and Björn Wittrock to conduct more systematic national case studies and sketched how this model makes sense of the development of France, Germany, UK and US. Finally he introduced the structural model of Richard Whitley, he is now continuing to develop<sup>31</sup>. Surprisingly the article was more recited for its drifting apart thesis and in introducing a national perspective as pivotal. Hence Wæver's article should be seen as the key reconfiguration move to national community studies. Nonetheless the discourse coalition model as a systematic way of relating political, bureaucratic, cultural and intellectual developments was not picked up in these.

2) While Wæver explicitly used work from the sociology of science, Jørgensen (2000, 2003, Jørgensen and Knutsen 2006) has developed a more eclectic "cultural-institutional" approach – not as an alternative to Wæver's interpretation of Wagner and Wittrock, as one would expect, but in largely ignoring it. Jørgensen attempts to analyse IR by "connecting" three explanatory variables to the developments of IR. These three variables are political culture, the organizational culture of both science bureaucracies and university systems, and the "habits, attitudes and professional discourse" (Jørgensen 2002) within the social sciences and humanities. With some reference to science studies in the latter variable (Bourdieu, Wagner, Gunnarson), he nonetheless does not argue why such an understanding is superior to others (either Wæver's discourse coalitions or maybe even Hoffmann's three factors). Rather he claims that the three variables are self-explanatory by pointing to the 'facts'.

[...]

Despite the work of Joergensen (2000) and Wæver (1998) it is surprising how weak the relations to science studies are in this network of studies. For instance Friedrich's book (2004) survives without reference to *any* sociology of science (or bibliometric) literature.<sup>32</sup> The very recent edited volume on International Relations in Europe by Jørgensen and Knudsen (2006), which shows an explicit concern for scientific institutions<sup>33</sup>, manages to go along with some references to IR's disciplinary history (largely Schmidt 1998, 2002) and a discussion of Ole Wæver use of Richard Whitley works and Bourdieu's sociology of science, in one of the chapters (Holden 2006).

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<sup>31</sup> See my discussion of way 6.

<sup>32</sup> See also the related critiques of Friedrichs book by Holden (2005) and Stritzel.

<sup>33</sup> For instance Lucarelli and Menotti (2006: 48) attempt to identify "Pattern of interaction among domestic scholars and between them and the external community, and domestic factors that influenced the current shape of the country's IR Production"

*4) The Disciplines Historiographers*

Although contemporary studies in the emancipative network (way 3) gives some reference to IR historiographies, Gerald Holden (2006:226) is right in stating that “historiography and investigations of non-Anglophone communities have up until now been pursued largely in isolation from each other”. If this is true, it is an interesting observation as both (way 3 and 4) originate from the 1970s and 1980s greyzone of textbooks, histories of the discipline and other state-of-the-art products (a good examples is Holsti 1985). To isolate historiography as distinct network of studies required much the same reconfiguration work Wæver (1998) and others have done for the case of the emancipative studies. I would suggest that such a re-shuffling was initially undertaken by Schmidt (1994) and stabilized through the major books by Guzzini (1998), and Schmidt (1998), which led finally to the declaration of a historiographical turn (Bell 2001) – a timely practice.

Historiographies prime interest can be seen in deconstructing the identity of the discipline by reading its politics of history (most explicitly Thies 2002). The tools have been that of intellectual history (John Gunnell, Quentin Skinner), rather than of sociology of science. Although Guzzini explicitly adopts a Kuhnian framework (and might be better put in way 2), his reconstruction of realism is maybe most widely read among historiographers.

The main case of historiography has been the birth of the discipline and the first great debate<sup>34</sup> as the locus where the narrative (or myth) of International Relations begins. The issue was less if there was anything such as a first debate, but how the narrative of two camps of scholars led to the exclusion of interesting scholarship and still defines what the discipline today is, and what it is not. A comparable case arises for where the birthplace of IR is located, whether it has been developed out of classical political theory, and hence is a subfield of political theory, whether its birthplace is the founding of the first chair in international studies in Scotland, and hence is an academic enterprise shaped by the idea of European universities, whether it was established as part of the post world war one negotiations and institutionalized as peace research think tanks, and hence is a an policy-oriented endeavour, or whether it is located in the founding of the American Political Science Association and hence a professional science in the U.S. sense – locating the birth of the discipline is political in so far as it implies a distinct vision of what IR is and what it is not.

*From presentism to historiographies internal/external distinction*

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<sup>34</sup> Besides the already mentioned Ashworth (2002), Quirk and Vigneswaran (2005) and the more fact oriented Wilson (1998).

The major re-configuring move conducted by Schmidt (1994, 1998) was first in accusing other history writers of using history primarily as a device for legitimizing or delegitimizing a particular identity. This is what Schmidt called *presentism*. Second, he mobilizes Gunnell's approach to conduct a more sophisticated historical case study. This seems to be the more important move as it opened the debate of how history can be studied reflexive<sup>35</sup> – and indeed the move which leads me to speak of historiography as a way of studying IR.

In contrast to earlier studies that claim to have solved the problem by posing “W questions: what to study, where to study, study by whom?, why study? How to study, what not to study, and what was left out?” (Holsti 1998: 18). While all these questions are important, how shall they be answered? For Schmidt, Gunnell's ‘internal’ approach provided a convincing answer to guide such a research. What is problematic about Schmidt's move is that he develops the enemy image of a (presentist) ‘external’ account and seems not to use, according to Holden (2002), his approach empirically appropriate. Despite the existing differences between Gunnell and Skinner, in his recent reformulation Schmidt (2006:257-268) in which the immediate academic environment is the starting point of research and the job is “to reconstruct as accurately as possible the history of the conversation that has been constitutive of academic IR” (257), Schmidt blurs his earlier boundary between an external and an internal in a way that it becomes indistinguishable. [...]

##### *5) Theory and policy discussions*

A quite different network of studies conducting self-examinations has arisen around the question of how the achievements of IR (findings, statements, theories, ‘laws’) relate to processes of (foreign) policy making.<sup>36</sup> The query has been conventionally grasped as examining if and how foreign policy elites ‘use’ IR products. Albeit the concept of ‘usage’ has proven to be difficult<sup>37</sup>, the overall finding was that elites rarely do. The reasons were largely seen in miscommunication, different language games, logics, systems or weak institutional links. Based on these results several prescriptions have been developed of how academics can find open ears and by which

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<sup>35</sup> For a criticism on Schmidt's account see Holden (2002) and for a review of further critics Holden (2006:227).

<sup>36</sup> Cp. for the following the existing overviews of the literature in Walt (2005), Eriksson and Sundelius (2005), Lepgold and Nincic (2001) and our own discussion in Büger and Gadinger (2007a). I further refer to literature that needs to be added or has not been addressed adequate in my present context.

<sup>37</sup> See our critique of the concept of ‘usage’ or ‘utilization’ in Büger and Villumsen (2006), and the related sociological literature (e.g. Beck and Bonß (1989), etc.). Utilization is a key concern in many studies from development studies, working in the bureaucratic-academic nexus.

practices results are best communicated.<sup>38</sup> Critics have opposed the view that IR should be produced for elites and called for delivering to civil society and NGO's instead.<sup>39</sup> While initial approaches have been experience-based, sociology of science perspectives and findings have been integrated, empirical observations been conducted and conceptualizations moved to the recognition of interactive pattern.

a) Recent contributions have been shaped to a considerable degree by science studies' interpretations of an upcoming knowledge society.<sup>40</sup> In drawing on his earlier pragmatist works, for instance, Gunther Hellmann (2007b) uses Peter Weingarts (2001) interpretation of the consequences of the knowledge society to examine the state of the discipline. Weingart's system theory based thesis is that a knowledge society implies several reconfiguration processes between science and society. Politics is *scientified*, the sciences become politicized to a degree that they cannot escape political influences, *medialized* and *industrialized*. Hellmann takes up this thesis and argues by relying on the case of German IR that contrary to earlier diagnoses of a drifting apart, IR, politics and the media move closer together. Such a tendency is visible, although (or because, which is what Hellmann suggests) German IR is more theory oriented and more professionalized. Hence for Hellmann the social importance of IR's achievements is largely determined by the environment that orders science, politics and the media, and structural change occurring in it, rather than dependent on the actions of individual scientists – what seems to be the prevalent view in the majority of prescriptions.

b) Given that the interactivity and interconnectivity between science and society is a key issue in sociology of science, others have relied on these thoughts to study more local cases of the relation between IR and policy processes. Although the borders of IR now become a critical issue<sup>41</sup>, case studies I found (so far) that at least take inspiration in sociology of science are<sup>42</sup>:

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<sup>38</sup> See for instance those practices identified by George (1993). Others argue for a stronger orientation at an objectivist tradition (Nicholson 2000) or for revising the idea of IR as a planning device (Jentlesson and Bennett (2003), Zelikow (1994).

<sup>39</sup> See for instance Booth (1997) and Smith (2007), the critique of Cox's understanding in Duvall and Varadarajan (2003). Contrary to the 'problemsolvers' and 'technocrats' they oppose, IR's Critical theorists have hesitated to show how such an engagement might look like. See for instance the discussion in (Bühler 2002).

<sup>40</sup> This is an issue in Eriksson and Sundelius (2005), Lepgold and Nincic (2001), in the contributions in Hellmann (2007a), and related in Hellmann and Müller (2004).

<sup>41</sup> Many of these studies are either transdisciplinary (more historical or sociological) or stem from IR's subfields, such as Critical Development Studies, Peace Research, Strategic Studies, or New European Security Studies. Although the focus is not immediately on self-examinations and the focus much wider than on IR, I would include here also much of the work of Didier Bigo and Jef Huysmans on security professionals, some of the epistemic community studies, for instance Risse-Kappen's (1994) study on peace research and the end of the cold war or Emmanuel Adler's (see 2005) study on the Non-

- Inderjeet Parmar's work (e.g. 2002, 2004) on the role of foundations and think tanks in the foreign and development policies of Britain and the U.S;
- Ron Robin's study (2001) of the relation between the behavioralist revolution in IR, think tanks such as the Rand cooperation, and the security politics of the early cold war;
- Philip Lawrence (1996) analysis of the vocabulary of deterrence and the (security studies) scholars providing it;
- Adam Edwards and Pete Gill's (2002) discussion of the discourse of organized crime and the mutually constitution of scholarship and political interests;
- Martin Mallin and Robert Latham's (2001) analysis of the practices of "the interplay of research, practical innovation, and advocacy" in the case of security scholars;
- Miroslav Nincic and Joseph Leggolds (2001) analysis of the cases of *Democratic Peace* research and IR Institutionalism in US Foreign Policy;
- Nicolas Guilholt's (2005:166-187) study of IR scholars, legitimizing the politics of and constituting the "field of human rights and democracy promotion".

In contrast to the other studies discussed (way 2, 3, 4, 5, and early 'theory and policy discussions') concepts of the 'public' and/or 'society' are of pivotal importance in these works. Whether these are IR-media relationships (Hellmann, Mallin and Latham), think tanks role in organizing public consensus (Parmar), or IR's role in providing the vocabulary to justify policies before a wider audience (Lawrence, Edwards and Gill, Guilholt). This does not only prove to some degree that it is sometimes useful to have a look at sociology of science, but is a needed addendum to the discussion, given that other studies either ignore it (way 2) or reduce the 'public' to some miraculous concept of political culture, which is for instance the case of Joergensen (way 3).

Whether the knowledge society perspective is taken or more local cases are studied, this network of study contributes to disciplinary sociology in highlighting the dense interactivity of IR, policy processes and society. Hence what this network addresses are the relation between IR and its environment and, in so far as prescriptions are produced, thoughts on how theory translates into praxis are provided.

#### 6) *The structural perspective*

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Proliferation movement. Other works interested in the role of (social science) expertise in global politics might well be included. See my related discussion in Büger (2007).

<sup>42</sup> Some of the studies someone might expect here, I discuss in the frame of the next category.

A sixth network of self-examination studies I would suggest to consider as a “structural perspective”. Although there are close parallels to the early usage of Kuhn, the broad assessments whether the discipline is American, historiographies addressing the development of the discipline at large, general assessment’s of the disciplines relevance, and the knowledge society narrative, the studies I discuss in the following conceive IR as a (global) order – hence the term ‘structural’<sup>43</sup> –, are less interested in the Americanness quest and emphasize the functionality of IR. In contrast to emancipative accounts (way 3) who attempt to enable a cross-community dialogue by establishing borders between them first (in identifying national communities and describing them as somehow disentangled from the rest of IR), structural perspectives consider IR as a transnational ‘discipline’, ‘field’, ‘discursive structure’ or ‘global network’. In contrast to historiographies (way 4), who apply in many ways also a structural perspective, the concern has been much more on the present state of IR research. In sidelining many pieces that could be relevant in this context, let us consider at least the following two approaches

a) Post-structuralist’s appraisals have come up with an understanding of IR as a discursive structure. Scholars such as Roxanne Lynn Doty, R.B.J. Walker, Bradley S. Klein, James Der Derian and partly Steve Smith stress that IR is not so much a ‘science’ of something, but a representation of political discourse. Hence IR should be analysed as done with other political discourses, but IR discourse is seen as an especially significant discourse as it attempts to objectify and rationalize other discourses of global politics. This understanding is the counter-argument to those assuming autonomy of science (and IR). IR is a discursive structure embedded in and representative of other discursive structures. Scholars have hence attempted to identify the political in the discourses of IR. Findings have been that the representational discourses of IR draw boundaries, which excludes regions, people and issues from political discourses or drives them to the margins. Representational practices of IR stabilizes the identity of societies as ‘Western’, ‘modern’, ‘liberal’, ‘secular’ and ‘democratic’, etc.

Such a perspective is telling as it, firstly, turns the starting point of a sociology of IR upside-down in not starting with the organizational aspects of knowledge production, but with the translations between IR’s and political practice. The assumption that there is (or cannot be) an autonomy of IR, however, comes at the price of ignoring the organizational aspects and many of the power struggles (post-structuralists are otherwise so interested in) between IR scholars. Such elements need to be developed if post-structuralists want more then to provoke and instead contribute to disciplinary sociology. Nonetheless, these post-structuralists make a decisive contribution in returning the political to a sociology of IR and in opposing a view of IR (and the analysis of it) in

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<sup>43</sup> Wæver (2003) which I discuss below explicitly talks of a “structural” perspective.

which an autonomous IR sphere interacts with an autonomous political or social sphere. Scientific practice, IR practice is political.

b) Ole Wæver's (2003) neo-functionalist account – in many ways the most sophisticated form of disciplinary sociology in directly drawing on science studies – is less interested in the political of IR, and leads us into another direction. In his draft paper that has meanwhile been widely discussed (e.g. Holden 2006, Hellmann 2007a), Wæver responds to the national versus global community debate (3) and answers the American hegemony quest by giving us a world-system style answer in claiming IR to be “a global network centred on US journals, debates and job markets” (Wæver 2003:4); although IR is trans-national, it is a “trans-national empire plus distinct national nodes” (Wæver 2003:4): the US is the centre and the further we move away from it, the more we move into the periphery. Hence he rejects that both quests (the hegemony and national/global) are relevant ones.

In shortly discussing Bourdieu's notion of (French) academia as field of power struggles, Wæver stresses that the study of power effects need to become part of disciplinary sociology<sup>44</sup>, to consider power he promises us for the future (has he kept his promise?). Primarily Wæver discusses what we can learn from Richard Whitley's (1987) neo-Mertonian *The Intellectual and Social Organization of Science*.

Without going into all the details, Whitley's approach is centred on the idea that science is a type of ‘work’ distinctly organised. Two principles are pivotal in this organisation, which is the principle of dependency – scientists need to rely on the works of others to be able to conduct their work or to gain status– and the principle of novelty or uncertainty – new findings need to be presented and results cannot be a simple replication of earlier work. Whitley goes further in arguing that both principles have a “functionalist”, technological and a “strategic” dimension. The former refers to the degree in which scientists have to rely on equipment and earlier works, while the strategic dimension covers the way how scientists need to cope with the two principles to build a reputation. Whitley ends up with a two-by-two table from which he develops different types (different degrees to which principles and dimensions matter). The typology is used to compare different sciences. This is movies considered necessary, as Wæver stresses with Whitley, because many thoughts on the sciences have been too focussed on general statement about science. Wæver uses the typology to identify which type IR might be. While he finds the functional dimension in general to be low and the strategic to be high, he concludes that IR's

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<sup>44</sup> Wæver is aware that for Bourdieu the identification of a field is the result of research, not the starting point, and requires heavy empirical observations on the existence of a common *doxa* and *illusio*. Hence he uses the (for him less theory-laded) notions of a global network or empire, to make his claim of IR as a multi-level enterprise.

authorization practices (better: reputation gaining mechanisms) are heavily directed towards theory. Although Wæver's pragmatic game with Whitley is telling, it leaves the readers with nagging doubts if a disciplinary sociology should be based on a technological functionalist vocabulary in which scientists seem to do no more than passive reputation maximizing and task fulfilling, while the rest is determined by the structure of the "intellectual field". Whether this is Wæver's or Whitley's problem, the imaginary offered here seems to sideline creative agency or any transformative (political) capacity of a sociology of IR.<sup>45</sup>

To raise some more criticism: Wæver is well aware that IR is inherently political and structured by power effects. Although he points into a direction (Bourdieu)<sup>46</sup>, so far he has however not shown how the study of power can be integrated into his Whitley framework and into disciplinary sociology of IR at large. Second, Wæver claims to follow a strategy that starts from an internal understanding and adds (if necessary) external elements. Hence he follows the opposite directions the post-structuralists have taken. The problem is nonetheless the same. In starting from the organization of knowledge production in IR, he either forgets (or did not had the time so far) to add, or does consider other factors, than those identified by Whitley, to be marginal.

In sum, structural accounts are key contributions for the sociology of IR, trying to implicitly and explicitly integrate science studies major findings and approaches. Both perspectives discussed here, nonetheless, suffer from some weaknesses and if a future 'structural' perspective is to be developed it may well be some connection (or muddling together) of those two.

### *7) Scientific Practice and Professionalization Discourse*

Optimistically I suggest adding a seventh category, the study of scientific practice. I say optimistically because such an approach has not been fully developed for IR, nor has it been used empirically. Given the growing numbers of advocates for a focus on practice in IR and political science (Kratochwil (2007), Neumann (2002), Adler (2005), Huysmans (2006), Fischer (2003), Wagenaar (2003)), the increasing calls to understand IR as scientific practice(s) (this panel?) and that science studies' fourth perspective advocates for such an understanding, future studies are to be expected. The crux about practice theories might be seen in 1) their rejection of any a-priori position of whether internal or external explanations are to be favoured or wherever such a

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<sup>45</sup> In some sense this is a surprising move by Wæver (although understandable from the perspective of his 1998 article which is the starting point of this paper), as he seems to be in his influential writings on security in favour of agency, rather than technocratic or neo-neo-functionalist theories. Why not a speech act of IR?

<sup>46</sup> I would doubt that a marriage of Bourdieu and Whitley is possible. This is not only difficult as two very elaborated vocabularies need to be translated to each other, but also because Bourdieu in many ways relies on a sophisticated version of realism – at least this is my reading of Bourdieu (2004), see the related criticism in Lynch (2000) and Latour (2005).

boundary should be drawn; and 2) their attempts to balance structure and agency in seeing them in a mutually constitutive dynamic relation (“global microstructures”, “actors macro-structuring reality”).<sup>47</sup>

Poststructuralists have partly taken such a perspective, but they tend to be interested in the long-term structures and orders produced by practice, rather than in practice itself. In our own proposal for studying IR’s scientific practice (Büger and Gadinger 2007a, 2007b), we drew on Latour and argued to focus on the practices of concept development, self-governance, boundary drawing, alliance building, mobilization of the world and public representation. We argued that these are useful domains by which scientific practice can be ordered and analysed.

Further the study of IR practice is usefully combined with studying local actions and techniques, such as writing, quoting, presenting, styles of reasoning and publishing, practices of peer review, etc. Heidrun Friese’s (2001) study, observing the practices of authority construction at a sociological conference, is noteworthy in this regard. Dvora Yanow (2006) has put forward some concerns on such a perspective that sets up on laboratory studies. She argued that the laboratory so decisive for these studies is non-existent in political science. This is, however a very limited understanding of both, the laboratory, as well as laboratory studies. True, political science is not operating in a lab, as high energy physics does. But anyone who has ever had trouble with his printer, email or power point or has been astonished by the results SPSS has produced for him, will not doubt that contemporary political science is decisively shaped by (social and material, if we prefer to distinguish it) technology. Also we do not have to search very long for our laboratory sites, whether these are conferences or advisory projects.<sup>48</sup> The history of deterrence theory is instructive in this regard (Lawrence 1996, Robin 2001).

While these are debates on how to proceed with disciplinary sociology, I shall come back later to, we should not neglect that meanwhile considerable efforts are made to reflect on scientific work in outlets such as the section on teaching in *International Studies Perspectives* or *Perspectives on Politics*. Although I did not make the effort to review this literature in detail, the creation of these outlets and the renewed interest in pedagogical work and career pattern, form a part of a sociology of IR. Given that teaching makes up not only a major ground of justification for academic IR, but is time-consuming everyday work, it should not be considered a minor issue. Nonetheless the study of practice, whether in the format of studying everyday actions or of studying ordered practice is still in its beginnings.

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<sup>47</sup> Many would suggest that also Bourdieu as a major theorist of practice would fit into this. This is reasonable if it is acknowledged that Bourdieus work is less coherent and better be understood as offering to different readings, a more structural-objectivist and a more practice-experience oriented. See King (2000) for such a distinction.

<sup>48</sup> See Law (2004) for such an enlarged understanding. Bockmann and Gill’s (2002) discussion of Eastern Europe as a laboratory for economists is an interesting social science example for this.

*Summary: IR's disciplinary sociology*

Even in my limited inventory, sociology of IR proves to be a lively field. The range of scholars engaged in it and the growing recognition of its results suggest that it is a field of growing significance. Let me draw some conclusions from this inventory in the light of the questions I raised.

1) The relation between IR and sociology of science is not non-existent, but (so far?) largely limited to a) the American reception of the Kuhn-Lakatos exchange, b) some eclecticism citing sociology of science to make frameworks studying national communities more reasonable, c) Guzzini's reading of Kuhn, d) the knowledge society narrative, and e) Ole Wæver's several attempts to use the sociology of Randall Collins intellectual networks, Richard Whately's structural account and Peter Wagner and Björn Wittrock's discourse coalition approach.

To provoke, most scholars seem to be somehow aware that something must have happened in the sociology of science, since Kuhn, but have hesitated so far to engage with anything out there. Despite Wæver's engagement, scholars that at least point into the direction of using sociology of science (Smith, Holden, who else?) rather tend to rely on social theorists they read anyway (such as Bourdieu and Foucault), than first to start with reading an introduction to the sociology of sciences and then decide what is useful for their problem. As a general assessment, one is tempted to say that, thirty years after the publication of Kuhn's *Scientific Revolutions*, the Kuhnian revolution has still not reached IR.

2) Disciplinary sociology is more than discussions on how to write a good textbook, but also a wider field than some of the European protagonists want us to believe. It is far more than complaints about American hegemony, the study of some freaky indigenous IR communities or the demolition of mythical great debates. Those are topics of disciplinary sociology, but as shown other thoughts and discourses belong to it as well. This needs to be kept in mind, if it is only to prevent us from any argument that might be raised in future, of the kind "US IR might be hegemonic, but we (Europeans) do disciplinary sociology and reflect on what we are", There is no need to reproduce the U.S. versus. the rest of the world discussion on a meta-level.<sup>49</sup>

3) Struggles of how to conceptualize the relations in the trias between knowledge production, its environment and its translation into praxis, are a significant topic in the sociology of IR. While

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<sup>49</sup> Such a tendency is already visible in Steve Smith articles, and also somehow present in Holden's (2006) distinction between "Anglo-Saxon historiography", meaning those doing reflections on the discipline the American way, vs. "cross-community comparisons", meaning largely rest-of-the-world authors.

paradigmatic accounts offer us silence, poststructuralists claim the hegemony of the environment over knowledge production and the majority of textbooks offers us at least a causality between events and scientific change, sophisticated thoughts in this regard are:

- Historiographers addressing this issue as a problem of internal vs. external explanations. Schmidt (2006: 257) follows Holden (2006) that “the controversy of internal and external, or contextual is fundamental”. Scholars have failed, so far to actually demonstrate why it is so fundamental and more then a private debate between Holden’s reading of Quentin Skinner and Schmidt’s version of John Gunnell.
- National community researcher as a problem of content vs. institutional environment
- Largely unconnected from these, theory and policy discussions have raised the issue as a problem of expertise, as a problem of science-media-politics relations or as a problem of transforming theory into political praxis.
- [...]

In sum the interrelationship of organization, environment and translation has been recognized, yet we are not at the state where scholars went beyond description.

4) Is the sociology of IR narcissistic? The majority of disciplinary sociologies in my inventory stay detached. If they carry a prescriptive they do not exemplify it reflective. For instance Holden (2006:231) is right in criticizing the historiographical wing, by arguing that “the paradox of the argument put forward by Schmidt and others is that they leave themselves open to a ‘So what?’ objection, because they are unable to show why corrections to the conventional historical narratives matter in any major way for contemporary practices”.<sup>50</sup>

Those discussions stemming from the theory and policy debates that do provide prescriptions on how to transform scientific practice are unconnected to the rest of disciplinary sociology. Hence the whole issue area of how academic outcomes are translated into political practice is absent from the majority of disciplinary sociology. Those theory and policy discussions that work out prescriptions are themselves problematic, not only do they randomly build on systematic forms of observations on the discipline. and they do not exemplify the political nature of their recommendations.

Despite poststructuralists, disciplinary sociologies advocate for an apolitical view of IR. Politics is understood as something outside of IR. Scholarly debate about how this ‘external’ influences the

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<sup>50</sup> This is not to say that such a case cannot be made, rather to the contrary, and indeed it has been made partly by Thies (2002).

‘internal’ (apolitical, value-free?) IR discourses. With such a restricted understanding of politics, the politics and power-effects inherent in IR’s discourses come out of focus.

Even if disciplinary sociologies want to stay in the frame of any intern/extern distinction, and wherever this boundary is drawn, they need to acknowledge that from such a perspective a politics/science relation is threefold: 1) political practice transforms scientific practice, 2) scientific practice transforms political practice and 3) the practices of scientists are epistemological and political. Some science studies scholars rightfully stress that it is useful to speak about capital-‘P’-politics, referring to the politics and policies in a society and small-“p”-politics, referring to the politics of scientific practice. These might be a device for disciplinary sociologies to keep in mind the political of scientific practice. An alternative is to reject any form of intern/extern, content/context, knowledge/object distinction in recognizing that these dimensions are interrelated to a degree that they become indistinguishable. If we remember the case of Joliot discussed by Latour, this is what has been argued in the fourth tradition of science studies.

A third case of why disciplinary sociologies might be narcissistic arises, when we have a look at the arguments given by scholars for why they conduct disciplinary sociology. The following motives can be identified:

- Legitimizing and de-legitimizing certain ways of doing science or telling the history of the discipline
- To mark the boundaries of what is IR and what it is not
- To reconstruct the identity of the discipline
- Education purposes
- To enable “cross-community” communication.
- We have nothing else to do
- [...]

All of these are motives that do not guide the sociology of science into an engaged direction.

5) To sum up this discussion a range of pressing *problématiques* runs through the literature. The first and most obvious is a low degree of coherence and low connectivity between the different ways of studying IR and the related networks of literature and scholars. Although national community studies and historiographers are finding slowly to each other, other disciplinary sociologies are largely disconnected from this debate.

Second, much of the studies are conducted to legitimize or de-legitimize certain forms of knowledge production or ways of interpreting the history without providing alternatives of how to transform scientific practice. Schmidt describes this *problematique* as the problem of presentism. Holdens (2006) comment that this is a minor problem needs to be rejected. If we are out for harsh and uncompromising self-examinations, it is not sufficient to demonstrate that a certain way of doing IR is ‘degenerative’, a certain reading of IR history is ‘false’ based on the historical ‘facts’, others have failed to study the important issues, have used the wrong methods, that the wrong knowledge has been produced or scholars have spoke in the name of elites, instead of the “powerless”. If a sociology of IR is conducted to denunciate, or if it is perceived as such, it is useless. To some degree it is unavoidable that scholarly arguments, considered to be relevant by their colleagues, always are justification and de-justification or ‘presentism’. But this can be done in a reflective way. I would suggest that IR’s disciplinary sociologies can learn in this regard from a recognition of Bloor’s ‘strong programme’<sup>51</sup>. The principles of symmetry – statements to be considered true or ‘false’ by the actors need to be analysed by the same means – and of reflexivity – means used for analysis should be applicable (and applied) to the analyst – have been explicitly designed to cope with these problems.

The third and maybe most dramatic *problematique* are the narcissistic tendencies of disciplinary sociology. Studies often fail in leaving the self-referential circle. As I discussed above scholars tend not to recognize the multiple interconnectivities between scientific practice and political practice. They tend to neglect the importance of the transformative capacity of a sociology of IR. Further the current prime objects of disciplinary sociology are forms of communities either in the format of some de-contextualized paradigms/theories or in the format of national communities. Wævers contribution in this regard, in first clarifying that IR is (and can be) a global network as well as a local (national) one, and second in advocating for a perspective on the social science of which IR forms part of, cannot be emphasized enough. To use an analogy introduced by Johann Heilbronn (XXXX), IR has studied itself as many have studied the foreign policy of a state, in taking the perspective on the discipline for granted. Instead of recognizing the global politics of the social science and transnational, transdisciplinary relations, the focus has been on the state, the discipline. However, the same as a state is part of a system of states, a discipline is part of a system of disciplines. The same as the system of states (international relations) is shaped by transnational relations, is the system of disciplines (the sciences) shaped by transdisciplinary relations. Wæver seems to be aware of this. If his comparative disciplinary sociology will share

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<sup>51</sup> Although it has its own shortcomings. Some of the problems I discuss below. For better critiques see Latour (2005), Pels (1996) and Lnych (2000).

the same fate as comparative foreign policy studies in IR, is however open to the future. In sum, there is a need for the sociology of IR to go political, global and local.

### 3. ‘Cultural Studies of IR ’? Some Suggestions

In my short discussion of science studies, I argued that the fourth tradition of science studies might be the device by which we can cope with these issues. These are the researchers that have provided innovative ways to study science, and further claim to have found solutions for some of the problems just discussed.

In the following I like to elaborate more on the approach of a ‘cultural studies of science’ developed by Joseph Rouse (1996). I do not claim to be able to solve the above discussed problems – some of them are rather dilemmas and antinomies, than problems –, nor shall I argue against other concepts that are currently in preparation for usage in IR (for instance in this panel), or other science studies accounts that might be used for IR in future. I shall sketch how the argument presented by Rouse is significant in our present context and can open a path for revising disciplinary self-examinations.

In his *Engaging Science: How to Understand its Practices Philosophically*, Rouse makes a case for a distinct version of science studies by largely addressing philosophers of science. He opposes those attempts in the philosophy of sciences understanding science as determined by relations to objects or by distinct methods, as well as science studies’ social constructivist (the strong programme), who attempt to reduce the sciences to social ‘factors’.<sup>52</sup> Those studies form, what Rouse denominates as the “legitimation project”. Advocates of these perspectives:

“tried to settle questions about the legitimation of scientific knowledge by interpreting the structure or historical development of the content of scientific knowledge and the ontological status of its objects. Either the cultural authority and political autonomy of scientific inquiry have been justified by showing how the content of knowledge is determined and related to the world or challenged by showing representations are accepted in ways that afford no global legitimisation.” (Rouse 1996:20-21)

Rouse’ argument should be accessible for the ‘professional-trained IR epistemologists’ as well as those interested in sociology of science. Rouse puts at the centre of his argument the concepts of ‘significance’ an ‘intelligibly’, ‘practice’ and ‘narrative’, and ‘power’ and argues for an engaged way

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<sup>52</sup> In this way Rouse has a similar starting point (problematique) as Richard Whitley, whose work has been mobilized for IR by Wæver (2003:8, see my discussion of way 6b).

of doing science studies (“science studies should be understood as engaging scientific practice rather than just interpreting it” (31)). Hence he promises us a path to circumvent the narcissist problem, without falling back in to an sophisticated objectivism a la Bourdieu.

With some parallels to better known IR accounts – Iver Neumann’s (2002) concept of practice, narrative and discourse and the communities of practice idea (developed by Etienne Wenger (1998) and introduced to IR by Emanuel Adler (2005)<sup>53</sup>) –, Rouse argues that scientists operate in an environment of coherence and contest, provided by temporal and continuous restructured narrative fields, fields which are the grounds to make scientific practices intelligible and significant.

### *Significance*

Rouse argues that ‘significance’ is the most fundamental epistemological issue, certainly more fundamental than ‘truth’. Questions of significance govern the codification of achievements, answer which results should be published, how these results should be framed, and how research should be redirected accordingly. “Foregrounding significance reminds us that most truths about the world are scientifically irrelevant or uninteresting; recognizing the difference between important and insignificant claims is indispensable for understanding scientific practice” (21).

### *Practice*

Practice Rouse conceives in a way similar as *pragmatism* and *symbolic interactionism* have done. Practices are patterns of activity in response to a situation. Thus, practices are here understood in a non-representational way. They are not understood as the concrete doings of human agents by which agents created meanings, but as the meaningful situations by which doings are meaningful. As Rouse (1996:38, my emphasis) puts this, “meaningful patterns are not bestowed *on* the world *by* agents or their shared forms of life, but *emerge from* patterns of interaction *within* the world”. Practices are dynamic because patterns only exist by being continuously reproduced. Coherence and continuity of practice hence depends on the coordination work of multiple persons and things and the continuous maintenance of it. Such maintenance work is incredible hard work in the case of scientific practice, as scientists operate in very different environments (local contexts). Hence there is room for considerable slippage in the ongoing reconstruction the ‘same’ scientific

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<sup>53</sup> Although some doubts needs to be raised, if Adler has not misunderstood this concept by stressing the coherence of communities, over their network, entangled character. If the social is constituted by a meshed, intertwined plurality of communities organized around one practice, how can there be anything like an ‘epistemic community’ with shared values, beliefs and common policy project? This problem might however be well a problem of Wenger’s account as he continues to use the problematic term ‘community’.

practice, practices are interpreted very differently, and sometimes the pattern even breaks down<sup>54</sup>. Scientific practice thus is caught in a continual tension between significance and incoherence. Scientists manage this tension by what Rouse calls “the narrative reconstruction of science”.

### *Narrative*

By narrative Rouse does not understand the literary form (a story with a beginning and an end), but “a way of comprehending the temporality of one’s own actions in their very enactment” (27). Scientific practices and achievements are intelligible if they have a place within enacted narratives that constitute a developing field of knowledge, and they are important to the extent that they develop or transform these narratives (170). The appeal of such an understanding of scientific practice as narrative reconstruction lies for Rouse in being an alternative to standard view that scientific work “becomes intelligible and important against a background of a research community’s shared belief and desires”. Such a view is not plausible as it overstretches coherence, hence do not consider the interplay between significance and incoherence and cannot cope with situations in which scientific practice transforms a community’s prior commitments or changes what counts as the relevant scientific community. Instead of what constitutes a scientific community, what is its history and future is frequently at stake.<sup>55</sup> What is hence in common among researchers “is a field of interpretative conflict rather than any uncontested commitments about beliefs, values, standards, or meanings” (172). The future and the past become intertwined in these constitutive conflicts. “Conflicts of over what is to be the future course of research in the field [...] are simultaneously conflicts over how to interpret its past.” (172). To engage in one research project rather than another is to (attempt to) reconfigure the story that would make sense of that project within its historical situation. With similarities to Actor-Network Theory Rouse also speaks of narratives as “epistemic alignments”: “Skills, models, concepts, and statements become informative about their objects only when other people and things interact in constructive alignment with them” (27).

### *Power and an engaged science studies*

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<sup>54</sup> Which is, for instance often the case, when new things (epistemic objects) emerge that need to be integrated in the pattern, or a new practice arises. Disciplinary or sub-disciplinary differentiation or the division between theory-oriented work and policy-oriented work are cases of these. To give a very broad speculative example the discovery of the object “globalization” (see Fischer 2003) led to a crisis of the practice of doing international relations and attempts to establish transnationalism studies, and international political sociology, etc.

<sup>55</sup> Rouse provides a telling example: Why are textbooks and state of the art articles continuously rewritten? If new results have been produced, why not just publish occasional supplements? The answer is they are ongoing reconfigurations.

Narratives, alignments and (re)configuration practices are not only device by which significance and intelligibility is provided, but also constitute a range of power relations. They are narratives of power in the sense that they establish relations between knowing subjects and give authority to one or the other. Power is mediate by them and hence needs to be understood as situated, and dynamic. If this is the case, and this is Rouse next move, it needs to be acknowledged that science studies do not operate detached or independently from narratives and power relations, but *within* them. Science studies are situated in power relations, and this is certainly true for IR's disciplinary sociology. When science studies are aware that they are reconfiguration moves themselves, they have transformative capacity. For Rouse this implies that science studies have to be conducted in an ethical way, in asking by which narratives and reconfiguration practices, which power relations become established and who and what (objects, things, practices) become excluded or marginalized.

*Cultural Studies of Science and the disciplinary sociology of IR*

In what way this discussion open new (significant? intelligible?) paths for the sociology of IR shall come under consideration next. These are however some very sketchy initial thoughts that need to be developed further.

What is this thing called IR? Following Rouse there is now easy answer to this question. IR is no stable thing but a continuously reconfigured narrative structure. Nonetheless the fact that multiple people refer to "IR", signify its coherence, and the debates I discussed over these pages signify its incoherence. IR is not one thing, but interpreted in different narratives differently. To paraphrase John Law (2004), IR is not one world, but *many*. Such a statement implies that there are not unlimited IR's. There is no plurality, but *multiplicity*. Grasping multiplicity is however not an easy thing and will require much further work.

Should we study schools, national IR's, or a global IR? For Rouse IR is local, national and global the same time. An IR practice becomes significant, reliable and is indeed constituted by its reliance on a narrative field. This can be in principle the narrative of science, of social science, of social science in Britain, of IR in Norway, of political science in Germany or of global IR – to give some examples. Hence, instead of asking which communities are more significant (a global or a national?), the question is one of which practices become intelligible and significant in which narratives.

Is IR influenced by internal or external factors, by political culture and ideology? Again, now easy answer. It does first depend on what ‘IR’ (see above), we focus on. Second, identifying different alignments and narratives will lead to different networks of IR, and different ways in which things, objects, etc. are arranged. Again the question is not if IR is influenced by ‘factors’, but which practices, become significant in relation to which other practices, things, objects, narratives.

Do we need a disciplinary historiography of IR or a sociology of IR? From Rouses perspective it becomes clear that both are interrelated. If IR is conceived as a continuously reconfiguring narrative and if we want to make sense of IR (and transform it based on our results), it will be the job of a sociology of IR, to study IR practices of reconfiguring its past (historiography), present and future. If such an endeavour is successful (considered to be intelligible and significant) it will reconfigure IR and as such will become part of future studies.

How can a sociology of IR be political? It should have become clear that a sociology of IR is not only describing IR, but producing it. A detached sociology of IR, in which we describe what it is in ‘fact’ is not possible. A detached sociology of IR, enacts a detached IR – and this is what we want to avoid? A sociology of IR that neglects power, will leave these power mechanisms intact that we then talk about at coffee breaks, instead.

If we enact IR by studying it, the question might ultimately boil down to what Annemarie Mol has called *ontological politic*, to a question of what IR we politically favour. An IR driven by the power-effects of professional discourse? An IR in which scholars (we) are task-fullfillers and reputation-maximizer? An IR independent from society?, etc. However, as Law (2004:67) has put this, “in an ontological politics we might hope to interfere, to make some realities realer, others less so.”

#### **4. Conclusion: Paradigms, Cultures and Translations**

“Reflexivity is not an epistemological, moral or political virtue. It is an unavoidable feature of the way actions (including actions performed, expressions written, by academic researchers) are performed, made sense of and incorporated into social settings. In this sense of the word, it is impossible to be unreflexive.”

(Michael Lynch 2000: 27)

"The sociologically armed epistemological vigilance that each researcher can apply on his own behalf can only be strengthened by the generalizing of the imperative of reflexivity and the spreading of the indispensable instruments for complying with it; this alone can institute reflexivity as the common law by the field, which would thus become characterized by a sociological critique of all by all that would intensify the effects of the epistemological critique of all by all."

(Pierre Bourdieu 2004: 91)

"What does this mean in practice? The answer is that I do not know."

(John Law 2004: 156)

Michael Lynch (2000) reminds us that reflexivity is neither a virtue in itself, nor can it be identified with constructivism or any other critical or radical programme. Reflexivity is an essential human capacity. The present crisis of IR, marked by calls for useful knowledge, proposals of 'going beyond IR' and nagging doubts about any achievements the sciences can offer, increases the need to think consequentially (?) on reflexivity towards scientific practice, or self-examinations as I called it. In other words a debate of *how* IR wants to reflect on its own practices is needed.

I argued that epistemological debates and meta-theoretical reflections are neither useful in this regard. Nor are they, if we follow Peter Wagner's (2001:86) claim that the present situation "is probably a historically new experience, but it requires no new epistemology", even needed.

Instead, I stressed the importance of the currently evolving 'sociology of IR'. Such a disciplinary sociology, however, needs to avoid becoming 'disciplined' and 'narcissistic' by thinking to strict inside the borders of the discipline. Hence, I highlighted that attempts addressing the organisation and institutionalization of knowledge production, the institutional conditions shaping it and the translations between IR and other cultural spheres require attention. Sociology of science can be in this regard a powerful resource for IR. My inventory of current disciplinary sociology in IR led to the conclusion that IR researchers have only sparsely connected to these resources. So far, disciplinary sociology struggles with a range of *problematiques* in a way that they are rather unproductive reflectivity, but also inadequate means to cope with the present situation.

I suggested that many of this *problematiques* cannot be solved, but at least their consequences be milded by paying more attention to science studies. The principles of the strong programme, although problematic in itself, can be an initial guidance for future studies to cope with the problem of legitimization. To circumvent narcissistic tendencies, political consideration should return to the sociology of IR. There is a need to widen and to limit the perspective the same time.

In sum, a future sociology of IR needs to go political, global and local. To open such a path for disciplinary sociology, I sketched that a turn to a ‘cultural studies of science’ perspective, might equip us to handle these tasks. This is however only one way to go.

To end with some remarks on reflexivity, if the mission outlined in this paper is taken seriously, I have failed in many regards: Although I suggested that I did address significant problems of scientific practice, my own focus was on texts alone rather than practice (discursive practices?), I did not comply with any principle of self-reflexivity, for instance, in reflecting on my own status, my own objectives and my position in the field of IR (a PhD student at a ‘European university’?, a dilettante in science studies?) or in throwing the maybe too harsh and provocative criticism I raised against existing literature at my own paper; neither did I make use of a coherent science studies approach (ANT? CSS?), as I called for. When we reconsider the discussion of Law and Wiliam’s paper on how relevant articles are written, the presentation of the array I assembled, might even have failed to be considered ‘reliable’ or ‘valuable’ by my ‘colleagues’.

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