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From epistemology to practice: a sociology of science for international relations

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Without question there is no shortage of reflexivity in the discipline of International Relations (IR), to the extent that after several 'grand debates' and numerous 'turns', it seems to have reached a certain intellectual 'surfeit'. One of the reasons is certainly that many of the questions concerned are not logically solvable, and that debates on reflexivity tend to become affective if not religious from time to time. Another reason is that debates are often scholastic, and have nothing to do with either the social life of the researchers or the objects studied. This surfeit should not, however, be an argument for refraining from reflexive exercises. After all, standards of reflexivity are what distinguish scientific practices from those of other knowledge producers. Instead, this observation should lead us to reconsider the connection between the abstract, theoretical, conceptual and the practical everyday. Therefore, this contribution argues for an extended understanding of reflexivity centred on practice and taking advantage from works in the sociology of science.

Reflexivity that wants to connect better the intellectual and the practical needs to pay attention to many more aspects than epistemology. The promise of reconnecting theory to practice has been demonstrated in recent epistemological debates. Scholars drawing on 'old' or 'new' pragmatism (e.g. Kratochwil 2007; Pouliot 2007; Hellmann 2009) have elaborated perspectives

that escape the objectivist/subjectivist and realist/interpretivist divides. Indeed, practical epistemologies are possible without falling into the trap of relativism. Epistemological arguments, while important, are, however, only one piece in a larger puzzle. The focus of epistemology on ideal typical practices of producing knowledge does not grasp core aspects of the everyday scholarly life, which might be considerably more important than often suggested. Science is first and foremost a social practice involving many more activities than establishing relations between the knower and the known. Scientific projects are materially and socially situated; they require material, financial and human resources; they are structured by socialisation and disciplinarisation; they require knowing subjects, who are gendered, marginalised and/or authorised; they involve negotiations about relevance, significance, instruments and methods; they require a range of institutions and techniques; and they are also a political practice involving ethical considerations of all sorts.

To be clear, IR discourse is certainly not ignorant of the aspects of disciplinary life listed above. Yet, such reflexivity is practiced at sites and in formats and genres that certainly do not satisfy the standards usually considered obligatory for good scientific practice. Rather than in peer-reviewed journals, those aspects are debated in private, at conference receptions, dinner tables or coffee machines. Or the absence of public and systematic reflection is compensated by a profit-driven genre, which no one cites but many read: the blossoming market of practical advisory literature. Self-help books titled 'how to write a Ph.D.', 'how to write an op-ed', 'how to survive tenure track' or 'how to publish' have become bestsellers.

Some scholars have become increasingly aware of the need to change this situation and bring these issues to the heart of academic debate. Indeed, scholars have taken up the challenge to go beyond epistemology and have investigated the past, present and future of the discipline as one way of studying, directing or even constituting 'the international'. A nascent number of studies have re-told the early history of the discipline, providing different readings of its birth and evolution. Scholars have become concerned with how the structure, mechanisms and practices of the discipline have shaped the way the international is thought. Moreover, researchers have increasingly seen the discipline not only as a cognitively isolated one, but as a project shaped by institutions and structural and environmental factors. Taken together, these studies present a new, more advanced and broader project of *social* reflexivity. Disciplinary sociology as a significant field of inquiry and as supplement and alternative to epistemological reflections has emerged. It would be, however, an exaggeration to claim that disciplinary sociology has reached adolescence or even the core of the discipline's curricula.

One reason for this is that the majority of studies fall short in demonstrating the need for and potential of sociological reflexivity. Instead, a sub-disciplinary

niche has been created. Early research was shaped by a crude eclecticism and a lack of analytical rigour; anecdotic reasoning dominated, and there was a shortage in thorough engagements with recent sociology of science (e.g. Wæver 1998; Bueger 2007). This has changed in recent years, with more sophisticated frameworks and increasingly comparative approaches taken. A much more pluralistic and colourful picture of the discipline has been sketched, especially if compared with the prevalent categorical systems of a discipline divided along three paradigms or the axes of postmodern/modern, critical/problemsolving or rational/constructivist, and qualitative/quantitative. We have learned about IR communities in Iran or Russia; about how US IR might be hegemonic, but does not hinder other IR projects to develop; and about how vibrant transnational IR camps spanning disciplinary boundaries have been prospering.[1] Yet, strong arguments for why a project of sociological reflexivity should be pursued and how such a project can be advanced remain absent. I have argued that sociological reflexivity is a project that reconnects the intellectual and the practical. In other words, it is a project that goes beyond sending postcards from 'exotic' IR communities around the world, or telling heroic narratives about the achievements of non-US communities. To contribute to the debate on the purposes and usefulness of sociological reflexivity, in the following sections I draw on the work of contemporary social theorists, and first reconstruct the main objectives of a sociology of science for IR. Second, I outline some of the options for pursuing such a project in future research. I argue for the promises of a 'cultural studies of science' perspective and suggest focusing on practice, organisations and concepts.

The Value of a Sociology of Science for IR

A wide spectrum of social and political theorists agrees on the importance of directing scientific reflexivity towards everyday practice. This includes eminent thinkers in the tradition of Critical Theory such as Martin Heidegger (1933), Pierre Bourdieu (1975, 1984) and Jürgen Habermas (1978). Yet, such concerns are not limited to works in the tradition of Critical Theory. Thinkers such as Thomas Kuhn (1996) whose works have become part of the key IR repertoire argue along similar lines. It bears some irony that IR has read these works not as a call to study practice empirically, but as pleas for philosophical contemplation. Notably, this concerns Kuhn's work, who was above all interested in scientific practice and problem-solving activities, and indeed counts as one of the fathers of contemporary sociology of science. But the reasons why a wide range of theorists agree on the importance of basing reflexivity in practice are threefold. The first reason relates to the position that social scientists have in

society; the second to the quality of research; and the third to the education of future scientists and the evaluation of scientific achievements.

IR as a Constitutive Element of Global Politics

First, in the light of the dissemination of scientific practices in all interstices of political life, the study of the discipline can help us to understand better the knowledge that constitutes and informs political decision making.

As historians of science have reconstructed, science has always been about political order. Shapin and Schaffer (1989) have shown in their seminal *Leviathan and the Airpump* how the controversy between Thomas Hobbes and Robert Boyle about the status of experimental science was a controversy about the authority of science and how the political should be ordered. They showed how deliberations about science were fundamental to a new political order and argued that politics were tied up in what scientists did, rather than what they said. Carroll (2006) shows how science (such as population statistics) made the modern nation state thinkable, and Wagner (1989), investigating the disciplinary disaggregation of the social sciences, demonstrated the affinities between disciplinary discourse and political organisation.

IR has not neglected such results and arguments. Indeed, Oren (2003), for example, has demonstrated the co-constitutive character of political science and US foreign policy. Moreover, in IR the position that the discipline has a performative power — that it makes the world it studies — is much more widespread than often assumed. Indeed, a broad range of IR writers agree on this. Russett (1993: 136) stresses the prescriptive character of democratic peace research. Others argue that the 'ideas' of IR travel to the policy world and are one causal source of policy change (e.g. Risse-Kappen 1994). Studies of transnational communities and networks argue that IR scholars form part of these and influence the formulation of policy (e.g. Adler 1991). Scholars relying on field, discourse or network theory suggest that academics create spaces of authority exerting high influence over the epistemic foundations and the formulation of policies (Kennedy 2005; Bigo 2008) or define the realm of the thinkable through the development and elaboration of categories and vocabularies (e.g. Walker 1993; Smith 2004). In summary, IR scholars have cottoned on to the fact of IR's capabilities to influence policy formulation through their direct participation in policy formulation, as experts and advisors, through their participation in collectives, and (maybe most importantly) through their representations of the world.

This rarely recognised emerging consensus not only casts further doubt on the claim that there is a growing chasm between IR and the worlds of politics; it highlights the importance of a research agenda that sees IR

scholarship as a constitutive element of world politics and investigates the particulars of how IR makes the world it studies. That IR does something with the political world, and that we need to understand this performative capacity, hence constitutes the first major reason to engage with sociology of science in IR.

The Emancipatory Potential of Sociology of Science

Second, sociological self-examinations highlighting the practical plays of power are an important corrective for any scientific discipline. To say that scientific practice produces political order is not to elide the practical differences between science and other forms of doing politics. To paraphrase Latour (1983: 167), science is a form of politics pursued by other means. It is exactly the second major objective of sociologies of the discipline to investigate the character of those means. Investigations of the organisation and practices of knowledge production in the discipline can highlight how agendas are pursued and policed and how political interest is translated into disciplinary practice.

Like any other organisational form, scientific disciplines enable and constrain their members. Disciplines are a means of organising the research mission of universities; they are a means of establishing a scope of authority over a certain set of problems and phenomena and defending it against forces of political and social control; and they are a way of articulating and defending professional solidarity and identity (Post 2009). Disciplines are communities organised by a distinct set of shared practices, vocabularies and institutions (Bueger and Gadinger 2007). They give meaning and legitimacy to academic practices such as writing and presenting research, reading or teaching; they are a means to evaluate one's status, and give intelligibility to distinct claims to knowledge.

Sociology of science is a way of elaborating and reflecting on these enabling and conditioning dimensions of disciplinary life. Yet, as most forcefully argued by Joseph Rouse (1996), disciplines are not determining forces. The view that scientific work becomes only intelligible and important against a background of a research community's shared beliefs and desires overstretches coherence. Scientific practice is in a continual tension between significance and incoherence. Often a scientific practice transforms a community's prior commitments or changes what counts as the relevant scientific community. 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 (Rouse 1996: 170). A telling example in this regards are textbooks. Why are IR textbooks and 'state of the art' articles continuously rewritten? If new

results have been published, why not just publish occasional supplements? The answer is, textbooks are ongoing reconfigurations of disciplines. What is hence in common among researchers 'is a field of interpretative conflict rather than any uncontested commitments about beliefs, values, standards or meanings' (*ibid.*: 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' (*ibid.*). To engage in one research project rather than another is then to (attempt to) reconfigure the story of that project that would make sense within its historical situation.

Narrative fields and (re)configuration practices are not only devices by which significance and intelligibility are 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 mediated by them and hence needs to be understood as situated and dynamic. If this is the case, it needs to be acknowledged that any sociology of science operates not detached or independently from narratives and power relations, but *within* them. Sociological investigations are situated in power relations, and this is certainly true for IR's disciplinary sociology. When sociologies of the discipline of IR are aware that they are reconfiguration moves themselves, they have a transformative capacity. This implies that investigations have to be conducted in an ethical way, in asking which power relations become established by which narratives and reconfiguration practices, and who and what (objects, things, practices) becomes excluded or marginalised.

Rather than mourning the marginalisation of this and that kind of academic practice, the sociology of science is a way of using the scientific repertoire to point to disciplinary deformations, policing and marginalisation mechanisms, and power constellations; indeed, it is a means of reconfiguring them. In summary, sociology of science can be a powerful tool to reconfigure the discipline.

Education, Evaluation and Sociology of Science

If the first objective of a sociology of science for IR is to study the discipline as a constitutive element of world politics, and the second is its function as emancipatory reconfiguration, the third objective is to strengthen self-evaluation and education. Whether intended or not, evaluation and education have largely become external to scientific disciplines. With the rise of the managerial university, standards for evaluating academic performance have become largely defined by consultants and bureaucrats. A good deal of the

practical training in academic work is provided not by the members of disciplines, but by professional trainers, who teach presentation and writing techniques, advise in career strategies or provide methods training. The best a discipline such as IR seems to offer on these fronts appears to be abstract manuals.

If disciplines are to be seen as communities of practice governed by norms legitimising certain practices as appropriate behaviour (Lebow 2007), it is often unclear where these norms originate or should be developed. The conventional view in IR is that these norms are developed by philosophers of science. Traditional philosophers of science take norms to be epistemic: truth, justification, explanatory power and so on. The problem with these norms is that they are not only too abstract and generic to be applicable in concrete situations, but also that the political dimension of scientific practice discussed above is not considered (Rouse 2003: 465). A less well-recognised alternative is to develop norms from political theory. As most prominently given in Steve Fuller's social epistemology, the goal is to develop conventions of the (democratic) legitimacy of the conferral of authority and expertise (Fuller 1992). A different major source of norms is bestowed on scientific communities by 'managerialism', that is, the introduction of private-sector management practices (Anderson 2008). Managerialism emphasises particular forms of accountability, develops a market orientation, focuses on securing non-government funding, and is concerned with issues of efficiency and economy. It goes along with evaluation practices, including performance management schemes and benchmarking, the restructuring of academic departments or the implementation of budgetary devolution. The norms of managerialism are quantified and regulate scientific behaviour through the number of articles in distinguished journals, impact factors, grant sums or the number of graduate students supervised.

IR has primarily contemplated the first source of norms, widely neglected the importance of the second,[2] and no more than silently resisted (Anderson 2008) the third source. Arguably, all three types of norms matter in everyday academic life. Arguably all three are, however, not very responsive to the diverse sets of situations of everyday academic life. It makes as little sense to judge a theorist by the number of articles she or he publishes rather than the ideas that theorist develops, as it does to judge a policy advisor's work by the correspondence of her or his words to reality rather than the way the work enlightens a policymaker or helps to solve a problem.

Sociology of science provides an alternative source of norms, or maybe better a different take on them. Sociologists of science aim to elucidate norms as they come from within concrete, historical and observable scientific

practices. Scientific practice is a continuous struggle about which norms are appropriate in given situations. The normative settlements different scientific communities and disciplines reach, for instance by job interviews, peer-review mechanisms, in board meetings or conference panels, are, however, hardly explicated. Yet, if they remain private, tacit and implicit, they can neither be used in evaluation nor in educational processes. Sociology of science is not only a tool that allows one to get closer to practice, thus escaping becoming the object of either the abstract generic norms of the philosophers or the absurd norms of the managers, but it is also a tool that allows the knowing subject and its communities to regain a meaningful autonomy.

The public deliberation of norms in IR (as for much of the rest of political science) has used devices such as abstract manuals or contemplations of a positivist epistemology. Kratochwil and Friedrichs rightfully doubt the usefulness of these:

> Although it is doubtful that any IR scholar has ever conducted research the way King, Keohane, and Verba describe it, most scholars stick to the methodological 'organized hypocrisy' of positivism, which is a self-vindicating and self-justificatory discourse that seeks to establish social scientific credibility and rigor despite its practical nonapplicability. Everybody knows, but nobody recognizes openly, that no one actually follows the stylized steps of hypothesis formulation, testing, and so on. Popperian fantasies about ingenious conjectures and inexorable refutations continue to hold sway despite the much more prosaic way most scholars grope around in the formulation of their theories, and the much less rigorous way they assess the value of their hypotheses. (Kratochwil and Friedrichs 2009: 710)

Existing sociology of science in IR already provides alternatives in discussing knowledge generation from a broader and indeed more practical perspective. Yet, the usefulness of sociological accounts for education and evaluation purposes is hardly recognised or outlined in a straightforward manner. Indeed, a rewriting of the history of the first great debate may help the student to gain critical distance to the stories the teachers tell; studies of national disciplinary communities provide useful maps to access and navigate; statistical analysis of career and publication patterns may assist the early career researcher in choosing publication strategies; or a survey of top publishing houses, professors and graduate schools may assist the student in identifying the best graduate school and publication outlets. However, as long as the eye does not turn to scientific practices and to controversies and their settlement, a sociology of science for IR undersells its potential in both educating its future members in how to practice IR, as well as in offering alternatives to current norms of evaluation.

The Way Forward

The sociology of science is rich in approaches useful for the study of IR. If the objectives are to unravel the performative capacity of IR, to conduct sociological studies in an ethical manner with a transformative interest, and to elaborate norms for education and evaluation based in scientific practice, I suggest that approaches described as 'cultural studies of science' are the most promising path to pursue. Cultural studies of science take science to be a social formation constituted by practices and in constant relation to other formations (such as politics, journalism or neighbouring scientific formations). Science is understood as a continuously reconfiguring formation, given the slippage of scientific practice and the recurrent introduction of new practices, objects and concepts. The discipline of IR should be considered a living, evolving structure; it is made and re-made in everyday practices.

Such studies can take basically three routes. First, they can dissect single practices, such as the practice of writing; second, they can study the interplay of practices inside specific organisations, such as the *International Studies Association* or the journal *International Organization*; third, they can study how a distinct knowledge claim, an object, technology or concept, such as the concept of sovereignty, is circulated in IR.

Practices

If practices are what constitute scientific collectives, one route is to investigate the actions that enact and reconfigure a practice. Academic practices are multifaceted. Interesting instances may include the practices of writing, reading, teaching, supervising, publishing, presenting, conferencing, advising, consulting, organising meetings or hiring.

Although this is not the place to elaborate on the different versions of theories of practice,[3] studying practice will help to unravel the practical knowledge governing the discipline and to distinguish between different situations. If we consider the practice of writing in IR, it will be important to distinguish between writing an article for *International Organization* or an article for *Foreign Affairs*; writing a grant proposal will differ from writing a Ph.D. To differentiate between situations does not necessarily mean to document all of the byzantine complexity we will face. It will often be more useful to provide overviews. A promising starting point for categorising situations in which scientific practices thrive is Dick Pels' (2003) triangle of politics/the state, culture/science and economy/the market. Pels suggests distinguishing the various degrees by which scientific practice is influenced by economic, political and cultural rationalities. For instance, writing a policy brief requires considering political and market rationalities to a much higher

degree than does writing a theoretical article, which will be primarily influenced by cultural rationalities. Practices contain normativity. Different collectives and disciplines will have different understandings of what constitutes a good piece of writing, what can be written and how. For instance, a lengthy description of the preparation of a field visit will be appropriate in an anthropological context, but not in an IR one. Or consider the example of chairing a conference panel. Chairing is, besides other tasks, about regulating scientific discussions, and about defining what is a worthy contribution to the discussion and what is inappropriate cheap talk. Dissections of practices in various situations will assist in unravelling these regulatory norms.

In reconstructing these practices in the light of the three objectives, the task will not only be to get an understanding of the community under study, but to investigate how relations to non-IR actors are established and maintained in these practices, how disciplining power effects play out, and which norms govern the respective practice.

Organisations

Organisations are sites that host different practices. Investigating organisations such as the *International Studies Association* or the journal *International Organization* provides the opportunity to study the interplay of practices. As discussed in Bueger and Gadinger (2007), organisational practices can be categorised as the 'logistics' of science (such as the organisation of databases or self-governance), the 'formation of alliances' through the enrolment of funding agencies, clients and publics and the 'mobilisation of the world' (such as translating the actions of a foreign policymaker into a scientific article). In and through these practices, knowledge circulates and 'content' is made and re-made.

Practices will often be in harmony and perpetuate each other. Studying organisations is hence a means to investigate how a community maintains itself. But studying organisations as hosts of practices also offers the opportunity to bring controversies and struggles between practices and their diverse communities to the fore. Examples of such controversies in IR organisations include negotiations about prizes and awards, negotiations about amendments to the ISA constitution in the planning committee, negotiations over publication policies, or the annual bargaining over panel slots at the Annual Convention. Taking such instances as a focus, the analysis can gather important insights into the constitution of the community, in observing by which justifications and normative standards the community settles these issues.

Concepts

As historians of science have observed, scientific change and the birth of new communities — sometimes even entire disciplines — are often related to the invention of new (epistemic) objects, technologies and concepts. A third route to take is hence the study of objects, technologies and concepts, and how they transform communities and their relations to others.

The study of concepts has already shown promise for a sociology of science of IR. For instance, Buzan and Wæver (2007) discuss how the concept of '(national) security' led in the 1950s and 1960s to the formation of the sub-discipline of security and strategic studies out of a heterogeneous ensemble of military historians, weapon specialists, physicists, international lawyers, social theorists and political scientists. Discussions of concepts such as 'transnational organised crime' or 'failed states' have shown how IR becomes connected to a diverse set of actors ranging from Area Studies to the World Bank and foreign ministries. As shown, in the process of its circulation the meaning of the concept as well as the actors participating in its circulation are transformed. Worthwhile concepts to study will be notably those that prosper in the grey zones between IR, international organisations and state officials, such as 'peacebuilding' or 'global governance', to name but two. The strength of such a route lies in that it allows us to carve out empirically the practices by which IR connects to other actors, how it performs politics in collaboration with them, and when and how IR develops scopes of authority and defends them.

To conclude, if IR is a constitutive element of world politics, these insights are promising as they foster our knowledge about our main object of study and how we contribute to the constitution of it. They will help increase the quality of research, as disciplinary deformations, policing and marginalisation mechanisms are identified. They will improve education, given that teaching is based on the everyday practice of doing IR, and they will assist in regaining the space for autonomous self-evaluations based in the discipline's own interests. These are compelling arguments for why more of the scarce resources of IR are well invested in a project of a sociology of science of the discipline.

Notes

1 Cp. the contributions in Tickner and Wæver (2009).
2 But see the discussion in Ish-Shalom (2009) and Russett (2005).
3 See Bueger and Gadinger (2008); Neumann (2002); Hajer and Wagenaar (2003).

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