



## Everyday practices of international relations: people in organizations

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*Journal of International Relations and Development* (2012) **15**, 110–120.  
doi:10.1057/jird.2011.29; published online 2 December 2011

The idea that there are biases, blind spots or exclusionary if not oppressive forces in the very way scientific endeavour is organised still appears to be a rather strange idea. It runs counter to the ingrained idea that science is reflective. Science is still predominantly associated with the idea of a separation between values and facts and a clear separation between subject and object, that is, the normative ideal that researchers are detached from their 'object of study'. With it comes the idea that knowledge and power need to be separated before the scientific enterprise can enjoy the fruits of objectivity and neutrality. True knowledge can only be produced where power is absent. Yet, regardless of whether one subscribes to, for instance, the Kuhnian notion of paradigm shifts, Wittgenstein's idea of therapy, or Foucault's arché,<sup>1</sup> as soon as the well-trodden paths of positivist philosophy of science are re-situated within a series of relations, practices, institutions, and persons, questions regarding scientific endeavour stop being solely confined to objectively instituted rules of evaluation.

In line with this development, we have witnessed the emergence of a growing literature in the sociology of IR which shows how IR produces its knowledge about the world, what its modes of inclusion and exclusion consist of, how newness emerges, and how contingencies are produced and its scientific vocabulary given meaning. While many of these contributions reconstruct the sociology of IR by examining syllabi, books, journals, the geography of authorships, and/or conference participations, ours points to the *organisational* and *quodidian* dimensions of IR: practices of IR, from everyday work done in front of some screen to the publication of new articles and books, are continuously reproduced by people in organisations like universities, scholarly associations, conferences, publishers, state bureaucracies, ministries, and research funding institutions. This contribution thus pursues a rather modest question: what comes into sight, what topics and problems are raised, when one approaches the sociology of IR from the perspective of sociologies of



the organisation and the everyday? How can we account for the fact that practitioners and organisations are not disembodied vectors of the scientific endeavour but its agents? To pursue these questions, we first briefly review the link between post-positivist thought and sociology of IR, show how the current literature in the sociology of IR is structured, and point to organisations and people as a possible locus to understand IR better as an academic field. The second part looks deeper into the sociology of organisations. The third part tries to outline how the effects of organisations can be made visible by reconstructing everyday practices.

### **Losing my discipline: lost hegemony, lost soul?**

The sociology of IR has so far identified two major themes of inquiry. The first theme analyses the biases resulting from the fact that IR is predominantly a North American science (see, in particular, Wæver 1998). Accordingly, it is often mentioned that there has been a strong divide between North American and European approaches to IR. While American social sciences are said to be dominated by various strands of positivist and rational choice approaches, European social sciences are said generally to have successfully resisted those voices (see Jørgensen 2000 for an attempt to explain the sources of such differences). The literature has so far not only shown that American journals, American conceptions of what 'true' scientific work entails, and American organizations dominate practices around the globe, but that one can even identify a 'de-Europeanisation of American IR. American IR is cutting itself off from those of its roots that are continental European and is building increasingly on a liberal, Anglo-American philosophical tradition' (Wæver 1998: 688).

The second theme focuses on how IR has gained autonomy as a discipline and reproduces/protects its constitutive boundaries. The way the discipline focused on the concept of 'anarchy' to delimit an exclusive scientific field where a specific expertise was needed is a probing example here. Anarchy somewhat became the identification criteria for one to be part of IR. As long as anarchy and its adjacent concepts, such as sovereignty, war, and of course the state system, were seen as the prevailing heuristic force behind (North American) IR, the discipline and its researchers were clearly identified. However, anarchy seems to have lost its pivotal role for theorising IR: the 'death' of state is no more the sole imminent and pressing problem of world politics in the face of *global* risks and uncertainties, and the advent of 'new' actors, along with new conceptualisations of spaces and temporalities, challenges the reduction of *international* politics to the state. This more complex approach to IR comes at a scholarly and identity price: what IR is, where it is to be found, what the disciplinary boundaries are up to, or which literature is to be read and



mastered, is today utterly dependent on what one think the ‘international’ actually *is*. Ultimately, IR redefines itself and its relation to other disciplines. International Political Theory and International Political Sociology have developed as new fields of study; IR scholars engage with key debates in social and political theory; and we now study everyday lives, states, cities, diasporas, regional organisations, and networks among other ‘units’ of analysis.

Both themes suggest that the sociology of the discipline is in various ways linked to questions of the post-positivist turn in social theory, which has entered IR via the so-called third debate (Lapid 1989). That said, this debate has not only a philosophy of science dimension (i.e., the difference between explaining and understanding), but also a sociological one. So far, the sociology of IR was reconstructed by evaluating the structure and content of syllabi, the geographical dispersion of authorship, the geography of journal indexing, and conference participation. Though these accounts are certainly relevant, we think that a look at our everyday practices such as publishing and teaching classes cannot evade the existence of organisations. In our everyday lives as academics, organisations are omnipresent: when we publish journals or publish a book, when we attend conferences and exchange ideas, when we apply for research grants, administer money, hire people, accept students to programmes, we do so within the context of specific organisations.

Much of our everyday communication in academia occurs in terms of organisational decisions. Organisations are where practices happen, where they are disciplined, even fomented towards people, where people act, react, strategise and so on. It thus seems a natural step to ask: what comes to sight when we pursue the question of a sociology of IR through a sociology of organisations? The next section explores some of the contours such a sociology can take.

### **Power and knowledge and ... organisations**

Nobody has captured the particularities of organisations and bureaucracies like Franz Kafka. Kafka identified their pathologies and dark sides up to a point of unbearability. He showed how their violence remains hidden behind rational routines, how these routines create simultaneously transparency and opacity, how they impact on everyday life, and how they create subjectivities and identities. Organisations provide opportunities and constraints, foster inclusions and exclusion, openness and closure; they create a reality that is uncertain and hard to grasp. In Chapter 17 of Kafka’s *Castle*, a revealing exchange between the secretary ‘Mr. Bügel’ and the protagonist, the land surveyor ‘K.’, highlights how organisations are internally organised, how ‘issues’ create files, how files move from desk to desk, and how decisions are produced. Organisations are organised and continuously reproduced around documents: documents that make up files,



documents that prepare, that comment, request, command, inform, inquire, or proof. Documents deal with cases where peoples' lives enter only in a pale, distant, and incongruous way. Bügel shows that the heart of organisational activities is neither individual preferences nor the distinction of informal/formal organisation, but this very organisation and circulation of documents. Think about the continuous reproduction of documents within a university or the documentation of projects with research funding institutions like the European Union. Documents take on the form of applications, publications, requests, letters of recommendations, evaluations and so on. Candidates are employed as a result of some bureaucratic decision; the acceptance or rejection of some submission is impersonalised and presented as a result of some objective process; project proposals are decided; reports are continuously produced to satisfy organisationally created demand just in the same way as organisations require and conduct evaluations, produce numbers and rank papers, as the current overemphasis on publications' 'impact factors' illustrates.

This mixture of the assumed objectivity of processes and impersonal violence, the amorphous dispersion of authority, the everyday experience of ambiguity, fiat, and power, seems to us to be omnipresent in science as well. Consider the contrast between the often assumed objectivity, neutrality and rationality of hiring processes, acceptance to conferences, and the peer-review process with all the gossip and complaints about it that are exchanged at conferences' hotel bars. Promotions and tenure procedures, peer-review processes, evaluations for research funds, publications, projects management: they are all related to organisations and are often enough Kafkaesque. Who has not heard stories of personal interventions in search committees? Who has not heard of unsolicited letters? Or of dubious tenure rejections (e.g., Morgenthau's)? This is not to be misunderstood as the sole result of individual motives (although they are certainly relevant); this entire 'dark' side of every academic discipline, we claim, is somehow related to organisations and the way academic communications are structured.<sup>2</sup>

But what can this organisations-based sociology of IR look like? To develop this perspective further, we think three avenues are particularly relevant: (a) a sociologically informed concept of organisations; (b) an awareness of the changing constellations of organisations; and (c) a reconstruction of the forces at work in these constellations via oral histories.

### **A sociological concept of organisations?**

It would be wrong to accuse IR of lack of interest in organisations. The entire regime literature can be read as proposing different approaches to organisations, focusing on their impact on cooperation patterns, for example,



but usually by retaining an understanding of organisations as essentially autonomous entities. For our purposes though, without disregarding its value for other purposes (e.g., Finnemore 1996; Finnemore and Barnett 2004), this literature is limited. For the purpose of our discussion, it seems more promising to start with Kafka's insight that organisations are not given, but self-producing entities.

Organisations are systems that produce themselves (Weick 1977: 273, 1995; Brunsson 1985: Ch. 3, 1989; Luhmann 2000: 45). Organisations stabilise the boundaries between themselves and their environment; they show an internal complexity based on processes, rules, procedures, communication, and reporting channels. Through this continuous self-reproduction, institutions limit and filter their access to their environment: not everything that happens in the environment is necessarily of relevance. Even more: what and how something is or is not relevant is defined by internal rules. For example, it is the internal rules, structures, and procedures of academic organisations or of organisations linked to the financing, management, and exploitation of knowledge, that define: which grants and proposals scholars can apply for and how they should do it, the necessity of a report, the ways to submit a research proposal, the expectations behind a job application, the value of a letter of recommendation or of an evaluation, the requirement to attend committees and so on. Three corollaries can be drawn from this. First, within this conception of organisation, human beings in themselves are especially relevant in their specific organisational roles: as scholar, head of departments, clerk, secretary, or assistant (we will see later how we can turn the analytical focal point to the individuals themselves). Second, the individual actors' intentions or motives are less central to this sociology of IR, as the focus is put on the organisations defining who the actors are and what the range of their possible moves consist of. We believe it is the continuous self-reproduction of organisations that define science today: a promotion is denied, a candidate chosen for a position, a manuscript accepted for publication, a research grant denied, etc. Third, this implies that organisations cope and solve problems they themselves produce. The evaluation of scholarly by-products, 'inputs' or 'outputs', such as publication records, teaching records, teaching programmes, or conference participations reflects how these peculiar and inchoative events are only aggregately significant through organisations.

### **Change in the constellations of organisations**

With this analytic focus on organisations, the relevant question naturally becomes whether all organisations are alike: are there not differences between scientific and economic organisations? As such, the question of how



organisations relate to social logics and rationalities (like economic rationality, political rationality, legal rationality, etc.) provides a second path of inquiry for a sociology of IR. The clash of rationalities not only within but also between organisations is commonplace. For example, whenever an assumed *economisation* of science is opposed to its public function, or when the question of what kind of relations policy makers and IR scholars should, can, or must have (or avoid having) is posed, the question of these linkages and clashes is addressed. What will happen if the newly elected conservative government in the UK reduces public spending? Does science really play a public function, and if so, what does it consist of, or is science an entirely self-referential process? Do social sciences only possess legitimacy if they offer empirically value-added work as a way to foster better technocratic means to govern or solve problems? What is the relationship between politics, the economy, and science?

A good starting point to address these ever-changing constellations of organisations is the discussion around what has been termed ‘Mode 2’ within sociology of science (Gibbons *et al.* 1994; Nowotny *et al.* 2001). This literature assumes that we witness a significant overhaul in the way scientific knowledge is produced and legitimised. Whereas ‘Mode 1’ describes a disciplinary and theory-oriented mode of knowledge production, which puts universities at its centre, ‘Mode 2’ is more policy-oriented and interdisciplinary. With ‘Mode 2’, scientific organisations overlap to a higher degree with economic, political, and legal organisations. As this literature points out, in this latter Mode, universities alone are neither the sole purveyors of knowledge nor the sole organisation able to define what knowledge consists of; communities of practice outside their boundaries increasingly define what knowledge is, and how it is to be applied and used.

For example, policy-oriented organisations are gaining influence on the production of scientific knowledge. Take the scientific output of the IMF (Kessler 2008), for instance. This output impacts on scientific debates, and actually helps transform the legitimacy of the IMF itself: whereas the IMF’s ‘old’ legitimacy derived from its status as an inter-governmental organisation with the specific mandate to facilitate cooperation, today’s IMF is a transnational organisation that legitimises itself on the basis of having ‘better’ economic knowledge. Its authority and legitimacy are irremediably linked to its expertise in economic knowledge. Another example is professional schools for public policy in well-regarded universities that offer special ‘access’ or a specific relations to policy makers (one could think of the Kennedy School at Harvard). In the absence of such professional schools, it is likely that conflicts between more ‘theoretical’ and more ‘policy-oriented’ research orientations occur within the same department rather than between different organisations.

Although we can only hint here at future lines of research, we suggest there is a need to understand ‘Mode 2’ as a process between different and evolving constellations of inter-organisational linkages that have an impact on the ways



by which IR is constituted as a field of knowledge practices. With this in mind, we conceive of three areas for future discussion and research: (a) ‘Mode 2’ and its links to the economisation of knowledge and the neo-liberal governmentality; (b) the politics of scientific knowledge and the relation between IR and policy makers; and (c) the will to numbers through universities’ increased dependence on external research money, and its links to the objectification of knowledge through many form of metrics (bibliometrics, impact factors, journal ranks, citation statistics, university or department rankings and so on). However, a fourth line of study, not exclusive of the aforementioned ones, is also worth singling out: everyday practices.

### **Observing everyday practices: IR of the people, by the people, for the people?**

While the two earlier sections concentrated on the organisational dimensions of IR, we would like to conclude with an engagement with the very people whose practices are at the heart of these organisation, whose interaction, cunning, creativity, scheming, or enthusiasm within these constellations of organisations are the actual engines behind scientific endeavour. Though assessing the discipline in terms of organisations is a potential direction that a sociology of the discipline can take, it is certainly not the only one. Therefore, we hope to provide an alternative view as to how we can mobilise different sociologies to understand IR as an academic field. From what we just mentioned about organisations, it might seem paradoxical to suggest a turn to everyday practices. Yet, we think it would be a mistake to presuppose a specific conceptual framework and define ‘organisations’ in advance. We believe it is more interesting to reconstruct and make visible the effects, techniques, practices, and ‘rationalities’ of organisations through oral histories. We have to remember that organisations are part of everyday practices and it is through these everyday practices — at one’s department or university, in an editorial team, at a conference (preferably at the bar) — that one’s reputation is made or unmade, publishers met and charmed, projects designed, candidates evaluated and so on (Van den Bergh 1974).

While everyday practices are what ultimately lie at the heart of all our ‘conceptualisations, definitions, and narratives’ (Featherstone 1992: 160), they remain largely in the realm of ‘the familiar, taken-for-granted, common sense, and trivial — in short, the unnoticed’ (Hviid Jacobsen 2009: 2) when it comes to thinking about IR as an academic field. Though only some dimensions of this ‘unnoticed’ have started to be explored, most notably the practice of writing as either an alienating or redeeming practice from the perspective of the discipline (Doty 2004; Inayatullah 2011), we want to narrow our focus to what we believe is a central project, in order to understand the



impact and significance of organisations for quotidian practices on IR: to retrieve, record, inventorise, and study the memories and the recollections of IR scholars. This locates organisations within the context of three specific *problématiques*.

First, the history of IR so far generally favours one of two lines of explanation for its state: it is either done following an almost purely internalist comprehension of scientific progress (think of the infamous great debates) or an externalist comprehension that accounts for the evolution of the discipline as a result of external shocks, such as the end of the Cold War. However, these explanations for the evolution of IR as an academic field often take into account a historically inaccurate reconstruction of IR's past, underestimate the place of purely intellectual influences, and, let's put it straight, neglect 'fashion' or 'trendiness' as a major factor influencing the orientation of one's research (Schmidt 2002). The historiography of IR is thus excluding from its gaze more mundane activities such as conversations, the influence of a fashionable intellectual icon in helping a certain body of scholarship coalesce, emerge and spread, or the strategic positioning of one's own work in regard to specific debates. Conducting an oral history of IR can precisely allow us to identify, for instance, the effects of organisations. It would also allow us to identify which conversations, among whom and in which (organisational) context, led to the development of professional association. It could also help us map out which intellectual influences were crucial at certain moments and why they were so, how and why certain key individuals in the history of IR shaped the discipline, and how they used their power to shape departments, allocate research grants and so on, while impersonalising their interventions in bureaucratic terms. Such an oral history could also retrieve moments of the history of IR, of our history, that are left out from most of our textbooks, but still had their importance beyond a whiggish and western-centric reading of our field (Tickner and Wæver 2009).

Second, as a scientific community, it is striking to see how all of us share many stories about our field — anecdotes, hearsay about the past, representations of certain events, representations of certain scholars, and their subtle or not-so-subtle influence on the field and so on — yet none of this material has been used to understand IR as a (research) community. Oral history is one of the prime tools to (re)construct how a community represents itself; it enables us to map differences in these representations, identify what these differences sanction in terms of what other forms of sociology of knowledge tell us and so on. This is especially important, as IR is a community that can be understood as both a hypothetical and real group. It is simultaneously a group of scholars linked to each other through their positioning in organisations, hence the result of a heuristic 'work of symbolic or classification struggles', and a group of scholars whose definition as a group is the result of 'practical and political





work of organising and mobilising' (Isin 2002: 26–27). In other words, this community is composed of those who not only participate in a field, but also perceive and construct themselves as being scholars in a discipline. Furthermore, we should keep in mind that IR is not only *geographically* differentiated, but also *generationally* differentiated. Although the study of the international within academe is young compared with others, it has enough history to comprise several generations of scholars. An oral history of IR could thus help us understand and contrast the different conceptions and perceptions of the field offered by an intergenerational reading and mapping of IR.

Third, and finally, oral history can help us shed light on what we consider to be the most mundane practices of organised academe — publication, job application, reviewing process, tenure, job interview and presentation and so on — that constitute our identity as scholars in light of certain core values, excellence and objectivity being certainly the most important ones. These values often justify the special place the University holds in many societies. Yet, these values are somewhat relative to a myriad of practices that create many 'folk-histories' and myths. An oral history of these practices might help us not only better understand the extent and actuality of these practices, but also counter-balance the 'official stories' of academe. Overall, an oral history of IR's academic practices can help us demystify those official stories and constitute our collective memory as practitioners and scholars (Morrissey 2000). In the words of Charles Morrissey (2000: 16): 'Helpfully, this scrutiny might ventilate the academic hallways of power and upgrade the conduct of academic employment'.

## Notes

- 1 What these authors, among others, highlight is the situatedness of each form of knowledge. It is important to stress that we do not disregard or have contempt for 'positivism', but in order to understand International Relations (IR) as a scientific endeavour, we have to pay attention to practices that constitute it, as they are reflections of power relations.
- 2 For analysing the internal logic of organisations, it is advisable to distinguish the horizon of possible actions, positions, and powers that organisations create from individual actions based on specific motives. That both are separated is easily seen when we acknowledge the extent to which we can replace people from specific positions without changing the structure of the organisation.

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## Accounting for the politics of language in the sociology of IR

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*Journal of International Relations and Development* (2012) **15**, 120–131.

doi:10.1057/jjird.2011.30; published online 11 November 2011

With its 2009 report on the state of the discipline of International Relations (IR), the *Teaching, Research, and International Policy (TRIP) Project* of the Institute for the Theory and Practice of International Relations aimed to tackle directly Ole Wæver's claim that IR 'is and has been an American social science' (Wæver 1998: 687). Driven by the question of whether or not one could see national variations in the way scholars think about the discipline, and if one could agree on the existence of a single IR discipline, the TRIP project engaged in an ambitious ten-country survey about 'the state of the discipline' (Jordan *et al.* 2009). The choice of the ten countries surveyed then (Australia, Canada, Ireland, Israel, Hong Kong, New Zealand, Singapore, South Africa, United Kingdom, and the United States) reflects an obvious yet unmentioned selection criteria: these countries use English as the main language of scientific communication.<sup>1</sup> If no consensus could be reached as to whether IR was an American discipline *per se*, there could at least be an implicit consensus that to be considered and acknowledged — and thus evaluated, measured, and assessed — the discipline(s) of IR had to be Anglophone by definition. This presumption also underpinned Stephen Walt's recent commentary on the persistent dominance of Anglo-Saxon scholarship in IR. 'I'm still struck', he admitted, 'by the relative dearth of "big thinking" on global affairs from people outside the trans-Atlantic axis, including continental Europe. And by "big thinking" I mean ideas and arguments that immediately trigger debates that cross national boundaries, and become key elements in a global conversation' (Walt 2011).

I suggest that the impacts on knowledge production of this imperative to write in English in order to be *acknowledged* as 'doing IR' have been understudied in their theoretical, material, and emotional implications. This