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<CT>**Stubbornly Stumbling into Making History: Constructivism and Historical International Relations**

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<head1>**Introduction**

<text\_fullout>On the first lecture in International Relations (IR) we attended, we were met by the number 1648 written large over the whiteboard. This date, we were told, marked a supposed watershed in international affairs, the birth of international affairs as we know them. To Halvard, who had come to Political Science from studies in History, this did not resonate well. To Benjamin, for whom IR was the first topic studied at the University of Oslo, this only became problematic once he discovered that even Stephen Krasner – who was generally talked about in quite laudable terms by his professors – had, years before Benjamin embarked upon his studies in IR, sought to debunk the creationist myth of Westphalia (Krasner 1993). The feeling that the mainstream had an issue with history, pushed us towards it. The fact that this issue was reflected even in some of the biggest questions we were asked to grapple with as first year political science students, made us interested in how big things came about and changed.

We never planned on being constructivists. Nor were we supposed to be International Relations (IR) scholars. To be sure, we are still unsure about whether either of us, both

separately and together, can fit under the constructivist label, just as we have done our best to operate at the margins of the subject-matter of discipline, nourishing what to many surely must seem as a rather quaint interest in international politics past, rather than present. As the IR scholars we have become, through muddling through different topics and theories, we are probably what one may term Constructivists by coincidence or default, and Historians by association.<sup>1</sup>

Yet on our way to IR through the social sciences, through Political Science, Sociology and History, we stumbled into one another and onto historical ways of doing research in IR. And we stuck to those ways – stubbornly, and often at a cost. Put differently, history and politics intervened, and eventually we found ourselves doing something that prompted the editors of this volume to ask us about history and constructivism in IR. The editors have challenged us to elaborate on this trajectory, and, aiming to please, we will oblige by offering some reflections on how methods relate to broader questions of contingency and (auto)biography. How the methods we use can be less the result of strategic choices, and more of a way to (tactically) cope with often sub-optimal strategic decisions. We hope that these reflections on our experience may be of use to younger scholars engaged in the Constructivist enterprise, however broadly defined, and in what now has become known as Historical International Relations. Our experience may be unique, but being a product of history and politics, scholarship and friendship, it is not uniquely so.

The tactical aspect translates into this essay in many ways. At the most obvious level, constructivism as a tactic deals with how to go about doing research and answering questions posed within a constructivist framework. This aspect of constructivism has been less

important to us, as we have never engaged in what could be termed “mainstream constructivism”. To us, as we will elaborate on below, constructivism was a greater idea, the source of bad strategic decisions, but also, in the end, a way to make up for those decisions. Dissatisfied with the positivist mainstream we were taught as undergraduate students – a mainstream that would most probably have served us well in terms of jobs, job security, publications and funding – we were drawn by the allure of what we then understood as Constructivism. Opting for Constructivism was a choice utterly devoid of strategic considerations – we didn’t know yet that we would engage on the road now traveled – and with potentially dire strategic consequences. While this may not be broadly known, Norwegian IR is probably as American as US IR is – if not more so (see Leira 2014).

The editors have challenged us to include what they called an important auto-ethnographic element as a way to reflect upon how we have come to do research in practice. Autobiographical recollections are notoriously unreliable as source material. One should expect memory misattribution, embellishment of the authors’ role, forgetfulness and outright lies. Readers are hereby warned. Not least, one can expect more purpose than contingency. However, our goal here is the opposite. First to highlight contingencies at the expense of strategy, second to stress the extent to which what may be constructed as purposeful strategies, are in fact often the result of having desperately come up with a number of tactical choices in order to make up for bad strategy – or, most often, no strategy at all. As such, reflecting upon how we got to our mid-career limbos, the extent to which so little of it was planned and so much of it was the result of unforeseen circumstances, is striking. Befitting this volume, we have neither triumphant end-points nor successful strategies. The only way we can account for where we are today, is by emphasizing the contingencies involved at each

level. We have no better story to tell about how this all went down and have decided that the story will have to do. We hope to be lucid enough to make it a useful read, and candid enough to make it entertaining.

In the end, we both feel that we landed on our feet – whatever others may think (see Steele’s chapter in this volume) – and we are happy with where we have landed. Ultimately, it is this process of landing which we will reflect on here, thinking about constructivism as malleable, as a set of possibilities, and showing how these can be worked with. Our specific way of doing this has been through seeking to draw historical research and constructivism closer, by emphasizing not only their mutual affinities and commonalities, but the extent to which they depend on each other. Having now pursued historical scholarship through several waves of IR theorizing, and indeed the alleged end of such theorizing, we hope to be able to contribute to making IR a better-grounded subject. On this score, we hope to come up with a number of “methods as tactics” as suggestions for other scholars.

Thus, the aim of this chapter is threefold; first we try to recollect through the hazy dim of personal history and histories how we eventually became the researchers we are today. Second, we focus on what to us at the time – and, to some extent, still – appeared as contingent, random and haphazard experiences, so as to present a more coherent account, and account that we hope may be a useful tool – or at the least a good read – for younger scholars. In the process, we dwell on choices we have made with respect to how we have sought to approach the world; our approach and our sources. Third, we present an attempt at distilling what we see as the lessons that can be drawn from our work and trajectory, what we in hindsight may call “our approach”, in the hope that the reader will find some useful

tools for her own research, or that we at the very least help open up a space for this type of reflection. We elaborate on what we perceive to be the benefits of our preferred approach, and how it may be useful for engaging with scholars beyond the confines of constructivism.

## **<head1>Reflection**

<text\_fullout>Our story begins in Oslo, Norway, the world's smallest city or the world's biggest town. We came to the University of Oslo in the mid 1990s from different backgrounds, different places, and with different goals. Benjamin came from a multicultural upbringing in Geneva, Switzerland. He arrived at the University of Oslo with Marx's *Manifesto*, heavily underlined, in his back pocket and a penchant for Camus. While he had not yet really grasped what it meant to be a political scientist, he thought Political Science sounded like a good place to start. Halvard came from a deeply academic upbringing in Norway, having spent his childhood playing detective, with Wu Tang Clan in his headphones, aiming to further science. Benjamin missed out on most of the good music of the 1990s because of his obsession with the 1980s.

We followed similar undergraduate paths, both studying Political Science and Western European area studies, parting ways with Benjamin majoring in Sociology and Halvard in History. Reflecting back on why our association has been such intellectual and social fun, our common early attempts at making sense of large-scale changes through the texts of Charles Tilly (in our area studies syllabus, as it was deemed too sociological to be on our political science introductions) and Stein Rokkan (through our comparative politics courses, which *had*

to include a few texts by the one and only claim to fame of Norwegian ‘Historical-Political Comparative Politics’ – as our professors called it) were the common baggage that made us able to connect intellectually. We had both, as undergraduates, taken pride in actually making sense of Rokkan’s so-called Diamond Model, and thought his analysis – however complicated and misunderstood – brilliant. We shared an academic interest in exploring how things change, both at the macro and the micro levels, and a social and practical political interest in seeing how sausages are made.<sup>2</sup> In short, we were interested in politics and history.

Not coincidentally, we met while doing student politics at the Department of Political Science. It should be noted that this was a department where Wendt (1992) was introduced on the curriculum in 1997 only, but interpreted as the example of how wild things could be on the outside of the safe disciplinary boundary imposed by our carefully grafted departmental syllabi. As we learned later, the discipline of IR had been involved in a large transformation with a number of new approaches flourishing and ‘debates’ taking place throughout the 1990s. While we had managed to catch a glimpse of that, we had no real understanding of what it all entailed, and were in the late 1990s diligently preparing for our exams in what we were told was the forefront of the field, the Neo-Neo debate. With Norwegian Political Science being famously (and, some would say, overly) Americanized (Thue 2006; Leira 2014), and seemingly unresponsive to students’ request for diversity, we got engaged in a political struggle to change the curriculum. Not that we knew what we wanted or what was out there, but we sensed unease at the fact that we were not told the whole story, as our department was actively engaged in keeping to the safe ground of the Neo-Neos.

We persevered, but the gains were short-lived, as the few texts the student body managed to lure into our syllabi through a curious alliance with a few 'subversive' professors, were reversed as soon as we left. Not that any students had read them anyway, having been informed by their lecturers – we were told – that texts by such obscure authors as Max Weber were not really relevant to the final exam. Yet in the process we had gained, perhaps sooner than many of our cohort, lived experience with the political quality of any academic assessment. However, we had not yet learned the lesson that challenging the department establishment head-on, in a country that counted only one university where we eventually saw ourselves working, was not a wise career move. But that lesson would strike us soon enough.

And while it would take us some years to acquire the theoretical vocabulary to think and rethink our early experience in academic politics, something had been started. The process also marked our first encounter with constructivism. Constructivism was heresy. It was forbidden by departmental bulls, and therefore also curiously tempting. Even though we knew precious little about how Wendt's "Anarchy is what states make of it" related to the 'critical theory' a few graduate students had brought back from studies at the LSE, wanting to be a constructivist felt strangely liberating.

What we were also exposed to in this formative period was quantitative methods. We both had the pleasure of learning how to calculate Pearson's R by hand, but even though we did not find analytical statistics difficult, more often than not they did not speak to the questions we were concerned with. We did not choose Political Science in order to perform

mathematics. Formal training in other methods was scarce, and by and large we had to become autodidact, learning on the fly.

Partly as a result of our dissatisfaction with the malaise at the department, our paths departed, with both seeking foreign lands for longer or shorter terms: Benjamin left for the New School for Social Research on his quest for critical theory and Hannah Arendt, before landing at the University of Cambridge's little and cozy multidisciplinary Centre of International Studies where he would work with Charles Jones. Halvard returned to the University of Oslo after a year at the LSE, and finished his studies there. But yet one coincidence was to tie us together in our further pursuits. In the spring of 1999, Iver B. Neumann (through what must have been a glitch in security) gave a graduate course on "Identity in International Relations" at the department.

Neumann had for quite a few years already come to be seen as a symbol of heretical rebellion in Norwegian IR. Interestingly enough, to a large extent he is still considered so. His fleeting presence throughout our undergraduate studies through a lecture here and there had kept our interest alive. He was also largely the reason Benjamin had chosen the New School, having advised him to follow his heart in deciding where to go to grad school. At any rate, we both came to follow his course, from Oslo and New York respectively, and it changed everything. Until then, we had, from different starting-points, seemed destined to meet in some sort of comparative historical macro-sociology in the spirit of Tilly, Mann and Rokkan. Benjamin had even gone further down that road following a course on state-formation with Gianfranco Poggi at the New School. Now, we suddenly had to deal with constructivism and poststructuralism in IR, and thus with theories explicitly dealing with the political quality of



knowledge which we had recently experienced first-hand. Our different graduate schools (the LSE and Cambridge) also came to give us one more commonality: a proclivity for the English School in general and especially Hedley Bull, further complicating the question of methods for both of us.

On a more personal level, Iver Neumann was to once again reunite us, as we both in the first years of the new millennium landed (very) junior and insecure positions at the Norwegian Institute of International Affairs (NUPI). Equipped with our newly learnt theories and boatloads of empirics, we were urged by Iver to get our acts going on the international conference scene. Our first attempts reflected our unease with finding a space within the International Studies Association (ISA) sections, and also the fact that we were still trying out different options and voices. While few would remember our early attempts at carving out a voice on the conference circuit, we both loved being at the conferences. We cherished the general atmosphere, 'feeling' the discipline for the first time, meeting and getting to know people we had read before, and discovering friends. We have been attending (almost) every ISA since 2002 (Halvard) and 2004 (Benjamin). What we realized almost immediately, was that conferencing was essential – not necessarily for reputations, but more for feeling at home within the discipline within which we were increasingly grounding ourselves. Having, as mentioned above, some early experiences in navigating the politics of academia, we thus started to carve out strategies to guarantee our presence on a conference program in which we wanted to, but didn't yet, feel at home. But more on that later.

In between, we were trying to finish our Ph.D.s in areas at the intersection between politics and history. To us, this type of inquiry was at the core of IR. To IR, it was rather marginal. It

should also be noted that this type of research was not a central part of our jobs either. As such, our involvement with the scholarly community seldom included the research we undertook and the work we published on the basis of our everyday research jobs, but mostly stuff we worked on in the evenings and weekends (see below). While we both at the outset fiddled around with constructivist thinking, in different ways we settled on combinations of historical sociology and poststructuralism. While our methods were not strictly archeological (Benjamin) or genealogical (Halvard), we certainly were analyzing discourse, and both, independently, decided to do so through massive investment in primary sources, both printed and archival.

While the benefit of hindsight makes this seem like the obvious choice, at the time of writing it was not. It should be stressed that from the perspective of finishing degrees on time or building careers in Norwegian Political Science, we were repeatedly and stubbornly making the wrong moves with respect to broader theoretical frameworks and methods. We did what we did, in the way in which we did it, because we thought that would be the best way to find answers to the questions we were asking. Our choice of method was thus in a very strong sense a choice of necessity. We both found inspiration in Foucault's (2003, 2007) writings about the state, but of more importance was probably the support from our supervisors, Charles Jones (Benjamin) and Iver B. Neumann (Halvard). On top of that, we benefitted from interacting with eminent IR scholars who urged us on. On overall method and methodology, we should particularly stress the importance of Jens Bartelson and Patrick Thaddeus Jackson. Jens' book on sovereignty (1995) opened up new horizons in terms of what could be done historically in IR.

However, on historical methods, we were again largely autodidact. We had some basic training, akin to most historians, and had read a number of texts on the theory of history, but we ventured into primary sources without much of a map. However, our tools turned out to be adequate to the tasks at hand (or so we felt), allowing us not only to finish degrees, but to try to make some more general sense about History and IR and why approaching history as engaged amateurs might actually have its merits (reflected upon later by Halvard, the 'Historian' among us, in Leira 2015).

However, and this must be stressed, working at an applied research institute, we did not have the luxury of focusing strictly on doing historical work based on primary sources. Nor did we have the luxury of spending our time researching what we thought was important. Before, during and after our degrees, we were involved in a number of different smaller and larger projects, using a number of different methods for data-gathering and analysis. We have written interview-based studies, conducted surveys and presented historical accounts based on secondary sources, to name but a few examples. We have been (and still are) involved in more current policy issues. We are thus not purists about methods. On the contrary we have come to accept a relaxed pragmatist position: whatever works best for the question at hand. Accordingly, we have come to attach less and less importance to which theoretical 'camp' our methodological outlook and choice of methods puts us in.

But we digress. We were both involved in different projects at NUPI, but had found a common ground in historical inquiry. We both felt that *this* was the field we had to contribute to. Yet, in our early attempts at making it on the ISA program, we had trouble finding a space for our papers. Would a paper on the emergence of foreign policy fit in the Foreign Policy Section, or

was it a safer bet to add “... in International Society” at the end of the title and submit it to the English School? We had from an early point understood how conference programs are made. Remember, not only do we care strongly about historical stuff, we love making sense of practical academic politics. We understood that there were a few boxes that had to be ticked to increase our chances of making it on the program. We deduced that it made a lot more sense to submit panels than papers to conferences. Furthermore, we realized that panels with a “big name” contributor stood a much better chance of getting accepted than our junior-only panels. Our strategy was initially successful, with some hiccups, although there may have been a reason for that. For instance, we produced nine interlinked panels on English School institutions for conferences in 2007, commemorating the 30th anniversary of *The Anarchical Society*. Even so, the whole exercise of creating panels with insecure awards was inherently unsatisfactory. We didn’t have the seniority to launch these attempts as edited volumes, nor did we have the names to attract all the people we wanted. While the panels themselves were rewarding and made for new friends and excellent discussions, they sort of faded after the end of the conference. It was also difficult to create an environment where our presentations had an interested audience. We presented historical papers and were often confronted by discussants with why our papers didn’t really address the (often English School or Diplomatic Studies) topic of the panel.

The reason was simple. To be able to take part in an international conference setting, we realized we had to speak to an established institutional audience. As noted, adding the odd “... and international order” or “... in International Society” at the end of our titles, and Hedley Bull or some other key English School figure at the beginning of our abstracts became the way to take part in a conversation. Not that the English School wasn’t important to us, or that it

doesn't mean anything to us today. On the contrary, much of our thinking was (and still is) deeply grounded in traditional English School thinking. *But not all of it.* We were tired of having to pretend our papers were about something else than historical international relations *tout court*. What we had found out, though, from attending section business meetings here and there, was that – just as any academic environment – the conferences were also a site of practical political action. We gradually entered public service.

It started with Halvard. Having spent years doing diplomatic studies, he volunteered to be program chair for the Diplomatic Studies Section of the ISA. Careful politics ensured a steady presence of historical diplomatic panels at annual conferences, but still there was a felt dissatisfaction among historically inclined members of the discipline. During the joint BISA/ISA conference in Edinburgh in 2012, we made the initial steps to create an ISA section on Historical International Relations and, with Dan Green, Dan Nexon and Andrea Paras onboard, our efforts were met with immediate support. We had only recently been present at the first business meeting of the Theory section, and thought: if they can make a section of theory, why shouldn't the ISA have one for historical approaches as well? In 2013 the section was accepted, and through its first four years of existence, it grew to over 450 members. Much like in our approach to methods, we pitched the section as a "big tent", where anyone self-defining as doing Historical International Relations should be able to find a home. We had both been members – and still are – of other larger sections of the ISA, and were adamant in our desire to avoid what we saw as unnecessary positioning and bickering so prevalent in them. Thus, what started as a desire among us and a few others to be able to present our historical research at major conferences without making apologies for it, had, in a short time and through a few tactical accommodations, created an arena for hundreds of fellow-minded

scholars. From our perspective, the broad-church approach provides possibilities for furthering all kinds of scholarship. This is also one of the reason that we settled on the 'Historical International Relations' label (see Leira and de Carvalho 2015) rather than more established 'International History' and 'International Historical Sociology' which, in our understanding, while more established were also rather more narrow in defining their fields. While other sections of the ISA have fallen prey to debates about the "right" way to do e.g. constructivism, the HIST section is focused more on debate and engagement than on approach.

### **<head1>Expression**

<text\_fullout>Drawn to the general allure of constructivism through our early political engagements, we were not constructivists until we started writing. And, as noted above, our work tended to be more historical *tout court*, driven by a broader set of macro-historical concerns rather than by the exigencies of the mainstream constructivist agenda. Although our earlier work is much more of a rejection of constructivism – due to its "second generation" emulation (see the chapter by Steele in this volume, as well as Leira and de Carvalho 2016b) – in favor of a loose poststructuralist approach, we have, through working historically, come to reconnect with the constructivist wagon through realizing the commonalities between historical methods and broader constructivist tenets.

Thus, as a number of different methods have served us well, we continue to argue in favor of pluralism in methods for Historical International Relations. We firmly believe that pluralism

with respect to method and methodological outlook is in large part what draws scholars to the field, and that this is worth safeguarding. Especially in the face of different currents of historical research – from global history to more traditional national histories, conceptual history and more micro-historical takes – which mark the field today, we believe that openness to different methods and outlooks is the only way for a relatively parochial group of scholars to hold together. We have therefore sought to avoid all sectarian tendencies, and sought to avoid doctrinal statements on best practices. Yet, we still see great potential for many more scholars to engage more with historical material.

Paradoxically, in spite of the centrality of history to IR theorizing, there is surprisingly little knowledge of historical methods among our IR colleagues. This is even more surprising given the many tangents between constructivism and historical research (see Reus-Smit 2008; Barnett 2002; Leira and de Carvalho 2016b). Over the years we have noted time and again how constructivist and other colleagues, even those with a deep knowledge of methods and methodology, have a surprisingly cursory knowledge of (or interest in) historical methods. This is lamentable, for historical analysis is central to IR. Even futurologists and prognosticators look to the past for sources for their analyses. On the other hand, the lack of interest in methods might not be that surprising, given that ‘historical methods’ have often been a label attached to any methods used by historians. However, while historians do employ the same variety of methods as social scientists, their reflection on what comes before analysis, namely the status of different kinds of sources, is much more fine-tuned.<sup>3</sup> And all too often the disinterest in historical methods in IR is coupled with a skepticism about primary sources. We see few reasons for this fear of primary sources to continue.

To start from the basics, nothing counts as a source until a question is asked of it, which implies that no source 'speaks for itself', but also that any item can be utilized as a source. Utilization is the key here, as there are different ways of utilizing different man-made sources. Focusing for the sake of brevity on human-made sources, they come in two kinds. The first, remains, is often referred to as 'silent sources'. These sources tell us something about their conditions of production and existence, but in principle nothing more. Benjamin's university gown might serve as an example here. Narrative sources on the other hand are sources which refer to something beyond their condition of production; they are 'speaking' sources. Texts are the typical example, but visual and audial sources can be treated in the same way. The dozens of pictures of the two of us on Facebook, over a decade, are an all too telling example. These 'speaking' sources can be treated as relics, telling us something about their origin, but also as giving insight into what the narrative is about.<sup>4</sup> All too often, IR-scholars, constructivists included, forget the first part, namely that narratives also have a story of origin.

When reading narrative sources for content, they can conventionally be divided into three kinds (see e.g. Howell and Prevenier 2001). 'Literary' sources are sources with a message, explicitly telling a story, such as newspaper articles, journal articles and novels. These are quite clearly the most common sources used by IR-scholars, but also in a sense the most difficult to assess, since the story of origin might be difficult to assess. For the two other kinds of narrative sources, origin is typically easier to establish, as these are 'official' sources. Of a more technical kind are sources referred to as diplomatic (in the sense of dealing with 'diplomas') or judicial, such as court records, treaties, laws, wills and contracts. Finally, the third kind of narrative source is sources known as social documents, the stuff typically produced by government officials, ministries, bureaucracies and civil society associations.



Having established that the story of origin (or the 'relicness') of narrative sources is central, one final distinction must be made, namely between primary and secondary sources. A primary source is the first available information about something, like a journal entry or a royal promulgation. A secondary source on the other hand builds on other sources. One and the same source can be both primary and secondary, establishing its own narrative about something while at the same time referring to other narratives. This chapter is, for instance, a primary source for our collaboration, but a secondary source for our educational background. There is nothing wrong with utilizing secondary sources, on the contrary, it can be argued that by necessity most attempts at creating any sort of overarching historical account *have* to draw on secondary sources. However, what is often forgotten, particularly by social scientist turning to history to 'confirm' their story (or 'disprove' existing stories), is that secondary sources add another layer of origin and interpretation to the mix. In the same ways as other scholars, historians approach sources from specific theoretical and methodological perspectives, and try to make an impact among their own peers. Historians necessarily interpret data, and make more or less well-informed choices about which sources to consult. This is another point on which historical methods and constructivism should find it easy to converge.

On top of this bias comes the likely unconscious selection bias of the IR-scholar. Lacking knowledge of the field s/he might easily end up consulting just one 'school' of historiography. This challenge is particularly acute, since some within IR still believe in a form of Rankean notion of history, long abandoned by historians, looking to history for 'evidence' and timeless truths. While constructivists in IR have acknowledged the 'double hermeneutics' involved in the production of knowledge (the construction of social reality + the social construction of

knowledge) (see Guzzini 2000), there has been little further reflection about whether relying on secondary sources implies treating the world of the historians as social reality, and what can be done to do better. We suggest two partial remedies, first a widening of the historiographical scope (reading more secondary sources), second including primary sources in the mix.

At this point someone is bound to ask why we should bother dealing with primary sources, when historians are better trained to handle them. While this might be so, we strongly believe that engagement with primary sources makes for better research, and might even lead IR-scholars beyond the historians. At a very basic level, engaging with primary sources almost by default leads to contemplation about which sources have survived, and which one should consult (thus also to critical scrutiny of how historians have picked sources). As social scientists, we are also likely to ask different questions of the sources than what the historians do, and we might also be interested in different kinds of sources. Our approach to doing Historical IR has often been that of addressing the stories of origin, questioning the taken-for-grantedness of key concepts in IR (see, for instance de Carvalho *et al.* 2011; de Carvalho 2003; Leira 2011; Leira and de Carvalho 2018; de Carvalho and Paras 2015).

Primary narrative sources come in many shapes and forms, and IR-scholars are typically relatively accustomed to dealing with such examples as newspaper articles and parliamentary records. However, as sources these are not unproblematic. Newspapers aim to sell, and parliamentarians to impress their voters. And as we stressed above, when presenting our primary account of our careers, memory is a fickle thing. Historians thus typically prefer sources which have a documentary character rather than being produced for an audience

and/or for posterity, sources typically found in archives. Traditionally, archival studies or broad reliance on primary sources would imply huge investments, particularly of time. Technical developments, in particular digitalization, have reduced the entry-costs tremendously, to the point where lack of access to primary sources no longer is an excuse for sticking to secondary sources alone.

Catalogues and many other archival resources are available online, enabling both sophisticated searches and ordering of items, and high resolution cameras enables storing, thus greatly reducing the time one needs to spend in the actual archives. For primary, printed sources, the situation is even better. Many countries provide national gateways and/or databases for research into historical sources (such as Gallica in the French case), and even more sources are available behind relatively modest paywalls. Furthermore, Google Books (but also sources like archive.org or Gutenberg and Project Runeberg) provide previously unheard of access to books and other written material.<sup>5</sup> This treasure-trove is, as yet, little utilized by IR scholars.<sup>6</sup> Our stumbling upon it has changed the direction in which our scholarship has gone. There is no reason why IR-scholars in general, and especially constructivists, who already have a broad interpretivist outlook on their sources, should find it daunting to incorporate a more historically sensitive aspect into their research by incorporating more work with primary sources. Methodologically, there is no contradiction. Working with primary sources can only make our findings stronger.

In our own research, we have sought to ground narratives in what we hope are broad readings of secondary sources. Dealing with primary sources, we have utilized both archival data (typically social data) of multiple kinds and more straightforward printed sources (like

parliamentary records and statutes of the realm). When reading these primary sources, we have drawn on a number of traditions of textual analysis, in particular ones associated with poststructuralism and conceptual history. We have attempted to model discourses, identify genealogical breaks and establish when and with what meaning new political concepts have entered into usage (Leira 2016), and even combined this with regression analysis (de Carvalho 2016). In our opinion, this approach goes beyond, both in depth and scope, what are often considered standard constructivist analyses of historical phenomena, particularly of 'second-generation' constructivists. However, we see it as resonating fairly well with work by authors in the alleged or self-proclaimed 'third generation' (Leira and de Carvalho 2016b). Thus, we tend to think of our preferred approach to Historical International Relations as one fruitful way forward for constructivist scholarship.<sup>7</sup>

### **<head1>Tactic**

<text\_fullout>Doing Historical International Relations based on primary sources is time-consuming, and has traditionally not been very conducive to a successful career in a discipline which favors broad narratives based on secondary sources. Even so, we would not have been able to do our research in any other way, and firmly believe that others in IR would be well served to delve into the past a bit more often. As we have sought to argue above, given a modicum of sensitivity to source criticism and constructivism's penchant for interpretation, this is not a daunting task. It can be attempted without great cost and can offer great rewards.

Returning to the first section above, it could be argued that historical methods, broadly defined, were our key tactic both in terms of answering the questions we were so set on answering as graduate students and in helping us make our path into the discipline where we, again, were able to carve out the intellectual and institutional space we had so craved in the form of the Historical International Relations Section. Doing detailed historical work, and getting our hands dirty was our way of gaining the respect of peers, and actively engaging in creating panels and then a section became a way to be heard. It should be stressed that methods for us has in this respect been strictly tactical, and not strategic. Very little of what we have come to engage in has been the result of long-term planning. Had it been, we would not have been where we are today. Strategic, long-term career planning in IR in Norway does not lead you to emulate the paths of historical sociologists long gone, but, rather, takes you straight to the highway of super-positivist number crunching, with all its rewards in terms of publications, citations and job security in an environment that has come to value that form of “IR” more and more. It would be counter-factual to speculate on whether we would have been happier doing that. Suffice it to say that none of us regret our strategic ‘blunders’ and that we have relished all the tactical moves necessary in order to redress them.

Ideas, both academic and political, emerge most of the time as we are trying to solve common challenges together. Throughout the narrative we have spun in these pages, having a trusted partner in crime has been crucial. Not only has it been a way of avoiding spending too much time alone looking for someone to talk to, but it has also been a way for both of us to generate projects and ideas we would never have come up with on our own. In light of this, we hope that our experience thus far illustrates how methods fulfill not only scientific, but also social and political needs.<sup>8</sup> Our methods initially allowed us to come up with answers to the

questions we asked, and while these methods were initially somewhat of a hindrance for us to ‘make it’, stubbornly making history also allowed us to force some doors to open and construct an intellectual space where we were able to continue this quest and meet other people sharing our predicament.

While our use (and focus on) historical methods in general, and within a constructivist framework broadly speaking, can be read in hindsight as a tactic for getting a foothold in the discipline (like most scholars, we had to at least seem to say something which no one had said before us), our focus on primary sources has primarily been a tactic for getting answers to our questions. We tend to think of methods as a toolkit, and to answer the questions which interest us, we tend to think that engagement with primary sources offers the best tool. In a sense, our focus on primary sources reflects a mindset, more than a tactic. And perhaps this is where our background in, and interest in, actual academic politics rears its head again – we have always been concerned with the nitty-gritty details of how the world around us comes into being, how the taken for granted becomes taken for granted.

Our focus on sources in general also relates to what we perceive as a problem in many discussions about method, and which we have encountered not only in our undergraduate training, but also in cutting-edge current research, namely the prioritizing of analysis of data over gathering of data. Political scientists have developed and adopted incredibly sophisticated techniques for handling data, both in quantitative and qualitative form, but scant attention has been paid to how data are generated, and what data might mean. We are unequivocally convinced that a better understanding of what comes before coding and analysis will lead to better outputs at the end.

Even more than that, we believe that an engagement with primary sources opens up possibilities for contact across boundaries. One such contact is interdisciplinary, and relates to interaction with historians. While they do tend to view people doing Historical International Relations as amateurs, a joint engagement with primary sources might allow for an acknowledgment that we are amateurs in the sense of pursuing a similar activity for the love of the activity, and not only in the sense of being less skilled (Leira 2015). Furthermore, IR-scholars might unearth sources which historians have not considered significant, and they are likely to consider comparative cases which historians (for reasons of disciplinary nationalism) may not have even reflected upon (see Andersen 2016 for a case at hand). Being conversant with at least some of the relevant primary and secondary sources is, all told, a *sine qua non* for discussion and mutual engagement with historians.

Primary sources offer even more. In some of the same ways as the ‘new materialism’, engagement with primary sources is often read as a ‘concrete’ way of dealing with the stuff of the world. While we would in no way prioritize the material over the discursive, there is a tactical point to be made here; engagement with primary sources may make those skeptical of constructivism more willing to engage, as one is seen to be doing ‘serious’ research. While fellow scholars may question the utility of doing research on e.g. privateering (Leira and de Carvalho 2011), there are always points to be scored for having dealt with primary sources. We have encountered this numerous times in our interactions with IR-scholars of different stripes – by virtue of having handled primary sources, our research somehow gets an extra veneer of serious scholarship. We suspect that this might be related to the current methods-malaise described above; since very few IR-scholars are trained to handle primary sources,

scholars actually doing so are viewed as a kind of Cirque de Soleil – strange people doing strange things, but definitely worthy of some sort of attention.

## **<head1>Conclusion**

<text\_fullout>Our trajectory can be read in at least two ways. As we outlined above, it does make some sense as a story of localized tactics, coincidences and a few strategic moves. Looking back, we are nevertheless reminded of the old Irish joke about the tourist asking a local resident about the directions for Dublin, and getting the answer: “Well, if I were you, I wouldn’t start from here”.

Yet, we started where we started. Reflecting on our trajectory so far, our engagement with historians has also taught us that although IR scholars may not always see our endeavor as ‘proper IR’ (whatever that is), historians *certainly* don’t see what we do as ‘proper History’ (whatever that is), carving out a space for Historical International Relations has made it possible for us to go on about our historical research program – however quaint other IR scholars have found it, and however uninteresting historians have found it – and participate in a scholarly community in which there is interest in this type of research. Reflecting back on developments in the field in light of our own experience, this was not a given. When doing doctoral research, we both felt the need to legitimize our undertakings with IR in the distant past every time we presented our research. In the early 2000s, there was no space for Historical IR proper, as we have argued above. Yet, a few years later there were enough works published that one could start seeing the contours of a trend. That this trend has become a



loose community of interest, we hope, will pave the way for more IR scholars to go down that road, and that traveling it will feel less daunting. As we have sought to argue, historical research may not be the easy way out, but there are no unsurmountable obstacles to undertaking it. Nor is there some magic trick which only trained historians can perform with their primary sources. A dose of source criticism and a willingness to engage (at least unilaterally) with historians is all it takes.

All in all, we were never all that strategic. And maybe this is what forced us to relentlessly be tactical in our moves. Having made the 'wrong' overall moves by not only following constructivist heresy instead of conforming to doctrinal positivism in a country 'holier than Rome', but doing it historically, we had to work to make up for it. Choosing to approach big questions historically was not a strategic move. It didn't help us land us any jobs, nor did it initially help anyone notice us. Yet, having made the choice of producing historical accounts in a largely ahistorical discipline, we had to overcome the difficulties it posed. We hope that parts of this account may help others in a similar situation, give them some ideas, and even inspiration. After all, while making choices which complicate careers, acknowledgment from peers and publications are bad for, well, careers, recognition and publication, they can also, as Brent Steele writes in his chapter, be at least as rewarding – if at times a bit 'tactically' demanding. Once we embarked on the scenic route, we had to get ready for a long and winding road, over the hills and far away. But, as we have tried to convey here, sound tactics can often make up for bad strategy. And in the end, while highways may be fast, they are seldom as enjoyable as the scenic route.

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#### <head1>Notes

<note>1 In principle, thanks for a chapter like this are due to all our previous interlocutors. Tactically speaking, we would nevertheless like to thank specifically Nora Callander, Minda Holm, Kristin M. Haugevik, Nina Græger, Morten S. Andersen and Iver B. Neumann, Harry Gould and Brent Steele. An earlier version of the chapter was presented at the ISA North-East Conference in 2016. We thank all participants for comments and encouragements.

2 At the point where we introduce sausages, we should note that while this entire chapter might seem superficially ripe for a feminist deconstruction, we have both engaged gender issues in our research, and we have actively sought to promote the careers of women doing Historical IR.

3 We treat sources, historical methods and archives in more depth in Leira and de Carvalho (2016a), see also Thies (2002) for an introduction aimed at an IR audience.

4 In the analysis of discourse, texts are treated in a way akin to the way one treats relics – not for narrative content, but for the establishment of conditions of utterance.

5 While we tend to prefer qualitative analysis of the data, once gathered, it can in principle be analyzed using a number of different techniques.

6 There are a number of challenges associated with these digital sources, one is selection bias and unwarranted homogenization of content (leading e.g. to perpetuated Eurocentrism), another is the limitations on machine-coding of texts and yet another is the need for linguistic competence (e.g. knowing that you must search for “forreyn” rather than foreign). These are certainly not unsurmountable, and from our perspective clearly outweighed by the ability for many more researchers to research many more sources.

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- 7 An added benefit of taking questions surrounding sources seriously is that they are relatively easy to teach. Students tend to grasp the difference between different kinds of sources intuitively, and they are easy to simulate through examples and in hands-on exercises. If nothing else, this should sensitize students to the need to consider the origins of sources, and taking questions of scholarly bias seriously. Furthermore, with the current availability of digital primary sources, students can be given assignments and tests actively engaging with such sources.
- 8 An obvious insight to anyone with the slightest interest in the sociology of knowledge and disciplines.