International Relations as a Social Science

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Abstract
In this written version of his inaugural lecture for the Montague Burton chair in IR at the LSE, Iver Neumann takes stock of International Relations understood as a social science. Having paid homage to predecessors, in the first part of the lecture, he detects and regrets a certain unwillingness within the discipline to address the full universe of pertinent cases. Inspecting the toolbox of the discipline, he finds things to be satisfactory where data collection, theory and meta-theory are concerned, but traces a glaring lack of attention to data collection method among qualitative (as opposed to quantitative) scholars. In the lecture’s second part, Neumann draws on Marcel Mauss’s idea that human agency draws on a constellation of social, psychological and physiological sources and on Emile Durkheim’s insistence that a social science has to privilege social sources of agency, without neglecting sources of other kinds. A nutshell review of relevant trends within psychology and evolutionary biology highlights work that competes with the discipline’s own. While insisting, with Durkheim, on the need to privilege social causes, Neumann calls for more work that explores the possible compatibility of new findings within these non-social disciplines and International Relations.

Keywords
IR, evolution, Mauss, psychology, body, performativity

1. John R. Vincent, mentor et praedecessor, in memoriam. Inaugural lecture, LSE, 13 February 2013. I should like to thank the audience for questions, and Morten Skumsrud Andersen, Benjamin de Carvalho, Thomas Hylland Eriksen, Elizabeth Keir, George Lawson, Halvard Leira, Cecilie Basberg Neumann, Andrew Ross, Ole Jacob Sending, Hidemi Suganami, Henrik Thune and Ann Towns for comments on previous drafts. For the delivered version, see http://www.lse.ac.uk/publicEvents/events/2013/02/201302131830vHKT.aspx

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It is customary to begin inaugurals with a word or two of thanks. Customs are not to be taken lightly. I became a social scientist because I was flabbergasted by the fact that most humans, in most places, most of the time, manage to live in relative peace with one another, and wanted to find out how this could be. The key reason for this is, I think, to do with customs; we keep on keeping on, and thank God for that. So let me honour custom and thank you for electing me, and let me thank you again for welcoming me so warmly.

Speaking as a legal alien, I should also like to add a word of thanks on political grounds. I should like to thank you a third time for having taken me into the country and into British IR. Although functionally, the days of the nation state may be counted, the nation state very much remains a social fact. As we may see from the somewhat terse debate about immigration to this country, or indeed to most if not all countries, it is not a foregone conclusion that foreign workers should be warmly welcome. Since Sir Montague Burton refinanced what used to be the Sir Ernest Cassel professorship of International Relations at the LSE in 1936, it has been held by seven people. The first holder, Charles Manning, was a South African, the antepenultimate holder, Fred Halliday, was an Irishman, and my immediate predecessor Barry Buzan holds dual Canadian and British citizenships. Coming as they were from the home country or from former colonies, none of them was an alien. Neither have there been any alien holders of the other IR Montague Burton Professorship, at Oxford, or of the Montague Burton Professorship of Industrial Relations, at Cambridge.2 Filling the chair with an alien must be in the spirit of Sir Montague Burton, who himself immigrated to this country from Kaunas under the name Moishe Osinskiy in 1900. It is also in the historical spirit of intellectual work. In European tradition, as well as in other long-standing intellectual traditions like the one emanating from the Persian world, roaming scientists were the historical rule. I am happy to embody a long-standing intellectual tradition that globalisation has helped bring back at an unprecedented scale.

In the old days, one of the key reasons why scientists travelled was to seek out opposite numbers. Scientists were few and far between, and when they tired of exchanging letters, they had to take their conversations where they could find them. Science was a unitary concept, so these conversations were typically wide-ranging. This is not the situation today. The last couple of centuries saw an explosion in the production of knowledge. In order for individual scholars to keep up, new disciplines have split off from old ones.3 The social sciences emerged out of history, jurisprudence and sundry now half-forgotten disciplines such as social arithmetics. Sheer volume made specialisation a necessity. Still, the ongoing splitting up of academic inquiry has consequences for each


and every discipline, and I do not think we discuss those consequences enough. By the very logic of science, every one of its disciplines may have a certain autonomy, but in order to maintain its membership, it still has to answer to certain common standards. The inaugural is a customary topos for such stock-taking. This is why I have chosen as my topics today how International Relations (IR) is holding up as a social science, and how it relates to other scientific approaches to understanding its subject matter, which is political and social life that plays out in a setting where there is a plurality of polities. I will take for granted that social sciences are sciences and that IR is a social science, and rather dedicate the lecture to two implicitly comparative questions that concern IR’s relative standing within academia. I will begin by looking at some areas in which I think IR still has some way to go in order to come across as a fully-fledged social science. I will end by discussing how IR should relate to other sciences that concern themselves with the same subject matter. I am thinking here first and foremost of psychology and biology.

**IR as a Social Science**

In a piece published in 2001, Buzan and Little noted an ‘assumption that IR is a “backward” social science’ in and out of the discipline, and themselves charged that IR ‘remains curiously insulated from the other social sciences’. By choosing this approach to assessing the discipline, they have set an example for the rest of us. I will single out three characteristics of a social science and ask how we are doing. The way I arrived at these three was pragmatic. Using what I know of politics, sociology and particularly social anthropology, I asked myself where we are coming up a bit short. I follow custom to the letter, and come up with three areas, for British IR traditionally thinks in threes. The first area in which there is ample room for improvement is to do with history and the need to study the full universe of cases. Most cases of polity interaction are historic. A science must cover the full universe of its cases. Therefore, IR would be lost without history. A second thing that must be in place for a social science to be worthy of the name, is a full gamut of tools: data collection, methodology, theory, meta-theory. The third and final characteristic that I want to discuss, concerns the relationship of IR to the non-academic world and the need for some degree of autonomy.

**IR and History: A Full Universe of Cases**

Jacques Lacan famously used the metaphor of the quilting point (point de capiton) when he wanted to discuss the emergence of a discursive field. IR is no exception. At some

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5. They are also a substantive example to follow. Only this year, Buzan and Lawson publish an article where they try to eke IR closer to the other social sciences by highlighting the throes of modernity as one obvious place for any social science, IR included, to start its inquiry from. See Barry Buzan and George Lawson, ‘The Global Transformation: The Nineteenth Century and the Making of Modern International Relations’, *International Studies Quarterly* 57, no. 3 (2013): 620–34.
point, a move was made that tied in some of what had happened before and laid it out as the field’s pre-history. This is not the place to discuss the emergence of IR as a discipline. I refer to the series of well-placed debunkings that have emerged over the last decade. There was the influence of international law. There were the more or less systematised treaties and handbooks on the arts of war and diplomacy. There were the odds and ends of philosophical writings touching on the problems that arose from there being more than one polity in the world. But first and foremost, there was the discipline of history. History was the main originary other of the social science. The academic study of history was, not totally unfairly, seen by early social scientists as a kind of idiographic telling of stories that the nomothetic stories told by social science were supposed to replace. History was what the social sciences tried to limn itself off from. That exercise was ultimately bound to fail, for by finding its identity in opposition to history, the social sciences made history its outside, and outsides are constitutive of the phenomenon that they limn off. It is, therefore, not surprising to find that IR has been roiled by this question for the entire century or so in which it has existed as an institutionalised discipline.

As an example, let me turn to the relationship between the first holder of the LSE Montague Burton chair, Charles Manning, and the man who was supposed to be his right hand, his amanuensis, Martin Wight. Manning was set on professionalising IR as a social discipline. He sometimes referred to it as global quasi-sociology, and spent considerable efforts at making the discipline compatible with other social disciplines. Wight, a historian by training and a Christian by conviction, wanted IR to set up camp in the humanities. There are no traces of social theory in Martin Wight’s work. Instead of attempting creative leaps that could bring forward new insights, he remained content with collecting facts and categorising facts and, instead of analysing them with a view to their specific area of validity, rather speculating about what their world-historical meaning might be. Martin Wight’s principal follower and posthumous editor, Hedley Bull, said it best: Wight’s was a project of historiosophy. Small wonder that Manning was a bit miffed. Embracing historiosophy is reactionary in the literal sense of the term, for treating historical matters as part of an immanent Plan – religious, evolutionary or otherwise – is a return to how we thought about social matters in general before the emergence of the social sciences. The very earliest social scientists, people like Comte, Marx and Spencer, were not fully successful in tearing themselves away from historiosophy, and that detracts from the value of their working methods for us today. Durkheim and Weber, on the other hand, were both historical thinkers, and matters historical dominate their work. They remain the acts to follow. Key early IR scholars were acutely aware of this, and behaved

accordingly. E.H. Carr was decisively influenced by German social science, mainly through the towering figure of early sociology of knowledge, Karl Mannheim.\(^\text{10}\) Morgenthau was a Weberian.\(^\text{11}\)

Social sciences must take to heart the Nietzschean and Collingwoodian point that we always approach history in terms of the present.\(^\text{12}\) In the larger picture, however, the main threat to IR’s standing as a social science is probably not the misuse of history, but the increasing amnesia that seems to be taking hold. A number of our colleagues, and particularly our colleagues across the water, seem to agree with Henry Ford, and treat history as if it were mostly bunk. That way lies perdition for IR as a social science.

The reason why we would be lost without history is very simple. Any science worthy of the name must aim to cover the full universe of cases that are pertinent to it. The subject matter of IR is political and social life that plays out in a setting where there is a plurality of polities. More specifically, the subject matter of IR is sovereign and suzerain relations between polities as they existed in the past, as they exist in the present and as they have been and are imagined to exist. It follows that IR has three, and only three, sets of cases to pursue, namely the logics and effects of extant relations between polities, historical relations between polities and imagined relations between polities.\(^\text{13}\) I note, ruefully, that we are not doing too well even on extant relations. In a recent article, Iain Johnston makes the point that, up until 1991, by largely not studying China’s foreign policy we left 20 per cent of the great powers out of our analyses. In the jargon of US political science, he notes that the neglect of East Asia leads to ‘omitted-variable bias’.\(^\text{14}\)

As an example, he points out that systematic study of the Chinese tribute system and the South-East Asian mandala system would enrich our understanding of hegemony.

I find it impossible to quarrel with that. As already highlighted by Barry Buzan and Richard Little in a book from 2001, the upshot of our neglect of historical cases such as these is that we have tens of thousands of years of history to cover.\(^\text{15}\) The discipline congealed around the study of the European states system. Much, but far from enough, has

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12. This is not to say that we cannot learn from those who attempt to approach the past in terms of the past, such as the Cambridge School of historians, only that we, as social scientists, do not have the luxury of making believe that history may be approached from anywhere else than the present.
13. To this relational approach may be added an actor-based one; even if we limit IR to the study of the states system, we come up a bit short, for we consistently concentrate on a handful of great powers, to the detriment of the vast majority of states, which are small or medium-sized; see Iver B. Neumann and Sieglinde Gstähl, ‘Introduction: Lilliputians in Gulliver’s World? Small States in International Relations’, in *Small States in International Relations: A Reader*, eds Christine Ingebritsen, Iver B. Neumann, Sieglinde Gstähl and Jessica Beyer (Seattle, WA: University of Washington Press, 2006), 3–36.
been done to live down this Euro-centrism by studying other systems. Having criticised Wight, I should also make a point of acknowledging his pioneering insight to the study of states systems.\textsuperscript{16} There is more than enough left that has hardly been touched, though. We have, for example, the earliest sedentary polities, as we know them from Mesopotamia, Egypt, China and Meso-America. As observed by Cambridge archaeologist Colin Renfrew, these polities are what he terms peer polities, by which he meant that it is not a question of one polity emerging singularly, but of an entire cluster emerging together.\textsuperscript{17} Key examples include the cities of Sumer and the Greek \textit{poleis}, but we find the same phenomenon on this isle, with the Welsh polities from the post-Roman period, and with the kingdoms – or, to keep it in archaeological parlance, chiefdoms – of the Anglo-Saxon period.\textsuperscript{18}

These relations have hardly been studied by IR scholars. That is a shame. Archaeological literature has, until recently at least, thought of primary state formation as a process unfolding from the inside and out. If, alternatively, it turns out to have been first and foremost a relational process, then we should have been the ones to establish that. At the very least, once archaeologists suggested that the very emergence of polities is a relational phenomenon, we should have rushed to their aid. Even a cursory glance at the material suggests that IR theories should have a lot to offer ongoing debates.\textsuperscript{19} IR has so far assumed that polities come first, and that relations between them arise later. If we follow Renfrew, however, this is not the case. Polities arise as a result of relations. Relations are constitutive of polities from the very beginning. This means that IR’s subject matter is foundational for political life in general, rather than being derivative of life inside different polities, as has been previously assumed.\textsuperscript{20} In the beginning, there was not one single pristine polity in Mesopotamia, one in China, one in the Anders, etc. In the beginning, there were ‘international’ relations. It also puts a rather different spin on

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\item 16. Martin Wight, \textit{Systems of States} (Leicester: Leicester University Press, 1977); an exemplary example of how areas should be opened to IR is Raymond Cohen and Richard Westbrook, eds, \textit{Amarna Diplomacy: The Beginnings of International Relations} (Baltimore, MD: Johns Hopkins University Press, 2000).
\item 20. To Buzan and Little, IR begins with sedentarisation, for it is only then that social multiplicity becomes political multiplicity, as Justin Rosenberg puts it. See Buzan and Little, \textit{International Systems in World History}; and Justin Rosenberg, ‘Basic Problems in the Theory of Uneven and Combined Development. Part II: Unevenness and Political Multiplicity’, \textit{Cambridge Review of International Affairs} 23, no. 1 (2010): 165–89 (175). I am not certain that I agree. There was certainly multiplicity of polities among ancient hunters and gatherers, for they famously lived in bands. We simply do not know enough about the interaction patterns of these polities to know much about how differentiated they were, but evidence from pre-contact Australia
\end{itemize}
Durkheim’s reading of how polities differentiate power, for it suggests that a relational, rather than a functional, logic is what drives that differentiation. To say it with Abbott, from the very beginning, polities are things of boundaries. That is good news for those of us who want relational sociology to play a larger role within the discipline.21

The same point may be made about non-sedentary polities. More than two centuries ago, Hegel pronounced any state worthy of the name had to be sedentary. As a result, social sciences have neglected the study of non-sedentary polities. That is a huge omission, for it rules out the study of relations between polities older than some nine thousand years, it rules out institutionalised systems in places like the African savannah and the North American plains, and it rules out an almost three thousand year long, uninterrupted steppe tradition of nomadic empires in Eurasia. These systems are interesting in their own right. They are, furthermore, crucial to the emergence of large-scale sedentary polities, which tended to grow out of relations between sedentaries and nomads. I mentioned my interest in this topic to my 6-year-old son over Christmas. He had watched the 1998 Disney film Mulan repeatedly, and felt empowered to chime in. ‘Steppe people are strong, but dumb’, he volunteered. Judging from the lack of interest shown by IR, I would say that our sin of omission in not studying steppe polities condones my son’s judgement, for how are we to get rid of clearly wrong stereotypes if we do not produce new knowledge with which to fight them? Studying these historical relations more thoroughly would also give us a better handle on present conflicts involving nomads and sedentaries, like the ones in Darfur and Mali. Following the imperative to study the full set of cases available to us also has the added, and extremely overdue, advantage that it will live down the discipline’s Euro-centrism, which is scientifically untenable.22

A Full Scientific Tool Kit

If history gives us something to think about, there is also the question of what we need to think with. To take a leaf from the debate about research programmes, a scientific discipline worthy of the name must have at its disposal tools for data collection, ways of ordering the data, ways of discussing the relationship between the data and the wider world, and debates about what that world consists of.23 I would argue that the area of philosophy of science is the area in which IR has made most headway over the last quarter of

democratizes that such interactions may evolve diplomatic practices. See Ragnar Numelin, The Beginnings of Diplomacy (Oxford: Oxford University Press, 1950). This suggests that ancient band relations, too, could be more or less institutionalised. We should not make the Hegelian and Marxist mistake of simply treating non-sedentary polities as undifferentiated, be that in their relations or otherwise; see Iver B. Neumann and Einar Wigen, ‘The Eurasian Steppe in International Relations: An Understudied Political Tradition’, Journal of International Relations and Development 16, no. 3 (2013): 311–30.


22. It is, of course, also politically and morally untenable, but that is not our topic today.

a century. When I came up, we discussed this in terms of method and theory. As I was writing my doctoral thesis, two publications emerged that raised the level of the debate overnight. In Michaelmas of 1990, there emerged a special issue of *ISQ*. The following year, Martin Hollis and Steve Smith published a book called *Explaining and Understanding in International Relations*. Since then, we have been speaking in terms of methodology, epistemology and ontology, as do the other social sciences. We have also evolved a philosophically informed IR literature on meta-theory which is unprecedented, and which has done a lot to consolidate IR as a social science. Meta-theory has had an uneven reception within the discipline, but that remains the situation throughout the social sciences. IR also has a well-developed set of theories. I think we are paying too little attention to the founders of the social sciences; Marx, Weber, Durkheim. Their readings of the phenomenon of the state is still the best starting point (but definitely not the end point) of inquiries into large-scale political processes like the ones we study. I also think that it is wrong that we, as a discipline, tend to rate most highly the books that deliver abstract theoretical arguments and nothing else. Social scientists should lean on philosophy, but they are not primarily philosophers. Our niche is between history, which tends to fetishise empirics, and philosophy, which tends to fetishise metaphysics. For me, it follows logically that the social scientific ideal is the sustained, theory-led empirical argument. This is indeed the classical ideal, as seen in Marx’s dissection of 19th-century European capitalism, Weber’s analysis of the protestant work ethic or Durkheim’s reading of religion in social life. It remains if not *the*, than at least *an* ideal in the other social sciences. Perhaps this is just me being old school. Be that as it may, the present challenge to IR’s ways of inquiry lies not primarily with our writing genres, our theories or our meta-theories.

It is, rather, to do with data collection and the ordering of data. Any science worthy of the name has a running debate about methods. As Weber put it, ‘The dilettante differs from the expert … only in that he lacks a firm and reliable work procedure’. Seventy years after Weber said that, Jean-Francois Lyotard still located the key defining trait of science in the work procedure, more specifically in its transparence, by highlighting the importance of referencing. The point is an old one, and it has been made repeatedly, but it seems that the importance of method must be insisted upon, again and again. In IR, the discipline’s quantitative practitioners are passionate about the problems surrounding data programming, in such a degree that one sometimes wonders if counting is not being substituted for thinking, but at least the quants do engage in a debate about methods. Those of us who mostly do qualitative stuff, however, must be severely faulted for

having largely neglected methods. Let me take a concrete example. The year 1995 saw an important institutionalising move in IR, the founding of the *European Journal of International Relations*. The ‘Aim and Scope’ section, which kept on being reprinted, noted that the purpose was to ‘foster an awareness of methodological and epistemological questions’. In the preface to the first issue, founding editor Walter Carlsnaes laid down that manuscripts had to pay proper heed to epistemology, method, concepts and normative issues.\(^{29}\) I would say that, on the level of printed articles, it has. What it has not done, however, is to foster a debate about methods. To date, 73 issues have appeared. I count one – 1 – article whose main concern is methods.\(^{30}\) That is something like 0.2 per cent of the total. To cap it all, that article was published under the unique caption ‘Discussion and Debate’. It remains the second most quoted article in the history of the journal. According to Google Scholar, as of February 2013, it had been quoted 441 times. Some might argue that this is because it discusses a particularly fashionable method (discourse analysis), but I think the main point is that we have here a strong indicator that articles on method are needed by the discipline’s practitioners, but that the need is not heeded by journal editors. When did we see a methods article in the *Review of International Studies* or in *International Organization?* *International Studies Quarterly* now publishes whopper issues, and yet I look in vain for an article on method.\(^{31}\) Our own *Millennium*, always the avant-garde journal, is as so often a partial exception – it is, among other things, home to an ongoing debate about the pros and cons of drawing on ethnography in IR – but all in all, method remains something that is at best touched upon in the opening pages of monographs. Perhaps an incoming team of *Millennium* editors might do something about this by making methods the topic of a special issue?

### Integrity of Inquiry

The third sense in which IR comes up a bit short as a social science concerns the strong influence of political concerns that we allow over our choice of subject matter. We tend to concentrate on phenomena that are politically important and agents that are politically powerful, to the detriment of studying stuff that would have added to our general knowledge of the subject matter that we call our own. Particularly for a reflexive science, there is always a balance to be struck between the applied and the basic, and taken as a whole, I think we tend a bit too much towards the applied.\(^{32}\)

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Social sciences are social not only in the sense that their subject matter is social relations, but also in the sense that their knowledge production rests in a wider social web of relations. There is a burgeoning move within the discipline, spurred on by developments in Science Studies and particularly within Agent-Network Theory, to make the relationship between IR knowledge production and wider social relations an object of empirical inquiry. This has the added advantage of calling attention to the need for science to have a certain degree of integrity of inquiry. I say a certain integrity, for there can be no such thing as absolute integrity. Writing emerged as a tool for registering work, which is to say that it began life as a surveillance strategy. Geography emerged as a handmaiden for ancient Greek military officers who needed knowledge of actual and prospective theatres of war. Ancient Greeks and the stray Ibn Khaldouns aside, the beginnings of the social sciences are to be found in 16th- and 17th-century European efforts to acquire knowledge about social worlds so that they could be turned into objects that might be ruled more efficiently and more effectively. The adviser to the King is a predecessor of the IR scholar. Furthermore, institutionalised IR owes a lot to people like David Davies and Lord Cecil, who were equally keen on harnessing the production of knowledge to a political project. Theirs was peace. To this day, the LSE Department hosts an annual David Davies of Llandinam Research (Dinam) Fellow. Many IR scholars, and I count myself among them, take time off to work for state institutions or international organisations.

All this is fine. It is also fine that IR has a sizeable sub-community of defence intellectuals, whose work is geared specifically towards solving problems for state and non-state actors, and that we do a lot of applied work regarding international political economy, development and the like. It is not only fine, but positively laudable, that IR scholars partake in the public debate about running affairs. After all, it is from the community at large that we take the resources necessary to do what we do, and so it is only fair that we give some of the knowledge we accumulate back to a wider public.33 The renewed emphasis in British academia on having an impact of public debate is a good thing.

It is a good thing, with the caveat that it is taken on face value and not simply a way of press ganging scholars into being at the beck and call of bureaucrats in search of some extra free labour. What would not be fine, were if we should lose sight of the fact that these applied pursuits are spin-offs of our scientific work rather than core tasks in their own right. To do social science is not to serve a certain political programme, but to model the social and highlight inconsistencies and the costs of doing stuff in this fashion rather than that.34 Rob Walker issued a related challenge to the discipline some twenty years ago, when he charged that ‘[t]heories of international relations are more interesting as aspects of contemporary world politics that need to be explained than as explanations of contemporary world politics’.35 To put it in its starkest form, if IR is only a handmaiden

of social forces, then it is not a social science. In light of this, if we ask why IR keeps on studying the practices of great powers, and pay less heed to small and medium-sized powers, why it is that IR uses infinitely less energy studying historical cases than studying present-day cases, then the answer usually given, that it is important to be policy-relevant, comes across as shallow. Being policy-relevant and being the handmaiden of specific forces, be that states, NGOs or some imagined collective, amounts to the same thing, namely to produce instrumental knowledge. With the government playing an ever greater role in the allocation of research money, and with scholars within universities being under ever more pressure to apply for external funding, we must expect there to be ever more instrumentally produced knowledge. The risk is that we end up drowning in papers, reports and articles that are of no interest to anybody a year after they were published.

IR used to have a competitor that may serve as an example of what happens when we stop asking overall questions and start concentrating on the minutia of government life. In 1965, David Easton published two functionalist books where he argued that political science should bracket the question of what a state was, and rather focus on what already existing institutions of government did in response to external demands.36 This was clearly a plea for policy relevance, for the underlying concern was how political scientists could help government officials perform better. It was also a blatantly reductionist programme. Let me highlight two reasons why. First, the area of validity of the framework proposed by Easton turned out to be democratic systems. If the relationship between state and society and the competition between parts of government follow other logics than the democratic one, Easton’s approach is of very limited value. I found this out to my cost when I started my career and tried to take standard political science models to the study of the Soviet Union. It did not work. The second reason why Easton’s influence on political science was so damaging was his exogenising of the state. Easton reduced to a given assumption what should have been the very focus of political inquiry, namely how there may be such a thing as a state, or a polity for that matter, in the first place. American and global political science was, seemingly indelibly, marked by Easton’s reductionism. In the case of the US, Easton basically moved social inquiry back to what it had looked like before the arrival of European exile intellectuals in the 1930s for, as John Gunnell has demonstrated, American political discourse traditionally did not know the concept of the state.37 In Europe, the recoils from Easton’s salvoes served to play down the significance of state theory. As a result, the important questions of what makes states and relations between states possible in the first place was backgrounded by intellectually less but bureaucratically more interesting questions about how specific institutions and humans relate to one another inside a political system that is simply treated as a given. Overall, this is a historical loss for the social sciences, but it has the advantage

that the majority of political science approaches do not pose a challenge to IR. In order to find our main competitors, we must look outside the social sciences.38

The Competition

Let me turn now to the competition. To repeat a point, IR is the social science that specialises in political and social life that plays out in settings where there is a plurality of polities. Ours is a discipline about group conflicts and the social preconditions for and effects of alterity. As a social science, we follow Durkheim’s dictum that social phenomena should be explained in terms of other social phenomena.39 To Durkheim himself, this meant that we should, first, do a functional analysis, that is, ask who fulfilled which tasks in what way. We should then do a causal analysis. As Virgil put it in the Georgics, book two, verse 490: rerum cognoscere causas. To Weber, it meant that we should understand and explain. For this textual moment, let us not get bogged down by ongoing, and I would think perpetual, debates about whether we should stress function maintenance or power struggles, order or justice, whether we should foreground a problem-solving or a critical mode of analysis, and so on. Whatever our proclivities regarding all that, as social scientists we take a particular interest in situations where ensuing relations are dense enough to be patterned, for this means that there are social institutions to study, and these institutions may be compared and certain general propositions about them made.

Social scientists are not the only people to attempt scientific analysis of what we consider our subject matter. As pointed out already in 1934 by one of the greatest social scientists the world has seen, Marcel Mauss, if we want to understand human action, we need to take into consideration three systems: the psychological, the physiological and the social.40 Psychology and biology are real competitors for IR, inasmuch as they share our key interest in group conflict and alterity.

Psychology

Freud was deeply interested in the logic of war, and he once made an attempt at psycho-analysing an entire nation, so psychological interest in international relations goes back.

38. As a double-hatted social scientist with a PhD in social anthropology, I am acutely aware that much more could be said about the relationship to other social sciences and also to a partly social science like geography, but I leave that to one side and refer the interested to Iver B. Neumann, At Home with the Diplomats: Inside a European Foreign Ministry (Ithaca, NY: Cornell University Press, 2012). As Halvard Leira pointed out to me, geography, in the guise of geopolitics, also confronts us with non-social readings; for a discussion see Stefano Guzzini, ed., The Return of Geopolitics in Europe? Social Mechanisms and Foreign Policy Identity Crises (Cambridge: Cambridge University Press, 2012).


40. Note how many present-day evolutionists start from a similar premise, namely that ‘human preferences and beliefs are the product of a dynamic whereby genes affect cultural evolution and culture affects genetic evolution, the two being tightly intertwined in the evolution of our species’. See Samuel Bowles and Herbert Gintis, A Cooperative Species. Human Reciprocity and Its Evolution (Princeton, NJ: Princeton University Press, 2011), 14.
However, psychology only took on urgency as a competitor when Kurt Lewin brought the insights of Gestalt psychology to social concerns and became decisive for the emergence of social psychology while doing so. One of his followers, Leon Festinger, published a book on cognitive dissonance in 1957 that spoke directly to IR concerns about how foreign policy decision-making processes proceed. The key point was that, rather than considering specific factors on their merits, humans are drawn towards understanding the world as a whole, and so they will tend to read out stuff that does not fit their preconditions. Stanley Millgram’s classic experiments into the importance of group pressure for action also spoke directly to our concerns. The key point here was that people will set aside interests that are literally vitally important in order to fit in. Findings like these speak directly to IR concerns about authority, group cohesion and group think. Traditionally, however, the value of cognitive psychology to IR and its concerns with alterity has been seriously impaired by the blatant ethnocentrism of the discipline. As late as in the mid-2000s, 96 per cent of all test subjects have been Westerners, and most were Americans. To the socially minded, it is quite obvious that these tests do not measure how the brain works. They measure how brains which have been socially shaped in specifically Western settings work. One would have thought that the series of anthropological works on how cognition varies culturally would have made psychologists question their universalism, but this has really only happened recently. In 2010 Joseph Henrich, Steven Heine and Ara Norenzayan published a paper summing up what we know about cultural differences in cognition and adding some breakthrough material on the cultural spread in how people think about reciprocity. Reciprocity is a key issue for IR, for it is the key precondition for collaborative action, and collaborative action is the key factor for establishing and kind of legitimate social order. As I know from my own studies on diplomacy, the degree and ways in which reciprocity is acknowledged by the parties is the keystone to establishing and maintaining formal relations between polities. As witnessed, amongst other places, in the series of Chinese, Byzantine and Ottoman refusals to acknowledge reciprocal relationships with other polities, it literally took centuries to


44. There are examples of socially informed cognitive psychology from around the same period, for example on the classic Mueller-Lyer illusion, see Marshall H. Segall, Donald T. Campbell and Melville J. Herskovits, ‘Cultural Differences in the Perception of Geometric Illusions’, Science, New Series 139, no. 3556 (1963): 769–71.

45. Henrich et al., ‘The Weirdest People in the World?’
establish even the thinnest form of reciprocity. It is not surprising that social problems in establishing reciprocity were reinforced by psychological factors, but unfounded rationalist universalist presuppositions have, so far, kept most psychologists and IR scholars from scouting this promising landscape.

When we embark on this work, we are not without predecessors, though. One of the more sympathetic faces of the so-called behavioral revolution in IR in the 1960s and 1970s was that insights from psychology appeared in IR scholarship. One early example was Alexander and Juliette George’s psychological study of key politicians. Such profiles are still drawn within political psychology, but they have disappeared from IR. As long as such studies disappear in between the lobe of their subjects and out of sight, this is a good thing, but the personality of leaders matters to politics, and there is no ipso facto reason why psychology should not be able to help us understand this factor better. The Georges’ study of the interplay of social and psychological factors in decision-making proved to be path-breaking, however, as did Jervis’s work on perception. There remain points of contention between IR understood as a social science and psychology. Jervis’s concept of misperception is not a social one – if a group of humans consistently act like X in situations of type Y with results of type Z, the point is that we are faced with a social type, not with individuals who ‘misperceive’. To my aforementioned son, Brits drive on the wrong side of the road. To the social scientists, faced with a dichotomous and largely arbitrary decision long ago, Brits and Swedes opted for the opposite of the rest of Europe, and, largely for identity reasons, Britain is taking its own sweet time coming around to the regional standard. Of course individuals have motivations, of course they matter to outcomes, and of course personal motivations are fair game for psychologists and psychoanalysts, but at least given the present state of psychological knowledge, IR should be wary of staking analysis on motivations alone. There are dangers on either side of that road. If, on the one hand, we look for specific motivations, as social scientists we may only study them indirectly, via their social effects. There is simply too much individual stuff going on between the social impetus of motivation to the formation of motivation itself, and too much stuff between the presumed individual motivation and the observable social intention that results from it, for this to be an obvious terrain for us.

47. Note that rationalists are not the only ones to blame. Henrich, who is a trained anthropologist, was denied a job by his anthropologist peers in the US for reasons of political correctness; see Ethan Watters, ‘We Aren’t the World’, *The Pacific Standard*, 26 February 2013, p. 3, http://www.psmag.com/magazines/pacific-standard-cover-story/joe-henrich-weird-ultimatum-game-shaking-up-psychology-economics-53135/ (accessed 1 March 2013).
To pick but one example, avenging the father may have been one motivation among many for George W. Bush invading Iraq in 2004, but how to disentangle that? Turning to the ditch on the other side of the road, if we look for a general motivation, we may go with certain psychoanalysts and stake our explanations on the foundation of desire. But desire, like any foundation, is a catchall, whose existence is in itself determining of action and so makes specific social inquiry superfluous. So, either way, as usually thought of, motivation is not the ticket if we want to remain true to our calling as specifically social scientists.51

There is, thankfully, an alternative available route open to social scientists to include psychology in our analyses. It goes through the study of emotions, which are a general human phenomenon and readily observable in social interaction.52 For Damasio, an emotion is a somatic marker that ‘forces attention on the negative outcome to which a given action may lead and functions as an automated alarm signal, which says: Beware of danger ahead if you choose the option that leads to this outcome’.53 This insight (which, I, for one, immediately recognise as true; I can even recall a number of times when I chose as I did exactly to enjoy that rash of danger) is crucial, for its corollary is that emotion is also the helper of rationality – it focuses the brain and forces you to concentrate. Work on emotions, be that as a factor in decision-making or in identity-formation, is clearly on the up.54 Here, I think, we have a nice example of synergy between disciplines. There is also a rapidly growing engagement with the neurosciences in IR that take a number of forms, from a largely behavioural one to one based on Science Studies.55

To sum up this section, we should maintain the dialogue with psychology, and learn from it, but we should do that by being true to the realm that we, as social scientists, have

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51. As pointed out to me by Andrew Ross, one obvious way out of this quandary is to treat motivation not as a private, but as a physiological and/or social category.

52. The standard categorisation is that we have six basic feelings: pain, pleasure, anger, fear, surprise, disgust, and seven social emotions: shame, love, guilt, embarrassment, pride, envy, jealousy.


chosen to privilege, namely the social. *Pace* Durkheim, social facts should be explained by social factors.\(^\text{56}\) The relative importance of the psychological and the social systems is the object of lively debate, and that is how it should be. There is another contender that is of much more pressing interest. I think a major debate between biology and IR is brewing.

**Biology**

Humans are meaning-producing animals. We may not be unique in this respect. Chimpanzees certainly learn, form alliances and make worlds, and the trend in animal research, not only on primates, but also on birds and insects, is in the direction of updating their cognitive skills.\(^\text{57}\) Nonetheless, the social scientist’s wager is that socially produced meaning – that in the form of culturally specific interest maximisation, republicanism, apotheosis or whatever – is so strong among humans as to be the decisive force in human life. In that, we are unique until further notice, and in this sense, Dilthey’s work was a precondition for the emergence of the social sciences.

I began this lecture by invoking Marcel Mauss and his suggestion that human action has its sources in three different systems, the physiological, the psychological and the social. I used Mauss’s insight to raise the question of how they should meet in scholarly conversations.

Mauss himself invoked these systems because he had other fish to fry. He wanted to make what he called body techniques and what we today call embodied practices an object not only of physiological and psychological, but also of social study. For Mauss, the three systems meet in the agentic body.\(^\text{58}\) Mauss succeeded in making the body an object of study for the social sciences, but he also met with opposition. Half a century ago, Jacques Derrida suggested that we bracket the material or, as he formulated it, put it under erasure (*sous rasure*). Since then, it has become a commonplace to put the physical body under erasure, and to treat it simply in its performative aspect. Judith Butler has probably delivered the fullest justification for such an approach.\(^\text{59}\) This has been a productive move. Inspired by Butler, IR scholars like Diana Saco and Cynthia Weber at long last began to theorise the body’s importance to IR.\(^\text{60}\) A problem remains, though. In IR theory, the physical body remains under erasure. I would argue that, as a discipline, it is just about time to excavate it. Let me use an example close to hand, namely myself. What you see before you is a performative body, me (not mine; me). It is a body that would not have existed if it had not been for the physiological fact that I had a necrokidney implanted five and a half years ago. According to Butler, performativity is ‘that aspect of

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discourse that has the capacity to produce what it names’.

If discourse produces the social fact that is the body, it does not produce the physical body from scratch. The physical body remains an ontic precondition for the performing body to exist. The necro-kidney was an actant that made things happen, independently of performativity. You piss, you live. The physical body and the performing body are not identical phenomena, they are both relevant for our social existence, and we cannot go on putting the physical body – and, by extension, biology and psychology – under erasure forever. A theory of the body that only takes into consideration the social system, and leaves out the importance of the physiological and psychological systems for the human condition, is by definition incomplete. And what goes for the body goes for materiality in general. We need a synthesis of existing views that would open up what seems to have become a rather locked debate here. Consider a recent piece by David Campbell, a scholar who did much the same for the study of security and identity within IR as did Butler within feminist studies (and who was, if I may add, a key inspiration for my own work on identity). Campbell attacks Wendt, I would say rightly, for arguing that we should bracket discursive understandings of what has happened when we find a material phenomenon such as a corpse with a bullet through the head. The reason Campbell gives for this is that the issue is not what happened, but how it happened, so that, ‘the constitution of the event and its elements is a product of its discursive condition of emergence’. I would agree, but it does not follow that the corpse is of no interest to us. As social scientists, we are not only interested in specific phenomena, but also in the human condition, and so we cannot simply ignore the physical corpse. As social scientists we are supposed to privilege the social system, but we are not supposed to take the division of scientific labour to the extreme of simply forgetting about other ways of producing knowledge about our subject matter. An exclusively social discussion about the body is doomed to remain exactly disembodied. The physical body must be brought back in. The question is how.

On the opposite extreme from Butler, we find those who contest the view that meaning-production is key to human life and wanted to privilege biology. Nietzsche makes the points in his Genealogy of Morals that nothing has only one origin, and that there is usually among them a dirty one – a pudenda origo. In the case of the social sciences, the dirty origin is Herbert Spencer. Spencer was a key influence on early social science. He was also a

62. The argument is, of course a long one. A kidney transplant is a discursive event and, as Foucault convincingly demonstrates, the entire western medical discourse produces phenomena. And yet, we need that material organ for the damn thing to work. To put it differently, the point about the menu and the courses is that the relationship between them is indeterminate. It would be a non sequitur to argue that you can eat the menu. In the French tradition, of which Butler is a part through Foucault, the starting point of this debate is the leading philosopher of the Third Republic, Charles Renouvier, who argued that any phenomenon has an inside (in this case, the physical body) and an outside (in this case, the performing body), see Susan Stedman Jones, Durkheim Reconsidered (Cambridge: Polity Press, 2001). Butler privileges the outside too much.
64. Ibid., 236.
biology-privileging social Darwinist, and here we have the reason why he has been largely read out of the canon and shorn Britain of a founder of the social sciences into the bargain. There is a lesson here about what happens to those who are loud, confident and wrong.

It is neither theoretically nor empirically inevitable that an interest in biology should lead to social Darwinism. At the time of Spencer, another key natural scientist in Britain was Count Peter Kropotkin. His major work, *Mutual Aid*, draws on the knowledge about the natural world of the day to demonstrate how the human species has evolved not by way of conflict, but cooperation.\(^{65}\) To this day, we find among natural scientists debates that are eerily familiar to social scientists: is human cooperation zero-sum or positive-sum? How important is the role of environmental factors?

Over the last decades, people like Edward Wilson, Steven Pinker and Richard Dawkins have put these concerns back on the map under the names of sociobiology and other evolutionary approaches to culture and social life.\(^ {66}\) Of particular interest to us are the approaches that emerged out of biology only to merge with psychology and present themselves as evolutionary psychology. The main reason for the merger is probably to be found in developments internal to science; the prominence achieved by the neurosciences and also the advances in genetics, fields that invite cooperation between different stripes of natural scientists and also experimental psychologists, seem to be crucial.\(^ {67}\)

There is no *ipso facto* reason why the social sciences and IR should not partake in this conversation. On the contrary, it would be unprofessional simply to ignore other attempts at understanding the issues than those that we call our own.\(^ {68}\) The question is how we do it. Do we, *contra* Spencer, confirm ourselves as social scientists by insisting on explaining social stuff with other social stuff, or do we, *pro* Spencer, simply argue that biology determines social behaviour?

Once again, I think a nutshell illustration is in order: 2004 was the year when the major biological theme of evolution arrived in IR in force, in the guise of evolutionary psychology.\(^ {69}\) Dom Johnson published *Over-Confidence and War*. Leaning on the likes of Festinger and Jervis, Johnson demonstrated how predictions of loss and victory depend, among other things, on calculations about relative strength in the present. A recent article in *International Security* sharpened the argument into a fully-fledged theory of war.\(^ {70}\) There is no doubt that these are social analyses; the explanatory work is done by comparing social actions in a specific kind of situation and hitching it to an

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67. For a particularly stimulating attempt at doing so-called third culture analysis of the human condition (that is, embracing what C.P. Snow famously named the two cultures, the natural sciences and the humanities), see George Lakoff and Mark Johnsen, *Methaphors We Live By* (Chicago: University of Chicago Press, 1980).
68. I would argue that this holds on the level of disciplines, but not on the level of individual scholars. The biological, psychological and social systems conspire to endow most of us with a capacity that is simply too small to cover everything.
argument that in situation X, we are likely to get outcome Y, whereas in situation A, we are likely to get outcome B. Johnson’s argument is unabashedly functionalist, and for that he may (and I think, should) be faulted, but there have been debates about the pros and cons of functionalism in the social sciences from the very beginning. Those debates are an integral part of what we do. I see no intellectual reason why Johnson’s functionalism should mean that his approach is not legitimate from a social science perspective.

Compare this to another book published in 2004, Bradley Thayer’s *Darwin and International Relations*. On its very first page, we read that ‘The progress of biological science is nothing short of revolutionary. It is as important for understanding human behavior as the great discoveries of Newton or Einstein are for the physical world’. This is already a non sequitur; biology may or may not revolutionise our knowledge of human nature, and this may lead to a reassessment of biology’s importance for behaviour, but that prospect is very far from being a necessity. For all we know by now, ongoing biological research, in the form of epigenetics or heritable changes in gene expression, may not upgrade the importance of our biological hard-wiring at all, but may, on the contrary, confirm the importance of social factors. A key moment in epigenetics came in 1999, when a group of biologists performed an experiment on mice, which demonstrated that administering folic acid to a mother mouse changed the colour of her fur. That was unremarkable. That she proceeded to pass on the new colour to her young certainly was remarkable, however, because it meant that the folic acid had modified her genes. Subsequent research has demonstrated similar effects in humans. The suspicions raised by observations that two of our genes seem to have evolved from viruses, have firmed into an increasing body of knowledge, which suggests that social factors are indeed inheritable. Lamarckism is back. Get ready for a new round of the never-ending nature/nurture debate. We already find a similar debate within evolution-oriented neurosciences, where a number of culturally oriented scholars have documented cultural variations in cognition. Where that debate will lead we do not know, and that insight is enough to conclude that Thayer is wrong when, by what may only be termed a sleight of hand, he

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73. Ibid., 1.

74. I should like to thank Dr Med. Monica Cheng for help with epigenetics.


argues that ‘evolutionary theory provides a scientific foundation for realism and rational choice’.77 Spencer and Thayer aside, that is an empirical question.

The key point in our connection comes, however, when Thayer explicitly attacks Durkheim’s position that social facts should be explained in terms of other social facts.78 Thayer explicitly leaves behind the social as explaining here. One thing is to be interested in the interplay between psychological and biological factors in order to understand the social, as is Johnson. It is another thing altogether simply to bracket the social and seek the explanation of social outcomes in biology, as does Thayer. That, I would argue, is untenable, for it leaves out a logically necessary link for our understanding of the constitution of the social, namely the social itself.79

One would have thought that a student of IR would not be blind either to this, or to the political results that have previously accrued from letting biology overshadow the social in our understanding of human action. The most recent addition to the huge literature about this is John Hobson’s recent book on racism in European thinking on international relations.80 But no. Thayer is blind to all this. In a recent piece, he states the following:

Whether an ageing Europe is able to integrate its Muslim populations and re-assert heretofore dominant political principles, economic practices and cultural beliefs or Europe’s Muslims integrate indigenous Europeans into the dhimmitude of Eurabia identified by Bat Ye’or remains to be seen (Ye’or 2005).81

The work referred to, Bat Ye’or’s book *Eurabia*, is not a scientific work. It is a classic political conspiracy theory, built on sources that are so secret that, to the scientific eye, they remain nonexistent. I take a particular interest in Bat Ye’or and her book, for here we have the key source of inspiration for my herostratically infamous countryman Anders Behring Breivik. On 22 July 2011, Breivik released a manifesto that followed the train of thought outlined by Bat Ye’or. Then he went on a murder spree on a tiny island where hundreds of children had assembled to take part in a youth camp organised by Norway’s ruling party at the time (the Labour Party), killing over sixty children. Needless to say, Thayer is not directly responsible for the evil deeds of someone who read a book that Thayer, too has quoted. It should also be needless to say, but clearly is not, that the kind of bond between biological thinking and right-wing terrorism that we see here was a staple of 20th-century life, and is an ongoing concern, not only in Norway, but throughout the world. As I have tried to demonstrate, Thayer’s brand of biological thinking is questionable on scientific grounds. It is also morally uninformed

77. Thayer, *Darwin and International Relations*, 17.
78. Ibid., 14.
79. The opposite error, of leaving out the physiological system altogether, is also widespread; see discussion in Masoma Sherazi, ‘Chapter Two: Theories of Emotion’, MS dissertation, London School of Economics, 2013.
and politically dangerous. The overall point regarding biologically and psychologically informed thinking as a challenge to IR is straightforward: A number of developments in these disciplines, particularly in the areas of neuroscience, evolutionary psychology and epigenetics, are important and potentially enriching for social sciences such as IR. Contra Butler and followers, biology has to be brought back in play. As exemplified here by Bradley Thayer’s work, however, the original Spencerian sins of the social sciences are still tainting what is otherwise certain to be a fruitful scientific exchange.82 The issue is not if we should engage biological and psychological thinking about our subject matter, but how we should do it.

Conclusion

As a social science, IR has its work cut out for it. To do our job, which is to understand what makes relatively stable social relations between polities possible in the first place, and the effects that such stabilisation has on social forms and social outcomes, we need to study more cases, with more finely honed methods, with a certain autonomy, and with an eye to insights hatched in other parts of academia. There exists a whole swathe of peer polity systems that we have not even begun to look at. We lack a proper debate about qualitative methods. We do not stand sufficiently tall under the pressure from social forces, which may colonise our knowledge production. Scholars in other fields who take an interest in our subject matter are making interesting advances that we need to discuss what to do with. The good news is that we have the human power to do this. The transnational has become a tangible everyday force, and one response has been a quantitative explosion of IR. Since I was a student, the number of practitioners of our discipline has increased at least fourfold. A number of these have the languages and the cultural competence needed to round out the universe of cases. As to methods, I suppose those of us who are dissatisfied will simply have to write our own books. And when it comes to the challenge of biology, it has been with us since the beginning. The way to live down the biologistic idea that genes programme us is not to ignore the findings of biology, but to read up on what the more sophisticated biologists have to say about the matter and meet them in the stance that remains key to any scientific being-in-the-world, namely the dialogical one.

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82. After I delivered this lecture, I attended the 2013 annual conference of the International Studies Association, where Johnson and Thayer actually produced a joint paper, so Johnson does not seem to have a problem with Spencerian leanings. See Johnson and Thayer, ‘The Origin of Offensive Realism’.