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**THEORISING INTERNATIONAL RELATIONS:  
LESSONS FROM EUROPE'S PERIPHERY**

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# Abstract

Many regional academic communities in International Relations find themselves as passive recipients of ideas and theories developed elsewhere. Shedding off the role of simple 'ideas-taker' and becoming an autonomous voice in International Relations, academic communities need to develop the conditions for independent theorising. This paper deals with the potential intellectual and institutional obstacles to autonomous theory formation.

A first section argues that the primary obstacle lies within Western IR itself, namely the particularly damaging tradition which denies the very need for more theoretical reflection, at best some day-to-day adaptation of a truth we already know. This position comes in two often combined forms, stating either that IR knowledge is all in historical experience, not fancy theory, or that such theory has been developed long time ago and cannot be superseded (for the unchanging character of world politics). Only if the unfoundedness of this position is shown, can we really tackle the issue of proper IR theorising: 'which theory?'

My second claim is that the peculiar confusion of IR theory with foreign policy paradigms (often wrapped into the infamous realism-idealism divide), and a topical approach to IR theorising are further obstacles to the understanding of the role and significance of IR theory. I argue that it neglects the constitutive function of theories and hence the value of a theoretical enterprise that assesses assumptions at the theoretical and meta-theoretical level, as well as a conceptual analysis which is self-reflective to the context, regional and historical, within which such concepts have been evolving.

Finally, I address the institutional obstacles IR theorising can encounter. Those, or so I will argue, are at least of three kinds. Some obstacles have to do with the intellectual legitimacy of theoretical research in IR within the national academic division of labour, where IR is often relegated to an inferior position, its theory being handled by the 'real' subject-matters. Then, IR theorising, as all research, needs a certain material autonomy. Yet, since the type of theorising I stress in this paper is usually connected to basic research, a claim with little legitimacy in the social sciences, the obstacles are far higher. Finally, the way the field of expertise is organised in a country can contribute to undermine the social legitimacy of the theoretical expert which is looking long-term and might not come to sound-bite ready conclusions. And yet, as I will show in the conclusion, for moving out of the periphery, independent theorising is crucial.

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# Introduction: IR theory in the periphery

‘Why are there no East Asian IR theories? Are there limits of grand paradigms in IR in their applicability to the region? Do we need region or country-based IR theories?’ My task for the conference was to reflect upon the conditions for the development of an independent or at least lively practice of IR theorising, as seen from (some) European experiences. In particular, I wish to raise points with regard to several underlying themes in those questions.

First, there is the classical theme of ‘IR as an American Social Science’.<sup>1</sup> Although it might sound curious to outsiders, many European IR communities have found, and often still find themselves in an acute sense of periphery, under the spell, or the long shadow, of the over-towering US production in IR, IR theory included. Major reference books are written by US scholars, the leading journals are US based (IO having moved just North of it for once), their citations and references invariably US centered.<sup>2</sup> Publishing in those journals are handled as trophies on European CVs with which to make careers. Since the early 1980s, this has produced a type of backlash in Europe.<sup>3</sup> The UK has seen an impressive development of its IR community, also by attracting outsiders in the probably most open and open-minded labour market in IR in the world. There has been also a renewal, almost a re-invention of the so-called ‘English School’ in IR.<sup>4</sup> The book series edited on behalf of the *British International Studies Association* since the late 1980s has become, at least in Europe, the major reference book series in IR. In Germany, the IR section of the German Political Association decided in the early 1990s to become more assertive, and founded the successful *Zeitschrift für Internationale Beziehungen*. The journal quickly took off with a major debate on rationality introducing a Habermas-inspired discussion to confront rational choice on its own terrain.<sup>5</sup> The particular experiences of *Ostpolitik* and the late Cold War are also at the base of the development of the so-called *Copenhagen School* in Security Studies which put even little Denmark on the map of new IR theorising (indeed, it seems to have become fashionable to found Schools and stick some geographic labels on them).<sup>6</sup> On a European-wide level, the *Standing Group of International Relations* of the European Consortium for Political Research was founded in the late 1980s and has organised major conventions ever since.

<sup>1</sup> Hoffmann 1977.

<sup>2</sup> Holsti 1985.

<sup>3</sup> Zürn 1994.

<sup>4</sup> Among the many, see Dunne 1998, Little 2000, Buzan 2001, 2004, Linklater and Suganami 2006.

<sup>5</sup> A summary of the debate is accessible in English through Risse 2000. The Habermas research programme is still going strong in Germany. See Deitelhoff 2006, Niesen and Herborth 2007.

<sup>6</sup> Again, see among the many original writings, Wæver 1995, and Buzan, Wæver et al. 1998.

In 1995, it launched the by now leading IR journal in Europe, the *European Journal of International Relations* which has joined the very top of journals in the Social Science Citation Index (leaving also the US equivalent *International Studies Quarterly* behind).<sup>7</sup> And more recently, Central and Eastern European IR communities try to enhance their region's visibility with their own *CEEISA* and a new journal, the *Journal of International Relations and Development*. In this context, it should be noted that both the prestigious BISA series and the EJIR are heavily political theory-driven. In other words, a first theme underlying those conference questions is the asymmetrical institutional and personal presence of regions in a world-wide community of IR researchers, in particular IR theory researchers. And the Europeans now having joined the core, at least to believe from all their own hype about their successes, are obviously now (again?) contributing to the same core asymmetry.

Closely related is a second underlying topic, namely whether the sheer weight of US production is not creating too much of a bias in the choice of topics, theories and methodologies. How much does the weight of the US in the international IR community pre-ordain a certain understanding of the salience of topics, definitions of science and hence legitimate research? Whereas the first theme meant for the European periphery to find out how best to join the game – as e.g. by building up institutional structures and independent channels for publication, by labelling and branding, finding niches – here the question is more fundamentally whether there is only one game in town. And of course, attempts at placing European IR on the map were often combined with arguments about a 'regional' difference, not just in topics (which is not unimportant, but self-evident), but also in theoretical and methodological traditions. Now, with considerable simplification one could make the argument that the European backlash did try to re-valorise a different tradition in social science theorising as the US mainstream was perceived to have. It is somewhat telling that among its 30 or so sections, there is no independent 'IR Theory' section within the North American *International Studies Association* and that there is now talk about setting up a US based *Journal of International Theory* (with Cambridge) to carve out some autonomous terrain for IR theory within the existing IR discipline in the US (there is less of a difference between 'Europe' and Canada).<sup>8</sup> This second theme underlying the questions of the conference is then the very definition of IR theory and of legitimate IR theorising.

Finally, a third theme, but one I will be able less to touch, is whether the specific regional historical experiences are of such a kind to make different conceptions of politics (e.g. less the

<sup>7</sup> Whether the SSCI deserves to be taken as a standard is a completely different question.

<sup>8</sup> This is a trend, no more. There are strong defenders of the US mainstream of social science also in Europe, in IR not the least visible in the *Journal of Peace Research*, published in Oslo. Inversely, there is probably no position in the social sciences whose most radical defender is not a US American.



European Machiavellian *raison d'État*) and another set of social science concepts necessary than the historical experiences, in particular of Europe and North America, which have become imprinted into the way 'we' approach global politics. As a European, this is difficult for me to answer, since indeed most institutions of international society (consulars and their protection, international law, etc.) either originated in Europe or have a strong European mark and so they come to some extent 'natural'. And yet, I can see how the very different reading of the end of the Cold War might be connected to different conceptions of politics even within the 'West'. Here, then, I would re-phrase this third theme as about types of theorising which would more prominently allow for contextualisation and self-reflection.

These three themes can then also be seen as different ways of thinking about obstacles to the development of IR theorising in the 'periphery'. One set of obstacles is connected to the institutional support for IR studies in general, and IR theory in particular. Another set is connected to the different theoretical obstacles independent theorising of IR can face because of certain theoretical predispositions.

My paper will take up these three themes and obstacles, but in a different order, since I have first to face a very curious obstacle to IR theory. My first section will argue that the primary obstacle lies within Western IR itself, namely the particularly damaging tradition which denies the very need for more theoretical reflection, at best some day-to-day adaptation of a truth we already know. This position comes in two often combined forms, stating either that IR knowledge is all in historical experience, not fancy theory, or that such theory has been developed long time ago and cannot be superseded (for the unchanging character of world politics). Only if this position is shown to be unfounded, can we really tackle the issue of proper IR theorising: 'which theory?' My second claim will be that the peculiar confusion of IR theory with foreign policy paradigms (often wrapped into the infamous realism-idealism divide), and a topical approach to IR theorising are further obstacles to the understanding of the role and significance of IR theory. I argue that it neglects the constitutive function of theories and hence the value of a theoretical enterprise that assesses assumptions at the theoretical and meta-theoretical level, as well as a conceptual analysis which is self-reflective to the context, regional and historical, within which such concepts have been evolving. Finally, I will address the institutional obstacles IR theorising can encounter. Those, or so I will argue, are at least of three kinds. Some obstacles have to do with the intellectual legitimacy of theoretical research in IR within the national academic division of labour, where IR is often relegated to an inferior position, its theory being handled by the 'real' subject-matters. Then, IR theorising, as all research, needs a certain material autonomy. Yet, since the type of theorising I stress in this paper is usually connected to basic research, a claim with little legitimacy in the social sciences, the obstacles are far higher. Finally, the way the field of expertise is organised in a country can contribute to undermine the social legitimacy of the theoretical expert which is looking long-term and might not come to sound-bite ready

conclusions. And yet, as I will show in the conclusion, for moving out of the periphery, independent theorising is crucial, although this does not yet mean that no common theoretical universe can be reached.

## I. Theory? No need!

Whenever I discussed the need to include theories of international relations into the curricula of IR education in Central, Eastern Europe, and also Southern Europe, I faced two prominent types of reactions. The first was simply paying lip-service to the ‘obvious’ need of such theory, seen as the answer any self-regarding academic should provide, an answer given without much conviction. After some discussion, IR Theories appeared as either too abstract (and incomprehensible for someone not trained in some philosophy, social theory or econometrics), or too generic to be of much use for the analysis of world politics. Here, all what was needed, or so the story goes, was to learn from historical experiences which ‘teach’ us what to think of international politics. If there are general patterns, then history will show. Let us call this the approach of *theory as historical experience*. The other reaction was that surely theory was important, and surely we need some system in our analysis which goes beyond the shallow talk of our politicians. If they had only read and understood Morgenthau and Huntington, Clausewitz and Brzezinski, they would know. Let us call this approach *theory as geopolitics*. Both approaches see ultimately no need for any further theory, one because theory is useless, and the other, because although there is useful theory, we do already know it. In a sense, they are the academic equivalent of the ‘know-it-all’ diplomat and military strategist.

It would be easy to brush these concerns aside simply by stating that these are old-fashioned academics who simply do not wish or are not able to understand the need of ‘real’ theory for what they are doing. But that would be far too complacent. Indeed, in somewhat disguised form, these two attitudes are very present in the very core of IR, too. In some of the academic battles in the alleged core regions of IR theory, theorists face the very same resistance. The programmes which are taught in professional IR MA programmes in North America and now in Europe are often playing out a combination between the theories of historical experience and the reduction

of IR theory to geopolitics.<sup>9</sup> Indeed, theory is usually relegated to secondary status, since, being theory, it is allegedly less useful here for future practice, a view I have tried to rebuke elsewhere.<sup>10</sup>

Let me therefore elaborate a bit on the extraordinary resilience of these two positions who ultimately deny the need for more theory. They can be combined (and many realists have been oscillating between the need to defend their approach through a theory, and the very refusal to do so), but for the sake of clearer presentation I present them as ideal types side by side.

## 1. NO THEORY NEEDED: THEORY AS HISTORICAL EXPERIENCE

This position has a long pedigree. It is almost a founding position for the modern study of International Relations, at least as it developed in Europe. For this has been the way IR practitioners has always defended their claim to superior knowledge against outside intruders. In their bandwagon, also those scholarly observers revert to it when they wish to move the analysis of politics away from normative ideals to empirical experience, to ‘politics, as it really is’.

Friedrich Meinecke will be my witness for this position. In his treatise on ‘the reason of state in the newer History’, Meinecke retraces this tradition back to Machiavelli (but not Hobbes). He goes as far as to claim an elective affinity between the work of the (international) statesman and the (modern) historian.<sup>11</sup> Similar to thinkers of the reason of state, History should be studied with the history of ideas as integral part to it, and yet a history of ideas of sorts. For that history of ideas should not be conceived in terms of a clean but sterile history of dogmas. Rather, it should distill ideas out of the actually experienced (‘die Verwandlung des Erlebten in Ideen’) as seen through the eyes of the political movers.

Indeed, the modern historian and the statesman in the tradition of the reason of state (should) use the same empirical methodology. In his opening page already, Meinecke writes that, strictly speaking, there is only one ideal way for state action, one ideal reason of state. ‘To recognise this is the burning endeavour of the acting statesman, as well as of the historian looking back.’<sup>12</sup>

<sup>9</sup> I should perhaps add on a personal note that I have experienced this myself during my three years at Sciences-Po Paris in the 1980s.

<sup>10</sup> Guzzini 2001, in which e.g. I try to show that a theoretical education allowing to translate from one theoretical language to another is crucial for diplomats who need to acquire an inter-cultural sensibility.

<sup>11</sup> Meinecke 1957 [1924/29], pp. 22-25.

<sup>12</sup> Meinecke 1957 [1924/29], p. 1: ‘Sie zu erkennen ist das heiÙe Bemühen des handelnden Staatsmannes wie des rückschauenden Historikers.’

During the times, when, according to Meinecke, historiography was still under the spell of natural law and its concern with the ideal state, the discourse on the reason of state taught already how to think (and make) History as ‘practical History’. And here the parallel in terms of thinking *Staatskunst* (the art of government) and History comes to the fore:

Acting according to the reason of state reached relatively early a way of seeing and understanding which was akin to modern historical cognition. Modern historical cognition, in turn, profited also from the reason of state, from the attraction that emanated from the teaching of the interests of states, which was used as auxiliary practical science for the ‘art of government’ since the 17<sup>th</sup> century by those involved in the latter.<sup>13</sup>

Note how easily Meinecke slides from the *action/practice* of the *raison d’État* to the *understanding* of politics and History, as if practice and the knowledge of it are intrinsically connected, a position typical for the classical position in IR: ‘tradition’ becomes both a repertoire of practices and heuristic devices – in that order first. But then, if the reason of state is a privileged partner for establishing an empirical methodology for the history of ideas, such history also becomes the distilled essence and sedimented knowledge for the art of government. Statesmen and the modern historians blend into each other in the quest for understanding states and their interests in the motion of world history.<sup>14</sup> The result is not universal knowledge, but practical ‘maxims’, which is the way Meinecke defined the reason of state.

In this utilitarian understanding of politics, means and ends replaces the classical Aristotelian concern with the best government or the common good. Consequently also a different morality applies to this ‘politics’ so redefined, in which certain situations demand *of necessity* (as Morgenthau would later say) the trespassing of usual moral and legal norms *by the statesman*. Indeed, good statesmanship is defined by this practical knowledge for artful trespassing in the interest/reason of the state. It is not fortuitous that Meinecke stresses the need to look at the

<sup>13</sup> Meinecke 1957 [1924/29], pp. 22-23: ‘Das Handeln nach Staatsräson gelangte also verhältnismäßig früh zu einer Art des Sehens und Erkennens, die dem modernen historischen Erkennen schon verwandt war. Das moderne historische Erkennen aber profitierte deshalb auch wieder von der Staatsräson, von ihrer Ausstrahlung in der Lehre von den Interessen der Staaten, die seit dem 17. Jahrhundert von solchen, die der Staatskunst nahestanden, als praktische Hilfswissenschaft derselben gepflegt wurde.’

<sup>14</sup> See also Meinecke’s argument that the German Historical School after Ranke created a ‘historical realism’ not based on scholastic opinion (*Schulmeinung*), but on real forces (*reale Kräfte*). That programme was then understood by few, but realised by Bismarck and so became ‘through Ranke the fundament of all real historical thinking and through Bismarck the fundament of all independent (*unbefangen*) political thinking.’ Meinecke 1916, p. 4.

reason of state through the eyes of the practitioner. Nor that he sees the emergence of this discourse as parallel to the emergence of modern diplomacy during the Renaissance.<sup>15</sup> It is the language of diplomatic practice as it evolved over time. As Jutta Weldes remarks elsewhere, the language of the national interest is the language of IR practice.<sup>16</sup>

Now, it is no difficult argument to see in the reason of state, i.e. in its empirical epistemology, its utilitarian or rationalist definition of politics and in its practical maxims, including balance of power politics<sup>17</sup>, a forerunner of (many) realist theories in International Relations. Yet, it is first and foremost a practical knowledge, precisely not yet the attempt to turn it into a social science. Meinecke is very explicit that a purely empirical and utilitarian study of the reason of state is necessarily limited and general catalogues for the ideal behaviour of states not possible.<sup>18</sup> He ridicules the attempt to understand politics like ‘clock mechanics’ (and reads Hobbes in this tradition).<sup>19</sup> Indeed, Meinecke says that by its very nature, a clear definition of the concept of reason of state is not possible.<sup>20</sup> Nor is a calculus of the real interests of states always possible, since the interests are often ambivalent themselves, so that the statesman will often have to choose between Scylla and Charybdis, and since the dilemmas of political necessity escape a clear assessment.<sup>21</sup> Looking at ‘politics, as it really is’<sup>22</sup>, Meinecke sees the classical world of tragedy as the field of statesmanship: only with the sense of history and the experience of politics given by the reason of state can statesmen hope to acquire the art of statesmanship (*Staatskunst*). It is a

<sup>15</sup> Meinecke 1957 [1924/29], p. 176.

<sup>16</sup> Weldes 1999, p. 3.

<sup>17</sup> Meinecke 1957 [1924/29], p. 100, sees in the *Lehre* of the European balance of power nothing else than a detail of the general *Lehre* of the reason of state.

<sup>18</sup> Meinecke 1957 [1924/29], pp. 174-175.

<sup>19</sup> Meinecke 1957 [1924/29], p. 188.

<sup>20</sup> Meinecke 1957 [1924/29], p. 245.

<sup>21</sup> Meinecke 1957 [1924/29], p. 275.

<sup>22</sup> Meinecke 1957 [1924/29], p. 165.

reflective, but ultimately practical knowledge. Hence, neither might this practical knowledge be transferable into the experimental logic of explanatory theory, nor the other way round.<sup>23</sup>

## 2. NO NEW THEORY NEEDED: THEORY AS *GEOPOLITIK*

Besides this position which denies a ‘scientific’-‘theoretical’ status to our knowledge in international relations, there is a second tradition which accepts that status, but basically believes that all important things have already been said. The contemporary theoretical discussion which one would find in, say, the *European Journal of International Relations* or the *Review of International Studies*, even in *International Organization*, not to speak of the more quantitative studies in the *Journal of Conflict Behaviour*, appears as misplaced, an ill-conceived attempt which confuses the internal requirements of scientific rigour with the more mundane and contingent logic of the world of International Relations. Academic references will instead inevitably go to *Foreign Affairs* and its equivalent in the UK, *International Affairs*, or at most to *International Security* which fashions a language often close to geopolitical thought. Early critiques of the ‘scientific’ turn include of course Hedley Bull in the famous ‘second debate’<sup>24</sup> and more recently the attack by Stephen Walt about the ‘rigor mortis’ of rational choice<sup>25</sup>. But let me take some remarks by Hans Morgenthau which are perhaps even more scathing.

Morgenthau could declare that although Martin Wight’s ‘Why is there no international theory?’<sup>26</sup> was wrong (for Morgenthau, there *is* theory), ‘[i]ts fourteen pages contain more insights into the intellectual issues posed by theoretical concern with international relations than a whole shelf of books and articles that, following the fashion of the day, spin out theories of international relations and embark upon esoteric methodological studies on how to approach such theory-

<sup>23</sup> This is a reformulation of the main thesis in my book on realism. I argued that the realist tradition in IR can best be understood as the repeated, and repeatedly failed, attempt to translate the maxims of classical European diplomacy into more general laws of a US social science (Guzzini 1998, p. 1). But I found the reasons not to be lying with the nature of social theory or the impossible theorising beyond historical experience, but with the vague conceptual apparatus upon which practical knowledge draws its lessons and the type of scientific theory aimed at. In other words, the problem is as much with the practical knowledge itself, as it is with the probably unreachable ideal of a positivist social science. To this, see also below.

<sup>24</sup> Bull 1966.

<sup>25</sup> Walt 1999. Note, however, that this attack is itself misdirected against rational choice, when the very basis of the defensive realist programme, to which Walt subscribes, is based upon the same rational actor assumptions. ‘Rational choice’ stands here rather for the attempt to turn towards defining as respectable only science which is quantifiable – which is a different point.

<sup>26</sup> Wight 1966.

making'.<sup>27</sup> This quote clearly shows his two targets: the attempt to fashion theory in terms of testable models and the tendency to discuss methodology, or, to put it differently discuss theory in meta-theoretical terms (what he calls 'theory-making'). Those mis-conceived 'theories' mistake 'politics' as something fully amenable to reason and measurement. This contradicts 'the objective character of international relations' and produces 'dogmas', a 'kind of metaphysics, regardless in what empirical or mathematical garb it is clothed'.<sup>28</sup> By not talking truth to power in a way relevant, such theoretical research only serves to bolster the *status quo*.<sup>29</sup> By expressing itself in a language and with formula inaccessible to common sense, this research ultimately only serve the narrow interests and psychological self-satisfaction of scholars who do not dare to make statements whose closer contact with 'political reality' could disconfirm them.<sup>30</sup> Hence, this 'new scholasticism' displays worrying similarities with religion: 'the suggestion of profundity and mysterious knowledge implicit in unintelligibility'.<sup>31</sup> Useless, misconceived, coward, self-serving and uncritical to power (during the Vietnam war), dogmatic and religious... with colleagues within your own discipline like this, who needs enemies? A colleague of mine used to have a cartoon on his door which shows a person answering with relief: 'Ah, you are a terrorist; I first understood you were a theorist'.

But this little excursion was not just meant to provide some nice quotes. For Morgenthau's aim is twofold: against Wight, he wishes to defend to the possibility of an international theory, against behaviourism (which he connects *pêle-mêle* with economic approaches, liberalism, utopianism), he wishes to define its necessarily limited character. And here, the story gets suddenly more muddled. In the very same papers in which he expresses this scathing critique of the new type of theorist, Morgenthau also sketches the nature of possible IR theory, of the real theorist like himself. It is a theory which goes beyond a philosophy of history, in that it makes explicit the theoretical assumptions upon which philosophically inclined historians (he refers to Thucydides and Ranke) have made their analysis and then uses history to 'demonstrate' their validity.<sup>32</sup> And this goes beyond mere historical experience, since politics is both contingent but also rational; and this limited 'rational element in political action makes politics susceptible to theoretical

<sup>27</sup> Morgenthau 1970 [1964], p. 248.

<sup>28</sup> Morgenthau 1970 [1967], respectively pp. 242, 243 and p. 246.

<sup>29</sup> Morgenthau 1970 [1967], p. 247.

<sup>30</sup> Morgenthau 1970 [1964], p. 261.

<sup>31</sup> Morgenthau 1970 [1967], pp. 246, 247.

<sup>32</sup> Morgenthau 1970 [1964], p. 251; here referring to the same body of inspiration as Meinecke. Obviously, this view of history as a museum deposit from which to freely pick exhibits for impressing the public of the day will meet little agreement with both proper historians and social scientists who wish to control for selection bias.

analysis', whereas, to conclude on a comfortably ambivalent position, its contingent element 'obviates the possibility of theoretical understanding'.<sup>33</sup> 'Within these limits, a theory of international relations performs the function any theory performs, that is, to bring order and meaning into a mass of unconnected material and to increase knowledge through the logical development of certain propositions empirically established.'<sup>34</sup> Indeed, in a suddenly positivist answer to Wight's critique that whereas domestic theory is about progress (and hence amenable to theory), IR is the realm of recurrence and repetition, Morgenthau writes that it is 'this repetitive character of international politics, that is, the configurations of the balance of power, that lends itself to theoretical systematisation.'<sup>35</sup> Theory meets *geopolitics* here understood as the objectivist component of realist theorising.

Hence, Morgenthau looks for regularities which can be empirically established and historically demonstrated and finds it in classical balance of power theory. He does see that any historical explanation necessarily relies upon theoretical assumptions which need to be made explicit. But then why would he oppose the attempt to systematically test those regularities in controlled and often quantitative studies? How would we otherwise know whether the balance of power applied? And why would he believe theoretical critiques of other theories to be useless, when they do exactly perform that exposing and discussing of underlying assumptions? If empirical regularities cannot be established in a quantitative way because no historical case is really like any other, also his own theory cannot be established or justified. If making theoretical assumptions explicit is the work of theorising, then the theoretical critique of such assumptions where theorists improve on theorists, is an unavoidable and fundamental part of IR theory, not idle self-centred talk.

In my view, Morgenthau displays here a dilemma of the 'classical' tradition in IR, mostly but not exclusively realist. Following Kissinger's analysis of Metternich<sup>36</sup>, I have proposed to call this enduring dilemma of realism the 'conservative' or justification/tradition dilemma.<sup>37</sup> Faced with criticism of realism's scientific character or its findings, realists have been repeatedly tempted to lean towards less stringent understandings of their own theory's status. Realism then refers to a

<sup>33</sup> Morgenthau 1970 [1964], p. 254. Such an ambivalent position leaves it obviously Morgenthau to decide when there is enough, and when too much science. The measure is his own way of theorising.

<sup>34</sup> Morgenthau 1970 [1964], p. 257.

<sup>35</sup> Morgenthau 1970 [1964], p. 251.

<sup>36</sup> Kissinger 1957, chapter XI.

<sup>37</sup> For this paragraph, see Guzzini 2004b, pp. 534–35, and 546–48.



philosophical tradition or more generally ‘an attitude regarding the human condition’<sup>38</sup>. Yet, when realists want to retreat to a more ‘traditionalist’ position, they are caught by a dilemma which has existed since its beginnings in International Relations. Despite Morgenthau’s early insistence on the intuitions of statesmen and the ‘art’ of politics<sup>39</sup>, realism derived much of its appeal from its claim to understand reality ‘as it is’ rather than as it should be.<sup>40</sup> But ever since the foreign policy maxims of *Realpolitik* have ceased to be commonly shared knowledge or understood as legitimate politics, realism can not refer to the world as it is and rely on its intuitive understanding by the responsible elites. Instead, it needs to justify the value of traditional practical knowledge and diplomacy. To be persuasive, such a justification comes today in the form of controllable knowledge. Moreover, since realism self-consciously refers to the world ‘as it is, not as we would like it to be’<sup>41</sup>, it makes a reference to an unproblematic reality, whose truth is not there to be established, but to be empirically discovered. This, in turn, requires a kind of objectifiable status to be checked by methods which can be shared by a wider knowledge community.<sup>42</sup> It means to deal with it in a ‘theoretical, that is, an objective, systematic manner.’<sup>43</sup> In other words, realism cannot have the cake and eat it, too. By avoiding justification, realism loses its persuasiveness in times of a rational debate it decides not to address. Alternatively, by consistently justifying a world-view that should be natural and taken for granted, realist defences testify to realism’s very demise. Today, there is no way back to a time when realism needed little justification; there is no way out of the dilemma. And yet, much of the realist reaction has oscillated between pretending a non-scientific, if not largely anti-theoretical position when attacked from the quantitative corners in IR<sup>44</sup> and a more scientific position when comparing itself with the more traditionalist and classical ‘English School’.<sup>45</sup>

This position always comes back to say that although theory is needed, there is really nothing new under the sun, some amendments to the existing body of balance of power theory will do. It is a remarkable logical circle: since international relations is all recurrence and repetition, as Wight said, also our theories are; and so they must according to *theory as geopolitics*, because so is ‘reality’.

<sup>38</sup> Gilpin 1986 [1984], p. 304.

<sup>39</sup> Morgenthau 1946.

<sup>40</sup> Carr 1946.

<sup>41</sup> Mearsheimer 2001, p. 4.

<sup>42</sup> Brown 1992, p. 90.

<sup>43</sup> Morgenthau 1970 [1967], p. 254.

<sup>44</sup> See e.g. the exchange which followed Vasquez’s (1997) intervention in *APSR*.

<sup>45</sup> E.g. Copeland 2003.

### 3. A FIRST CONCLUSION: THE END(S) OF THEORY – EVERYWHERE?

This first section dealt with obstacles to IR theorising, internal to IR theorising itself, which severely limit the type of theory admissible. By proclaiming a particular relationship from practice (and historical experience) to theory (despite acknowledging the need of prior assumptions for the very understanding of that practice), the aim of theory must be to uncover and discover the truth which lies in history (and human nature, for Morgenthau). This truth covers both the rational aspect of reality, amenable to theorising, and its contingent one which escapes theorisation, at least if the latter is defined in terms of discovering regularities, as Morgenthau does. In a circular move, the end(s) of theorising are also the end of it, since no knowledge which goes beyond and against the nature of international politics, as asserted by Morgenthau, is possible. Any theory beyond is but the personification of the liberal rationalist hubris; any debate about ‘theory-making’ only irresponsible scholastic narcissism.

But how typical is this? Referring, as done so far, to some Germans or German-American might not necessarily go very far. Here, I can only speak from my own experience from several parts in Europe, smaller and bigger states, former Empires or not, countries with longer traditions in the discipline of International Relations and new post-communist communities which are re-arranging their expertise and curricula. From what I have experienced there, the height of the obstacle might differ, but it is present almost everywhere. I will not use those European examples which have been able to reserve a terrain for flourishing IR theorising, in particular the UK, Scandinavia (if less Sweden), and Germany. But let me give some examples of countries which still struggle, both West/South and East.

In Italy, the IR community still struggles to establish itself. At the last convention of the *Società Italiana di Scienze Politiche* (to which IR belongs), for the first time, several panels could be organised and a critical mass of scholars defining themselves as an IR community became visible.<sup>46</sup> True, there have been the occasional islands in academia, most prominently Luigi Bonanate’s work defending a normative theory of IR and hence mobilising the strong Italian tradition in political philosophy.<sup>47</sup> But social theory has been rarely applied to IR. Foreign policy analysis or international studies used to be strongly in the hands of the ‘no new theory’ group, the most representative ‘textbook’ on IR after 1989 being written by a General and on geopolitics<sup>48</sup>

<sup>46</sup> When Marco Clementi tried to arrange a special theoretical issue on Institutions in IR, eventually only the *Rassegna Italiana di Sociologia* showed interest (vol. 44, no. 2, 2003).

<sup>47</sup> Bonanate 1992, 1994.

<sup>48</sup> Jean 1995. For an assessment of the Italian debate before the very recent move, see Lucarelli and Menotti 2002.

and the most successful new journal being *LiMes*, is similarly a journal of geopolitics (although that is here a catch-phrase to talk classical IR). Still, as mentioned, the situation is slowly improving in that a new generation of scholars is able to defend the need for ‘more (and a different) theory’.

In several Central and Eastern European countries, IR theory hardly exists outside of the grip of theory as geopolitics. That was the initial reaction in Russia, which has seen a quite remarkable turn around from its Cold War ideology to a debate in which geopolitics looms dominant.<sup>49</sup> In particular, Alexander Dugin, his *Fundamentals of Geopolitics* and his political activism have attracted the scorn of critics.<sup>50</sup> Estonia has been closer researched than many other countries of the region. Although the exact status of geopolitical thought in Estonia is still disputed<sup>51</sup>, the status of Huntington’s *clash of civilizations* is remarkable. The Estonian Minister of Foreign Affairs wrote the foreword to the 1999 Estonian translation of the book. At the launch of the translation, Huntington visited Estonia and spoke at a press conference together with Estonia’s Prime Minister and Minister of Foreign Affairs.<sup>52</sup> It was extensively reviewed in major newspapers and has more generally become part of popular discourse.<sup>53</sup> For different reasons, also Turkey has stayed mainly in the ‘no further theory’ context where geopolitical discourse is strong although the self-awareness is rising.<sup>54</sup> Even where the geopolitical grip is not strong, as in the Czech Republic<sup>55</sup>, IR theory has so far only achieved a precarious hold on the terrain of IR expertise (although the same Petr Drulák has written one of the first Theory textbooks in CEE by a CEE scholar).

Let me close with the example of France which is multifaceted in many regards and allows me to make an opener for a theme I will take up in the last section.<sup>56</sup> France has an academic tradition in IR which is close to policy making, thereby informing both development studies (Empire!) and foreign policy analysis. This is paralleled by an effective presence of think tanks and semi-academic institutions in IR. At the same time, it has a strong theoretical tradition in both

<sup>49</sup> Tyulin 1997, Sergounin 2000. For an interpretation to see this geopolitical turn as a version of ‘romantic realism’, see Morozov 2002.

<sup>50</sup> Some of the critics not shying away of likening him to a neo-fascist, see Ingram 2001.

<sup>51</sup> For an overview, see Pami Aalto 2000, 2001.

<sup>52</sup> Kuus 2002, p. 307.

<sup>53</sup> Aalto and Berg 2002, pp. 261-262.

<sup>54</sup> Bilgin 2007.

<sup>55</sup> Drulák 2006.

<sup>56</sup> For a still valid interpretation and overview of the French scene in the early 1990s, see Giesen 1995.

sociology and also history (*École des Annales*). That the classical reference to IR theory, Raymond Aron, is a trained sociologist is perhaps not fortuitous, just as the at some point rivalling textbooks openly referred to a *sociology of IR*.<sup>57</sup> Still, it has been only a recent development that theory reflection and also production has gone beyond the grip of the very empiricist ‘no further theory needed’.<sup>58</sup>

Well, with one qualification. All depends how one defines International Relations. If one imports ready-made definitions by the Anglo-American core, then indeed, there is little theory, since the established French debate linked up with the diplomatic and military language often antithetical to theorising beyond geopolitical strategy. If however one thinks in terms of theorising ‘the international’ not predefined as its practitioners do, but in terms of the internationalised research agendas of other disciplines, such as international economics and globalisation<sup>59</sup>, comparative state-building also applied to non-Western states<sup>60</sup>, etc., then the French context has shown far more theorising than meets the ‘IR-focused’ eye. It is perhaps therefore not accidental that French scholars in IR move into the general IR scene, when the boundaries of IR are again contested.

## II. Which theory? What type of theorising?

Whereas the first section dealt with attitudes which discourage further theoretical work and hence provide obvious obstacles to further IR theory development, there are also some theoretical enterprises, which encourage theoretical work, yet only of a certain limited manner. In this section, I will try to show that this limitation can stem from two tendencies. First, there is a tendency in many countries to confuse foreign policy paradigms with IR theories. Relatedly, theorising is asked for, but also guided, by foreign policy events and puzzles which, apparently new, seem to ask for ‘new’ theories. Rather than following the tracks of theoretical research to

<sup>57</sup> Merle 1982, Badie and Smouts 1992.

<sup>58</sup> Representative for that move, see Bertrand Badie and Didier Bigo, both at IEP Paris and at IEP Bordeaux Dario Battistella 2003, now in its second edition. Bigo 1996a, 1996b who attempts to use sociological theory in IR (in particular Pierre Bourdieu) had first launched the French journal *Cultures & Conflits (Sociologie politique de l'international)* which had a stronger theoretical component than usual in France and now also the journal *International Political Sociology*, part of the North American ISA journal package.

<sup>59</sup> See e.g. the French Regulation School (e.g. Michel Aglietta, Robert Boyer and Alain Lipietz).

<sup>60</sup> Badie 1986, 1992.

the analysis of events, the analysis imposes itself in search of a (new) theory. Second, discussing IR theories is often done with only one of the functions of IR theory in mind. Whereas the instrumental/explanatory function sees theories as ‘toolboxes’ for subsuming events and deducing relevant action, there is also a constitutive function of theories checking and providing the hopefully coherent assumptions upon which any empirical analysis is made, just as Morgenthau acknowledged.

## 1. IR THEORY AND FOREIGN POLICY PARADIGMS AND AGENDAS

Theory means different things to different scholars. Whereas the first two examples of theory as historical experience and theory as geopolitics are actually obstacles to further theorising, there are also two more traditional ways of theorising in IR which I believe to have a negative effect in that they again narrow the function of IR theory. As an example of these two other ways of theorising, let me refer to a debate which took place at the convention of the German Political Science Association in 2003 and was later published in the *Zeitschrift für Internationale Beziehungen* (vol. 11, no. 1, 2004). This debate was part of the wide-spread soul-searching in many IR communities after 9/11: what should be its effect on IR theories? A tendency in this debate consisted in positing a particularly salient policy issue, here terrorism or asymmetrical warfare, and then ask theories what they had to say about it – and whether it affected those theories. Two main strategies for this test were devised, one which tended to reduce IR theories to foreign policy paradigms and one which defined theorising from the event backwards to theories and not the other way round. Both are neglecting a significant part of IR theorising.<sup>61</sup>

Mistaking foreign policy paradigms with IR theories is a first fallacy detrimental to encompassing IR theorising. In the Germany debate, this was exemplified by Charles Kupchan. He noted that ‘if terrorist groups do gain access to nuclear weapons and resort to their use, a paradigm shift in the field of international relations would be necessary. This development would call into question our understanding of deterrence, the consequences of power asymmetries, and other core concepts within the field.’<sup>62</sup> But that is not at all self-evident. It is not clear whether any of the core concepts of our *theories* were challenged by 9/11, as in fact other contributors to the debate noticed. But that is actually not really Kupchan’s target. The target is foreign policy concepts; not deterrence as such, but our changed understanding of successful deterrence policies. In other words, his argument rests on a confusion between foreign policy paradigms (or general strategies) and IR theories, very frequent in foreign policy circles around the world. Often this confusion is

<sup>61</sup> For the complete analysis, see Guzzini 2004c.

<sup>62</sup> Kupchan 2004, pp. 102–3.

packaged in the infamous realism-idealism debate, where both get attached to certain foreign policy strategies. This is then paired with strategies of deterrence versus strategies of reassurance, or ‘containment’ versus ‘engagement’. Indeed, much of international analysis comes down to specify whether or not we find ourselves in a situation in which rather one than the other strategy should be taken.<sup>63</sup> In this optic, IR theories *are defined backwards* from the foreign policy strategies countries have at their disposal. De facto, theories are reduced to foreign policy paradigms (of major powers).<sup>64</sup>

As a result, the ‘realism-idealism’ debate is often pitched at the level of foreign policy strategy, not at the level of theory. Indeed to many in that debate, it is not quite clear what difference there might be. In another famous exchange at a meeting of the *International Studies Association*, John Mearsheimer said that the Balkan wars showed him that high hopes for post-1989 could not be uphold and that realism was to reign supreme again. To this, Friedrich Kratochwil answered that, if he understood correctly, realism was right, whenever things go bad in international relations: it was the ‘shit happens theory of IR’. And indeed, innumerable newspaper articles but also scholarly pieces seem to make the assumption that whenever there is conflict, realists get it right, and when there is cooperation, then liberals score. But putting it so bluntly makes clear, just how ludicrous this argument is. Both theories have to account for both conflict and cooperation (and why things have always to come in binary divisions in the major US debates is yet another relevant question). The question is not whether there has been conflict and hence realism is right, but to find out *why* there has been conflict, and there realism provides just one set of hypotheses (and the same argument goes the other way round for cooperation and liberalism). For instance, if ever *A clash of civilization* is going to happen, geocultural realists would point to Huntington’s thesis which derives from objective cultural differences and their necessitous clash (under certain conditions). Peace researchers would make the argument that such clash could well happen, if all believed in Huntington’s thesis and acted accordingly: a classical case of a self-fulfilling prophecy.<sup>65</sup> The clash itself, were it ever to happen, does not yet score points for any one theory, as long as the causes are not clarified.

<sup>63</sup> See e.g. the classical analysis by Robert Jervis (1976) whose very elaborate framework is fundamentally establishing many devices for finding out whether one faces a status quo power (and hence reassurance) or an expansionist power (hence containment).

<sup>64</sup> A similar tendency to define IR theories from foreign policy strategies can be found in Russia. See Sergounin 2000.

<sup>65</sup> For a discussion of peace research’s interest in self-fulfilling prophecies and its lineage to contemporary constructivist theories, see Guzzini 2004a.

But there was also a second way of handling theory in the wake of 9/11 during that German debate, which narrowed the scope of IR theorising, although in a far lesser manner. Again, one would start from the event *backwards* and not from theories to the events. Hence, Thomas Risse, after having clearly shown that there was no real need to change any of the theories, tried nevertheless to see why there was so little research on terrorism.<sup>66</sup> But in a sense, that way of putting the question already pre-determined the answer: we had a foreign policy/public label for which we needed scientific clarification. But what if that particular label would come as a subcategory of other more important issues, such as, for instance, the decline of the monopoly of legitimate violence or, more generally, the diffusion of collective violence away from the territorial state for which terrorism would be just one, and not necessarily the most salient issue, as the recent discussion on Private Military Companies shows?<sup>67</sup> What if theory points towards issues which are not necessarily high on the present foreign policy agenda but possibly of long-term significance? Theorising is basic or fundamental research, eventually applicable, sure, but far wider than it appears in the application of the day.

## 2. EXPLANATORY/INSTRUMENTAL AND CONSTITUTIVE FUNCTIONS OF THEORY<sup>68</sup>

Another way of limiting IR theorising consists in a certain understanding of theory where it only serves as a toolbox for analysis and action. If a certain policy or event cannot be easily subsumed under some 'theories', if, worse, theorists do not agree on how to explain the event, sometimes not even about how to classify or name it (was the attack on the Twin Towers an incidence of 'war?'), then basically theory is useless, indeed at times damaging good political judgement, its teaching to be discouraged.<sup>69</sup> But this neglects a very important second function of theories, which derives from the constitutive nature of knowledge. The latter makes its teaching absolutely crucial for developing the capacity of critical analysis and self-reflection, and hence independent learning once today's students will have become tomorrow's leader. I will make my argument by showing the constitutive nature of knowledge, the therefore crucial importance of conceptual analysis and the reflexive relationship between observation and social reality in which observation can in itself have an effect on this very reality it is supposed to describe.

<sup>66</sup> Risse 2004.

<sup>67</sup> See e.g. Singer 2003, Avant 2005, Leander 2005a, 2005b, 2006.

<sup>68</sup> For this subsection, see also Guzzini 2001.

<sup>69</sup> Wallace 1996.

In the philosophy of science, there is nobody, positivists included, who holds the position that ‘data speaks for itself’, that is, that we can neutrally access empirical data. Any empirical explanation relies on a priori concepts. The question then becomes whether the choice of such concepts, albeit necessary, can be neutral with regard to the event to be explained. And here, some scholars are however let to bracket those questions. In IR, the possibly most famous research programme in this more empiricist tradition is the ‘Correlates of War’-Project. This project, led in the early days by David Singer, wants to find out which antecedent conditions correlate with war.<sup>70</sup> The project is based on a large historical database of international conflicts for which we have enough information to code them. The project is inductively driven, in that it wants to derive knowledge from empirical correlations about which antecedent events correlate with war. In its cautious self-understanding, this is the only possible way to get unbiased information.

Apparently absent, theory enters twice into this type of explanation. First, as the scholars stress themselves, theory is needed since these correlations do not *explain* anything in a strict sense of the word. For they do not answer the question, *why* things correlate. Only an argument about causes can help us to find out whether the correlations are not spurious. But second, theories enter the analysis already *before* or rather *for* the establishment of these correlations. As already mentioned, we need concepts to code these events. Without concepts as meaningful data-containers, we cannot distinguish music (a meaningful fact) from sheer noise.<sup>71</sup> Pure induction is not possible. In turn, such concepts simply cannot be divorced from theoretical or pre-theoretical assumptions. This is also called the necessary theory-dependence of facts. How do we know, for instance, that the things we compare over the millennia, and which we call with the same concept, here: war, are actually the same? Did they mean the same to the actors then and now? Does that matter? The very possibility and risks of ‘conceptual stretching’<sup>72</sup> is dependent on certain assumptions about e.g. history or of human nature.

Such an awareness of the conceptual and theoretical underpinnings is also crucial for understanding the possible interrelationship between the observation of social reality and the very reality itself. Such reflexive relation between theory and practice can happen both at a macro-level

<sup>70</sup> For assessments of the findings, see Vasquez 1987, Geller and Singer 1998.

<sup>71</sup> For an early statement, see Sartori 1970. For a more recent restatement, see Gerring 2001, Part 1.

<sup>72</sup> Sartori 1970, Collier and Mahon 1993.



or at a micro-level.<sup>73</sup> At the macro-level, it refers to the already mentioned phenomenon of self-fulfilling prophecies. Just imagine all European powers would have heeded the advise of Mearsheimer and prepare for an inevitable German rearmament and possible nuclearisation.<sup>74</sup> Post-Cold war Europe would probably have been so antagonistic as to encourage the Germans to do exactly this whether they initially had even contemplated such policy or not. On the micro-level, this reflexivity refers to what Ian Hacking calls a 'looping effect'<sup>75</sup>, that is the phenomenon that social facts and in particular identities interact with the way they are called. Hence, the concepts we use can have a performative effect, not just reflecting but interacting with social reality.

Exactly because data does not speak for itself, because all observation is theory-dependent, and because observation can in itself have an effect on the very reality it is supposed just to describe, it is obvious and fundamental that observers of international relations, whether practical or academic, be trained to become aware of their own and others' assumptions. They must understand both the explanatory and the constitutive function of theories.

### 3. A SECOND CONCLUSION: THEORISING IR WITH META-THEORY, CONCEPTUAL ANALYSIS AND HISTORICAL SOCIOLOGY

But for doing so, theorising is more than just hypothesis testing or historical induction, so prominent in the so-called 'Second Debate'. Instead, it strongly focuses on the meta-theoretical and philosophical, as well as on the conceptual level where most of our basic theoretical assumptions are buried.<sup>76</sup> Looking at this particular level, also enables a wider sensitivity to historical or cultural differences, not only for the *results* of studies (Western theories applied elsewhere), but already in their theoretical setup. At the meta-theoretical level this refers to basic assumptions about ontology and epistemology and here certainly also cultural differences can play a role. At the philosophical level, this could most prominently refer to the different ideologies and political cultures that might surface in the very understanding of the nature of politics. And finally, a crucial focus on concepts must open up for an approach not solely in

<sup>73</sup> For a fuller exposition of this mainly constructivist concern, see Guzzini 2000, and applied to conceptual analysis, see Guzzini 2005.

<sup>74</sup> Mearsheimer 1990. For an early response, see Van Evera 1991.

<sup>75</sup> See e.g. Hacking 1999.

<sup>76</sup> See, for instance, the work of Friedrich Kratochwil 1989, 2006; Alexander Wendt 1987, 1992, 1999, Part 1; and Colin Wight 2006.

terms of a positivist conceptual analysis (defining the most appropriate meaning of a concept for analysis), but more in the tradition of conceptual history and of historical sociology in which concepts are continuously updated in their meaning when confronted with larger historical developments. When Max Weber introduced a long conceptual apparatus at the start of his *Economy and Society*, this was both the condition for the possibility to develop his social theory, as much as it was already the fruit of his analysis. When scholars in IR are trying to update concepts like sovereignty or power, they do that in touch with ongoing historical developments.

In some sense, general theories are hard to come by in IR. But a type of a frameworks of analysis is possible, whose meta-theoretical assumptions have been discussed in the light of our present knowledge and whose conceptual apparatus has been updated and re-thought when trying to deal with ongoing events. This might move the whole issue from theoretical imperialism to a type of meta-theoretical imperialism, since this approach to 'theorising' also imposes potentially Western meta-categories to other cultural environments. Perhaps. But since it does not come in terms of general theories, it should allow specificity and, possibly, some cross-cultural communication about it.

### III. Which IR? Institutional obstacles to IR theory

Institutional obstacles are more country-specific. And yet I think one can still generalise about three main types of obstacles. First, and quite trivially, theorising is in less dire need to justify itself if scientific traditions in that country used to reserve a major importance to theory. In particular, the conditions for IR theorising are much improved if, in this context, IR has succeeded to carve out an independent terrain within which it is allowed to draw on several scientific traditions and not being reduced to only one. Second, a shift from basic research funding to competitive funding can undermine the role of IR theory research, at least as understood here. For IR theory is then part of basic/fundamental research in the social sciences where such research has a weak standing in the general public. Moreover, topics considered relevant there might not be driven by concerns of theoretical innovation. Third, IR profits from, but its theorising can also be threatened by, a relation to political actors, public or private, when the latter wish to define the relevant agenda. IR is close to politics, closer than even political science. The demand for IR expertise is large. When that demand intrudes or starts even to dominate the research agenda, it can have a deleterious effect on independent IR theorising. In a sense, these are three layers within which IR theorists have to defend their autonomy and

legitimacy: intellectually within and across the discipline, materially within the political economy of research funding, socially within the wider field of IR expertise.

## 1. INTELLECTUAL LEGITIMACY: SCIENTIFIC TRADITIONS AND THE HISTORICAL DEVELOPMENT OF ACADEMIC DISCIPLINES

The obvious point is that IR theorising has it easier if there is generally a strong theoretical tradition in a nation's academic culture. That academic culture might well start at high school already. In my own experience, such a culture permeates all social sciences. It basically prepares for, and legitimates the role of, precise abstract thinking (as opposed to 'theoretical' in the sense of general and generic statements). The German academic culture has certainly the reputation of being theory-laden, which is visible in its strong tradition in the philosophy of science (and not just methodology), the sociology and philosophy of law, social and political theory and hence also IR theory. During my years of teaching in Central Eastern Europe, most of the theoretically inclined students were from Romania and Russia, formal and quantitative analysis often from John (János) von Neumann's home country Hungary.

A further institutional advantage consists in having a clearly defined autonomous and respected field of International Relations within the world of academic disciplines in that country. This is far from self-evident. Many academic traditions run International Relations as a sub-discipline of another discipline, most often political science, but sometimes also international law (some Latin American countries and some Mediterranean countries), international history (UK and France), 'polemology' or war studies (again UK and France), peace and conflict studies (Scandinavia and Germany).

Being a subdiscipline can have two negative effects on IR theorising. The first is that IR theory is neglected since all the relevant theory is done within the main discipline. After all, if IR had such a distinct theoretical body, or if its theorising, even if little distinct, was so avant-garde as to justify an independent set-up, then the main discipline would need to re-think itself. Hence, many political but also social theorists wait until the end of their career to add those pages on the international realm which are mostly added to the prior domestic analysis. To make things worse, sometimes those books are not materialising or cut short by the death of the author, as it was for Marx or Weber.<sup>77</sup> In more contemporary times, an academic division of labour results in which

<sup>77</sup> The passages dedicated by Weber to international politics in his *Economy and Society* are just 16 pages long and stop in the midst of a sentence. See Weber 1980 [1921-22], pp. 514-530.

IR is relegated to analyse peace and war (and perhaps a bit globalisation), or any topic for which there might be some 'external determinacy', i.e. all the residual terrain left open by the main discipline. And IR theory is imported only to the extent it does provide that residual knowledge.<sup>78</sup> By doing so, this type of academic division of labour reinforces a secondary role of IR theorising at best. In fact, the real theory has already been done elsewhere.

The second negative effect is that it cuts IR off its almost natural transdisciplinary base. Being connected to whatever other discipline imports the particular prejudices of that social science. It hardens divisions when IR should be part of overcoming them. In the new disciplines of the social sciences in Central and Eastern Europe, international relations is sometimes covered by scholars coming from political economy. This has some very healthy consequences in that they look at the subject matter through different lenses than the established IR discourse. But then, it also tends then to re-define simply IR in terms of that other tradition, not seeing how a wider trans-disciplinary agenda is perhaps warranted.

One should immediately add that part of the problem lies again with IR itself. Facing these problems of being relegated to the position of a residual theorist at best and facing the pressure to accommodate one rather than more disciplinary traditions, IR scholars have often been fighting for their independent departments (and have been successful e.g. in the UK). In an academic turf war, such independence would allow finally serious and respected autonomous IR scholarship. But the basis for this fight has often been almost as negative for contemporary IR theorising, as the institutional independence could turn out beneficial. For the normal justification for an independent subject-matter locks IR into the 'inside/outside' division which many or most IR theorists are wary about.<sup>79</sup> In that division, sovereignty and the monopoly of legitimate violence are the taken-for-granted starting point – precisely when both are in a major historical and conceptual re-definition today. But for setting IR apart, that divide 'functions' and is often repeated. Whereas within states, so the story goes, the government has the monopoly of legitimate violence, in their relations to each other, there is no overarching monopoly, but sovereign states meet unfettered. In their relations to each other, states are still in a state of nature, and if they were not, so Raymond Aron, an independent theory of international relations

<sup>78</sup> See e.g. Giddens 1985, who imports realist and statist thought into his social theory, as if that were the only one in IR.

<sup>79</sup> For the still major argument on how that inside/outside distinction locks IR (and political science) into a specific academic division of labour which is detrimental to a political theory that needs to be global, see Walker 1993.

would cease to exist.<sup>80</sup> And hence there is the clear risk that the benefit of autonomous work (and funding) is paid by being locked in a self-understanding which simply reproduces the old and indeed often theory-unfriendly environment of IR (see above).

One should add, though, that some IR departments have been able to then import specialists from different fields (a strategy for big departments). Also, in some circumstances, IR scholars have preferred to stay within the discipline and define their turf therein; they occasionally even succeeded in tasking over political science departments. For they have the advantage that IR, usually being considered secondary, was rarely enough to make a career and so IR scholars often publish also in a major subfield of political science (political theory, public policy, comparative government), whereas few political scientists have a thorough scholarship in IR to be able to do the reverse.

## 2. MATERIAL AUTONOMY: THE POLITICAL ECONOMY OF RESEARCH FUNDING

There are some basic material factors which can strongly influence the incidence of theoretical work, including for International Relations. If basic salaries are low, and teaching staff needs to make ends meet by teaching a lot, then research time evaporates. If the country is relatively poor and universities need to raise money from tuition, students will ask for marketable education. Depending on whether the market looks for personal maturity or rather technical skills, that can have the effect to crowd out theoretical teaching, at least anything beyond the ‘theory we know already’.<sup>81</sup> If the government believes that research is a luxury that needs to be financed independently of the basic educational budget, or if this comes in the new public management excuse of ‘competitive research funding’ (for cutting funds), then organisational standard operating procedures and the philosophy of the funders will kick in, be they research councils or private foundations. Since research councils have usually mixed juries, theoretical research projects are rarely funded, if they cannot quickly show an empirical application. Private foundations can be sometimes more even-handed and allow also fundamental research in the social sciences; but often, they are even less so. If theory is conceived as fundamental research, then it often stands a hard chance. Research councils in the humanities can sometimes pick up the bill although the philosophy professors might not know what to do with social theories of IR.

<sup>80</sup> Aron 1962, p. 19.

<sup>81</sup> Sometimes, students simply buy the reputation of a degree which is the most marketable component. Ironically that can mean that those degrees having the best reputation as *professional* degrees, are the ones which can allow more *theory* in, since they have less to prove themselves as ‘practical’.

But it will be increasingly hard to have someone producing an impressive work in social theory like Niklas Luhmann who, at the time of his hiring in 1969, described his research project as: ‘theory of society, duration: 30 years, costs: none’.<sup>82</sup> Time is a rare good these days and buying time is difficult for some type of theorising outside of basic funding.

This leads to the basic claim of this section: when research funding is becoming less guaranteed, but more application-based, and university funding increasingly teaching (and tuition) oriented, this has an influence on the *type* of IR theory which can still flourish. One of the issues which most enraged Stephen Walt in John Vasquez’s critique of the realist research programme was the latter’s explicit recommendation that realist research projects should not be given research funds, since the theory was degenerative and non-falsifiable.<sup>83</sup> Only quantitative empirical testing would stand a chance to be ‘real’ theory and hence fundable – a further prove to the still unconvinced, that debates in IR are about survival. Yet, whatever their differences, both traditions would probably combine forces to downgrade proposals in a yet different theoretical tradition more informed by philosophy and normative theory or conceptual analysis and social theory. For that type of theorising might be good for the humanities, but not for social sciences, or so the story goes, it should be ‘political science’ not ‘political theory’.<sup>84</sup>

Hence, just as with the academic division above, funding does not only touch theory in general, but some theorising more than others. It is quite typical that research funding is given to topics which clearly indicate their ‘social and political usefulness’. It would probably be an embarrassing picture to see how many research proposals were tuned to accommodate the expected ‘usefulness’ of research and their titles (check: ‘terrorism’). The more research is competitively funded and not steered by scholarly cumulation, the more such ‘adaptations’ can be expected to happen. Private foundations and funds often include a social statement for the improvement of social or human conditions which make appear funding to theoretical topics out of synch, that is topics, where the aim is to improve theory or theorising itself with a far more long-term effect. In some countries, public research councils are increasingly under pressure to justify their decisions in similar terms. And of course, this situation is easily exacerbated in countries, where governments (or private actors, for that matter) try to use their funds to influence research agendas.

<sup>82</sup> Luhmann 1997, p. 11: ‘Theorie der Gesellschaft, Laufzeit: 30 Jahre, Kosten: keine’.

<sup>83</sup> Walt 1997.

<sup>84</sup> Note that in some languages, there is no such difference between science on the one hand, and arts or humanities on the other. In German, also philosophy is a ‘science’ (Geisteswissenschaft).

### 3. SOCIAL LEGITIMACY: ORGANISING THE FIELD OF IR EXPERTISE

Whereas the previous section saw in a guaranteed (but ex post conditional, see below) funding a necessary condition for a broad fundamental research in IR, a funding in many countries provided by the state, this section cautions about another type of usually state intervention. More precisely, I want to argue that specific ways of organising public expertise in International Relations can undermine the role and place of IR theorising. The main claim is that if 'IR' expertise is strongly defined by the expectations of the short-term and tangible, that is media and government politics, this definition and self-definition of an expert tends to empower the camp of the 'no theory', the 'we have all the theory needed' and the purely instrumental understanding of theory mentioned above. Such a definition very quickly crowds out any legitimate need for any type of alternative 'expertise'.

In Europe, this development has been accompanied by a decline of the classical division of labour or democratic check-and-balance which a more critical understanding of the role of research had established several decades ago. That vision saw researchers as part of the civil society corrective to politics. The idea of an outside observer to political debate with the task of questioning the existing political agenda (whether progressive or conservative) has many troubles to survive in some Western European countries or become established in some communities in the CEE. In both a much more technical (and certainly less critical) vision of the expert is now in demand. Again that favours an understanding of theorising in a purely instrumental not constitutive manner.

In the worst case, the field of IR experts can become characterised by a type of war between the different claims to knowledge. In this environment, any academic who is not doing analysis declared politically useful by the think tankers, or publically communicative by the journalist is in a huge social justification deficit. Indeed, portraying academia, and theory therein particularly, as an elitist past-time can become part and parcel of the identity of journalists and think-tankers. Indeed, at times, think tanks, rather than becoming conduits for the passage of knowledge out of the research community can end up blocking it and/or replacing it, crowding it out (in particular in countries where short-term consultancy can make much more money than miserable research salaries).

That pushes theorists in a type of schizophrenia where they need to justify their real work by other communications (teaching often does the job, though). Hence, again, the pernicious effect is not generally on academic knowledge or the general status of the scholar, but mainly to that

practice of theorising developed above. It empowers the other academics in the field, just as the journal peer-reviewed publication standard usually help to empower the theorists.

What is at stake is not to stay isolated from politics. Quite to the contrary. Keeping political interference at arms' length does emphatically not mean that IR theorising should not be in contact with political changes. That would be its end, since its conceptual development depends from that contact (see above). Much of the changes in IR are to be directly observed at the places where they are happening. Today, this is just not primarily the Foreign Office, but somewhat unexpectedly perhaps, the military (which can be far more open than 'we know it all' politicians and diplomats), NGOs, MNCs (including now PMCs), financial actors, the system of offshore deterritorialisation, organised crime, or the negative (and somewhat surreptitious) European integration through the European Court of Justice, to name a few, or any their equivalents in the past.

#### 4. A THIRD CONCLUSION: FINDING THE BALANCE BETWEEN EXPOSURE AND AUTONOMY

Perhaps the best way to sum up the possible obstacles that can exist in the institutional setting, here defined in terms of intellectual legitimacy, material autonomy and social legitimacy, is in finding a balance between political and academic exposure on the one hand, and social and intellectual autonomy on the other. In some of the success stories, such as Germany or Denmark, for that matter, theorists have been able to find a materially secured and legitimate territory for their work, partly by being successful in launching theory-driven journals (like the *Zeitschrift für Internationale Beziehungen*, ZIB), partly by seed-funding research institutes or Institute of Advanced Studies with no commitment to teach and a fairly theoretical yet internationalised outlook, such as the late *Copenhagen Peace Research Institute* (COPRI) from which the Copenhagen School originated.

In these cases, such higher autonomy and legitimacy for IR theory was to be guaranteed by an ex post check in which research was assessed through its publication quality, internationally. In the ZIB, some rather established figures of traditional IR in Germany had seen their manuscripts refused in the double-blind peer-review process, something which used to be rather unheard of there (and still is in some other places in Europe). At COPRI, the entire publication strategy was internationalised from the start. If no such control exists, then autonomy might quickly degenerate.



Hence, for this strategy to work, such publication quality must be made to count in the portfolio of researchers and outlets for IR theory must be available. This is the case in Western Europe and starts to be also, with the *Journal of International Relations and Development* in Central Eastern Europe, even though it means predominantly publishing in English. And, of course, those outlets must become more sensitive to different research traditions and concerns. For if scholars get again and again compared and measured with a single set of criteria, those other voices will have a hard time to be heard, even if they do their theoretical job correctly. The internationalisation of IR research is both necessary to allow independent IR theory research as it is a possible threat by homogenising content and method. Hopefully, the more open-ended conception of theorising exposed here can avoid this to some extent.

## Conclusion: ways out of the periphery

In the European context, independent theorising has been able to flourish when a series of conditions were met. It meant first that the grip of the classical understanding that no (new) IR theory is needed, be weakened. It meant further that IR theory not be confused with foreign policy paradigms and theorising not only derived from the event backwards, but from long-term developments towards events. Most importantly, it is encouraged if IR theory is not only approached in its instrumental, but also in its constitutive functions. This valorises the study of ‘theory-making’, that is, assumptions at the meta-theoretical and philosophical level, as well as the crucial conceptual level where historical changes can be retraced if combined with historical sociology which is context and time sensitive. Finally, for such a theorising to gain momentum, it is important that it keeps a certain organisational and material (but not necessarily intellectual) autonomy from other disciplines and research funders, as well as a legitimate role in the social field of expertise which is not reduced to theory’s technical component. This autonomy needs however to be balanced with research-internal but neutral and international checks.

For some countries in particular in Central and Eastern Europe (here excluding Russia), this is a task of very tall, often impossible, proportions. But even for those for which it is not really so (say the Visegrad countries), a far easier and for some at least more lucrative solution consists in simply accepting a position in the international division of academic labour in which they teach and research only on their particular region, passively relying on theories invented by somebody else (or worse, without even any theoretical background which would make them able to relate to other phenomena). International research projects slot in regional scholars for filling out the chapters where ‘regional expertise’ is needed. ‘The view from...’ litters book chapter headings like titles in UN reports. PhD theses will be guided by these requirements mainly.

This position defines a very comfortable turf for some, in particular those who can control access to international funds. But it risks cementing the semi-periphery. Let me, for the sake of drama, express this in crudely economic terms. Knowledge follows, to some extent, a similar path as other products in international trade. Countries are, of course, free to specialise in raw materials, but the history of international trade has shown that there are limits to this. Exchanging their goods, these countries have come to know the dependence on international technology and tastes. Usually their prices are driving down as compared to high value-added goods with a high knowledge component. Instead of being a technology-taker, one should be a trend setter in new technologies. Know-how derives also from basic science, hence the latter can simply not be disregarded, whether, say, in telecommunications or in academic production. Of course, that takes time. But it does not happen if one never starts. Without acknowledging the need for theory, and without developing the possibility for theoretical studies to develop, academic communities risk staying or becoming simple theory-taker (i.e. passive knowledge consumer) and mere data-provider. Without theorising, they reproduce their periphery.

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