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## Abstract

The editors of the special issue, in their call for papers for this special issue, expressed a degree of disquiet at the current state of International Relations theory, but the situation is both better and worse than they suggest. On the one hand, in some areas of the discipline, there has been real progress over the last decade. The producers of liberal and realist International Relations theory may not have the kind of standing in the social/human sciences as the ‘Grand Theorists’ identified by Quentin Skinner in his seminal mid-1980s’ collection, but they have a great deal to say about how the world works, and the world would have been a better place over the last decade or so if more notice had been taken of what they did say. On the other hand, the range of late modern theorists who brought some of Skinner’s Grand Theorists into the reckoning in the 1980s have, in the main, failed to deliver on the promises made in that decade. The state of International Relations theory in this neck of the woods is indeed a cause for concern; there is a pressing need for ‘critical problem-solving’ theory, that is, theory that relates directly to real-world problems but approaches them from the perspective of the underdog.

## Keywords

critical theory, Grand Theory, liberalism, practice, problem-solving theory, realism

## Introduction

One of the questions the *EJIR* team have invited us to address is whether International Relations (IR) theory (specific theories or theory in general) is experiencing stagnation or even crisis in terms of ‘Grand Theory’ and, if so, whether this is a cause for concern.<sup>1</sup> It is, perhaps, worth examining briefly the origin of the term ‘Grand Theory’, which sounds like it ought to have a long and rich history, but, in fact, seems to have been coined by C. Wright Mills (in *The Sociological Imagination* (1959)) as recently as 1959

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as a way of referring to the kind of sociological theorizing exemplified by the work of Talcott Parsons. Wright Mills deplored Parsonian systems theory, regarding it as prioritizing the formal organization of concepts over the task of understanding the social world, which latter task he, as a radical, democratic socialist, regarded as not just the aim but the duty of social scientists.

Twenty years later, Wright Mills, who died aged just 45 in 1961, was more or less forgotten, but the term Grand Theory was revived to very different effect by Quentin Skinner in a collection of essays entitled *The Return of Grand Theory in the Human Sciences* (1985). Whereas Mills regarded the scientism of Parsonian sociology as actually characteristic of Grand Theory, the one thing the disparate group of thinkers celebrated by Skinner had in common was that their Grand Theories rejected ‘the assumption that the natural sciences offer an adequate or even a relevant model for the practice of the social disciplines’ (Skinner, 1985: 6). Skinner’s Grand Theorists were Hans-Georg Gadamer, Jacques Derrida, Michel Foucault, Thomas Kuhn, John Rawls, Jürgen Habermas, Louis Althusser, Claude Lévi-Strauss and the *Annales* historians — no room in this list for Niklas Luhmann, a very grand theorist one might have thought, but someone whose systems theory was clearly too scientific in aspiration to beat the cut; similarly, economists, game theorists and the like could not expect recognition in this company, and although Skinner’s introduction dutifully refers to the Women’s Movement as adding ‘a whole range of previously neglected insights and arguments’, no woman was grand enough to get on the list (and only one of the 10 chapters is authored by a woman, Susan James on Louis Althusser).

Wright Mills celebrated the imagination, and one could agree that Skinner’s theorists certainly displayed imagination, but whether they also investigated the social world is open to doubt — some (Habermas, Rawls) aspired to do so but most produced ‘world-revealing’ as opposed to ‘action-guiding’ theory, to adopt Stephen White’s useful distinction (White, 1991). Still — more to the point — what has all this to do with IR theory? To qualify as ‘Grand’, a theory should presumably have implications beyond the immediate discourse within which it was created, and Grand Theorists are, more or less by definition, figures who have name recognition across the human sciences as a whole, or at least some of the latter. It is difficult to think of any theorist within the discourse of IR of whom this could be said.<sup>2</sup> A test here is grammatical — Skinner’s theorists are mostly figures whose names can be turned into adjectives; Machiavellian and Clausewitzian are recognizable adjectives, and if we wanted, very anachronistically, to stretch a point, we could say that they are figures within our discourse, but there are no modern equivalents.

Of course, we in IR know what we mean when we call something ‘Waltzian’ and that, presumably, is one of the reasons why the *EJIR* team identify *Theory of International Politics* as initiating a period of innovation and contestation, but: (a) it is difficult to think of other IR examples (Wendtian constructivism perhaps?); and (b) however important Waltz’s work is to us, it is at best vaguely recognized within Political Science in general, and pretty much unknown in the broader field of the human sciences (Waltz, 1979). The implications of this are, or ought to be, somewhat sobering. IR as a discourse is self-reflective to a fault — we continually inspect the state of our art, engage in inter-paradigm debates, and so on — but we need to realize that very few people outside of our own parish are interested. We have been consumers not producers of Grand Theory; the

exchange between our discipline and the rest of the social/human sciences is pretty much one way, and not in our favour.

It is important to make these points to encourage a degree of realism (in the ordinary language sense of the term) in the way we approach our discourses, and to discourage the more grandiosely self-important accounts of IR theory that do occasionally appear. It will be suggested in this article that the last decade has seen the publication of some really important work in IR theory, and the argument will be made that if there is a 'crisis' at all in the field, it stems from a refusal to engage with 'real-world' issues rather than from any lack of theoretical imagination — but although the general state of IR theory is pretty healthy, this should not be taken as licence to overrate the importance of our discipline within the social sciences more generally. Nor should we take this relatively lowly status as a reason for importing uncritically ideas generated elsewhere. Instead, we should take pride in the fact that simply as students and observers of international relations — one of, if not the, most important terrains of contemporary social/economic/political life — we have a lot to offer to the wider world of learning, and there is no reason to be unduly depressed by the fact that this does not include many theoretical innovations that will be picked up by other social scientists.

## Contemporary theory: Then and now

The editors of the special issue appear to approach contemporary IR theory in a somewhat sceptical manner, with words such as stagnation to the fore — the implicit, and sometimes explicit, proposition is that the period of theoretical innovation and contestation post-1979 is drawing to a close, or, indeed, has ended. Then we had inter-paradigm debates and post-positivist critiques, now the excitement is over and we are becalmed in the doldrums. Is this so? It is not at all clear how one might approach this question and it seems implausible that any kind of rigorous answer is going to be available whatever method of doing so is adopted. Still, one thing that is clear is that this kind of judgement cannot be made without some kind of examination of the sort of work that is being done now and the work that was being done then. At least a rough-and-ready compare-and-contrast of the 1980s and the 2000s is called for — and the result is by no means a clear win for the earlier decade. Approaching this question in terms of the major theoretical works of the two periods may actually be a little too rough and ready, not least because it privileges books over the journal literature, but it seems the simplest way to go, and is not likely to be too misleading.

Consider first 'liberal' approaches, broadly defined. The most important liberal text of the 1980s was undoubtedly Robert O. Keohane's *After Hegemony* (1984), which should be coupled with Robert Axelrod's *The Evolution of Cooperation* (1984) — between them Keohane and Axelrod defined 'liberal institutionalism', which became the dominant form of liberal international theory, along with the notion of a 'democratic peace', where Michael Doyle's two-part essay 'Kant, liberal legacies and foreign affairs' (1983) is the ur text. For what the 2000s can offer in the way of liberal theory, the oeuvre of G. John Ikenberry from *After Victory* (2000) to *Liberal Leviathan* (2011) is central, and should be considered along with his Princeton colleague Anne Marie Slaughter's *A New World Order* (2005) and, 'republican' rather than strictly liberal, and somewhat idiosyncratic but cognate, Daniel Deudney's *Bounding Power* (2008).

For realism in the 1980s, again broadly defined, Robert Gilpin, *War and Change in World Politics* (1981), is a text of similar scope to Waltz's magnum opus — William Wohlforth in 'Gilpin realism and international relations' (2011) argues that it would actually have been a better foundation for modern realism than *Theory of International Politics*. Stephen Krasner's *Structural Conflict: The Third World against Global Liberalism* (1985) and Stephen Walt, *The Origins of Alliances* (1987), also must be mentioned as major texts of that decade. The 2000s can offer John Mearsheimer's *The Tragedy of Great Power Politics* (2001), the definitive statement of 'offensive realism'; Charles Glaser, *Rational Theory of International Politics* (2010), revitalizing the use of rational choice theory in realist analysis; and Richard Ned Lebow's *The Tragic Vision of Politics* (2003) and *A Cultural Theory of International Relations* (2008) which, along with Michael C. Williams, *The Realist Tradition and the Limits of International Relations* (2005), contributed in a major way to the revival of classical realism.

For the 'English School' in the 1980s, Hedley Bull's *The Anarchical Society* (1977) is a defining work, albeit out of the period but his *The Expansion of International Society* (1984) edited with Adam Watson was almost as influential, and R.J. Vincent's *Human Rights and International Relations* (1986) is a central text for 'solidarist' English School thinking. In the 2000s, Barry Buzan's *From International to World Society* (2004) pulls together constructivism and the English School, while Andrew Hurrell, *On Global Order* (2007), is a more conventional English School statement, as are Ian Clark's studies of legitimacy and hegemony in international society (2007, 2011).

'Constructivism' as a distinctive approach to IR was just emerging in the 1980s. The core texts here are Friedrich Kratochwil, *Rules, Norms and Decisions* (1989), and Nicholas Onuf, *World of Our Making* (1990), along with some essays by John G. Ruggie. For the 2000s, Alex Wendt, *Social Theory of International Politics* (1999), misses the decade by a few months, and Friedrich Kratochwil's *The Puzzle of Politics* (2010) is an essay collection rather than a monograph, but neither can be ignored in this context. Vincent Pouliot, *International Security in Practice* (2010), points constructivism in one direction; Samuel Barkin, *Realist Constructivism* (2010), in another; and Janice Bially Mattern's *Ordering International Politics* (2005) in a third. A subset of broadly constructivist approaches can be identified under the banner of 'securitization'; here, a key text from the 1980s is Barry Buzan, *People, States and Fear* (1983); modern work veers towards post-structuralism in the case of Lene Hansen, *Security as Practice: Discourse Analysis and the Bosnian War* (2006), and the English School in the case of Buzan (see, e.g., Buzan and Ole Waever, *Regions and Powers* (2003)), but the core securitization model remains very fruitful — see, for example, Buzan and Hansen, *The Evolution of International Security Studies* (2009).

International Political Theory (IPT) as a modern discourse owes much to foundational statements by Charles Beitz, *Political Theory and International Relations* (2000 [1979]), which brought contemporary analytical political theory and IR theory together for the first time, and Terry Nardin, *Law, Morality and the Relations of States* (1983), which offered a firm basis for a pluralist account of international society, grounded in the work of Michael Oakeshott. Andrew Linklater's *Men and Citizens in the Theory of International Relations* (1990 [1982]) is a key text in the history of IPT, and links to critical theory. IPT in the 2000s is more disparate, and, at the analytical end, more article-based, but still

there are major theoretical statements to admire; Simon Caney's *Justice Beyond Borders* (2006) and Thomas Pogge's *World Politics and Human Rights* (2002) stand at one end of the discourse, David Boucher's *The Limits of Ethics in International Relations* (2009) at another, with Toni Erskine's *Embedded Cosmopolitanism* (2008) somewhere in-between.

The critical/'late modern'<sup>3</sup> category of IR theory again was created in the 1980s; Robert Cox's *Production, Power and World Order* (1987) is important in establishing a Gramscian approach to IR, but his earlier *Millennium* article 'Social forces, states and world order: Beyond IR theory' (1983) was the most influential work of critical theory of the decade, establishing a distinction between 'problem-solving' and 'critical theory' theory that is still widely employed (and will be problematized below). 'Late modern' approaches to IR can almost be said to begin with Richard K. Ashley's *International Organization* article 'The poverty of neorealism' (1984) which, as well as giving a name to the perceived shift in the nature of realism in the work of Waltz, Gilpin and Keohane, is innovatory in introducing 'continental' thinking to IR debates. *International/Intertextual Relations: Postmodern Readings in World Politics* (1989) edited by James Der Derian and Michael Shapiro followed up this latter move. This is a category of theorizing that by the 2000s has become very disparate and any list of important works will be more idiosyncratic than in the case of the other discourses presented above; one reason for this disparity is the difficulty of thinking of late modern work in terms of research programmes (a point returned to below); another is that some late modern work can be considered under the constructivist label (see, e.g., the work of Hansen and Bially Mattern referenced above). Tentatively one could identify Maja Zehfuss, *Wounds of Memory* (2007), Kimberly Hutchings, *Time and World Politics* (2009), Christopher Coker, *Barbarous Philosophers* (2010), Ken Booth, *Theory of World Security* (2007), Jens Bartleson, *Visions of World Community* (2009), and Andrew Linklater, *The Problem of Harm in World Politics* (2011), as major statements, each highly distinctive. Postcolonialism is a field that originated outside of IR, but a number of IR scholars have contributed to this discourse: see, for example, Naeem Inayatullah and David Blaney, *International Relations and the Problem of Difference* (2004), and more recently Arlene Tickner and Blaney, *Thinking International Relations Differently* (2012).

Jean Bethke Elshtain, *Women and War* (1987), and Cynthia Enloe, *Bananas, Beaches and Bases: Making Feminist Sense of International Relations* (1989), along with the *Millennium* Special Issue of 'Women and International Relations' (1988), are foundational texts for the study of gender and feminism in IR in the 1980s; it is difficult to find equivalents from the 2000s with similar scope, but J. Ann Tickner, *Gendering World Politics* (2001), and Cynthia Weber, *International Relations Theory: A Critical Introduction* (2001), at least could be seen in this context; Laura J. Shepherd, *Gender, Violence & Security: Discourse as Practice* (2008), links to late modern work on discourse analysis.

This selection of works from the two periods is not in any way intended to be definitive and the categories are obviously not as clear-cut as this arrangement suggests. There is also the danger that scholars who are genuinely unclassifiable will drop out of sight; as the essays collected by Renee Marlin-Bennett in *Alker and IR: Global Studies in an Interconnected World* (2011) suggest, this is not a fate that should befall the

extraordinary work of Hayward Alker. The purpose of these lists is to establish the terrain for an assessment of the changes that have taken place over the last 30 years. To make such an assessment, we need to have some concrete idea of what we are talking about, and that means identifying bodies of work, difficult, and inevitably contentious, though that task may be. The claim here is simply that any scholar in the field — perhaps the reader of this article — looking to identify the major statements of the 1980s and 2000s would include a good proportion of these works. There is no requirement that the list presented here be the same as the list the reader would produce — as long as there is substantial overlap, a fruitful discussion is possible.

### Progress or stagnation?

Imre Lakatos defined a research programme as a collection of theories and techniques that cluster around a hard core, a central proposition that is protected by auxiliary hypotheses (Lakatos, 1970). On his account, research programmes can be progressive, growing in significance, generating new facts, better predictions, new techniques, even new theories consistent with the hard core, while degenerating research programmes produce none of the above. We should not take this too seriously. *Pace*, for example, John Vasquez's attempt to show that structural realism is a degenerating research programme, I would argue that in reality there are no theories in IR that meet Lakatos's criteria (like Karl Popper, and in spite of — or perhaps because of — his academic location at the London School of Economics, Lakatos never considered the social sciences as anything other than pseudoscientific), and in any event what we are looking at is the discipline as a whole which if it contains research programmes at all, contains more than one (Vasquez, 1997). Still, Lakatos does offer a collection of useful pointers to help us to assess progress or stagnation within the broad categories identified above; in comparing the book-based snapshots of the 1980s and 2000s, can we say of particular approaches to the discipline that they are generating new concepts, propositions or theories which build on past achievements? Are they generating new knowledge, or new ways of looking at and understanding the world? Or, conversely, are they stagnating, failing in the 2000s to build on advances made in the 1980s?<sup>4</sup>

I suggest that, taken in the round, progress is discernible, even though there are some areas of the discipline where a negative assessment would be difficult to avoid. At the level of the discourse as a whole, there have been attempts to argue its irrelevance from outside the discipline, but they have, it seems to me, failed. I have in mind in particular the critique of conventional IR launched by sociologists such as Anthony Giddens and political theorists such as David Held under the banner of globalization theory (e.g. Giddens, 1999; Held, 2004). Their position was (and is) based on the proposition that IR is too wedded to the state as the key actor, and thereby simply incapable of understanding the changes that are taking place in the world today. This was a product of the 1990s, and in 2012, no longer looks plausible — business-school hype about borderless worlds seems enormously wide of the mark in an age where homeland security is the dominant rhetoric and intractable problems ranging from environmental degradation to conflict in the Middle East require state-centric solutions. In any event, as Justin Rosenberg has pointed out, a theory which employs the same notion

— globalization — as its dependent, independent and intervening variable is not offering a particularly coherent view of the world (Rosenberg, 2002). None of this is to deny that some of the themes identified by theorists of globalization are of real importance, and, perhaps rather surprisingly, notions to the effect that some form of world government is inevitable have returned to the agenda, see, for example, Deudney's (2008) *Bounding Power*.

Returning to the discipline of IR, progress is discernible in only some areas rather than across the board, and — for reasons which will be elaborated below — progress is largely (perhaps by definition) concentrated in the more conventional areas of the discipline. Thus, first, the approach to liberalism characteristic of the 'Princeton Project' and associated writers seems to meet the criteria for progress set out above; it clearly builds on traditional understandings of liberal IR theory, but it adds a new, refined, understanding of the circumstances under which cooperation takes place. The emphasis that Keohane and Axelrod placed on institutionalizing cooperation, self-binding and the role played by lengthening 'the shadow of the future' has been taken up and given deeper significance by Ikenberry in his account of the value of using inevitably temporary dominance to create lasting structures of rules. Second, it seems reasonable to say that modern realism builds on the realism of the past — here, we can also see the development by Glaser of refined techniques of analysis as well as the extension of structural realist theory by Mearsheimer; Williams, Lebow and others have not just revived classical realism, they have given it firmer roots. The same might be said of various modern English School writers — *pace* the, I think undue, reverence paid to Bull and Vincent, figures such as Buzan and Hurrell have certainly advanced knowledge in this area.

These three sets of theories — liberalism, realism and the English School — are all branches of the discipline where the notion of a research programme would be internally recognized as more or less appropriate; their adherents, sometimes explicitly, sometimes implicitly, actually see themselves as engaged in developing such programmes; that is, at a risk of labouring the point, they see themselves as developing new concepts, propositions and theories which build on past achievements. The same might also be said (implicitly) of theorists of global social justice such as Caney and Pogge, but other normative theorists, Boucher for example and perhaps Erskine, understand what they are doing in terms of a wider discourse of political theory where the 'scientific' aspirations of a research programme have no place.

Here, we return to the notion of 'Grand Theory' referred to in the introduction to this article, and to fundamental questions about the nature of the social sciences; the key question is whether the kind of thinking about 'theory' that figures such as Lakatos employ is, if suitably adapted, capable of providing a model for work in the human sciences. Many realists and liberals and some English School and normative theorists would answer this question with a qualified 'yes' and so are comfortable with the idea that they are contributing to the development of a research programme. Others reject this aspiration and in this they are joined by scholars working within critical and 'late modern' genres, genres which are explicitly based on the rejection of 'neo-positivist' social science. Thus, when, in his Presidential Address to the International Studies Association in 1988, Robert Keohane invited the scholars he called 'reflectivists' and I am calling 'late



modernists' to develop a research programme, it was universally held by these theorists that he had missed the point of their work, which was precisely to avoid being caught up into such an approach to the social sciences (Keohane, 1988).

To ask whether late modernist theory shows signs of being part of a progressive or a degenerating research programme would be to repeat Keohane's category error, but this ought not to mean that there are no standards that can be applied when examining examples of this work from different time periods. Such an examination might well lead one to conclude that a particular work is, for various reasons, superior to another — for example, because it refines the argument, deepens it or challenges it successfully. In this way, even while not buying into the language of progressive versus degenerative or stagnating as a way of thinking about late modern discourses, one might still be able to assess whether 'progress' in a much looser sense has been made. The 1980s saw many programmatic statements about the potential value of the work of the Frankfurt School, Foucault, Derrida, Heidegger and other masters of continental thought for an understanding of international relations, and it is not unreasonable to ask whether this potentiality has come to pass. My inclination would be to suggest that on the whole it has not; a number of names have been added to that list — Lacan, Schmitt, Rancière, Agamben, Luhmann — and much good work has been done, but it is difficult to see the major shift in our understanding of the world that was promised then.<sup>5</sup> And the turn to continental thought has actually had some unfortunate consequences — just as formal modellers are sometimes with justice accused of being more interested in their models than in the world they are trying to illuminate with them, so late modern theorists seem often to start from the wrong place. Like the quantifiers of old, they have a hammer and are looking for a nail; instead of bringing to bear the insights of a particular theorist to deal with a specific problem, too often it is a case of moving in the opposite direction, starting with the theorist *du jour* and choosing the problem as an afterthought.

What of the category of 'constructivism'? It is, I hope, clear that constructivism is not a theory of IR in the sense that liberalism and realism are theories of IR — rather, it is a set of dispositions towards social reality that lead to placing more emphasis on ideas, values, norms and practices than is the case with both rational choice and structuralist theories. The role of theory here is rather different from that of its role in liberalism or realism, for example — much more to do with identifying areas for research than with developing explanatory concepts. To think in terms of a constructivist research programme even in the loose way in which I have used the term here would be misleading. Still, some element of progress in terms of pure theory may, or may not, be discernible and, in fact, I think the latter is the case. It seems to me that while it is possible to name a number of excellent empirical case studies illuminated by constructivist thought, there are no major theoretical statements that are as compelling as those by Kratochwil and Onuf nearly a quarter of a century ago — both authors have elaborated on their respective positions since then but the value of their original texts remains, I think, unchallenged by anything that has been produced since. Something similar might, perhaps, be said about feminist work in IR; there has been a great deal of really interesting, high-value work on women in the military and the gender dimension to the global economy, for example, but it is less clear that there has been much theoretical progress since the programmatic statements of figures such as Jean Bethke Elshtain and Cynthia Enloe in

the 1980s. Still, even more than in respect of other areas considered in this article, feminist IR theory is an area where the present author's grasp of the current discourse is somewhat insecure.

So, it seems plausible to suggest that in some areas of IR theory, innovation has not taken place and the promises made during the period of innovation and contestation after *Theory of International Politics* hit the discipline have not been met. Why have some areas flourished, and others not, and what are the consequences of this discrepancy?

## Problems and progress

The two areas of IR theory where progress is most obvious are the conventional discourses of realism and liberalism. These are also the 'problem-solving' areas of the discipline, to use Robert Cox's formulation in his 1983 article cited above. Cox compares 'problem-solving' theory unfavourably to 'critical theory' — but it can be argued that it is the very fact that these areas of our discipline are problem-oriented that has driven the progress they have achieved. Further, the 'problem' that writers such as Mearsheimer, Lebow and Ikenberry, in their very different ways, were confronting can be identified; it was the foreign policy pursued by the administration of George W. Bush, or, to be more accurate, it was not so much the foreign policy itself, but rather the attitudes that underlay it, which generated a great deal of the progress that took place in the last decade.

Consider, for example, Ikenberry's work over the decade. His essential position is that multilateralism is important, and that whatever dominance the US possesses, it should employ to design rules that will lock in place the liberal international order that has served the US so well over recent decades. Initially, in *After Victory*, he looked back to the post-1945 era to praise the efforts of the Truman and Eisenhower administrations in setting the scene for the quarter-century of economic growth that lasted into the 1970s, and exhorted the US to attempt the trick again in the post-Cold War era. A decade later, in *Liberal Leviathan*, he anticipates the end of any kind of American hegemony — the Liberalism 3.0 described in that book will have a wide group of leading states, certainly including China — but the story is still one of self-binding. It is in the medium- to long-term interest of the US to promote institutional arrangements which will lock in place rules that will ensure the future of a liberal economic and political order, even if this may occasionally be harmful to short-run American interests. This is the most coherent account of liberal IR currently available — although, as will be developed below, some important caveats about the picture of the world it presents need to be registered — and it seems plausible that it is an account that derives its urgency from the perceived need to counter the policy attitudes displayed by the Bush administration. The latter's, at times open, contempt for multilateralism and International Law, evidenced by moves such as the 'unsigned' of the Rome Statute on an International Criminal Court, the sabotaging of climate change agreements and the egregious violations of international humanitarian law during the War on Terror, presented a world-view that, from a liberal internationalist perspective, demanded a forceful rebuttal — and received one in Ikenberry's work.

The perfect expression of the attitude that demanded to be confronted by liberals (and, for that matter, realists) was offered by an unnamed aide in an oft-quoted interview with Ron Suskind, reported in the *New York Times* on 17 October 2004:

The aide said that guys like me were ‘in what we call the reality-based community,’ which he defined as people who ‘believe that solutions emerge from your judicious study of discernible reality.’ I nodded and murmured something about enlightenment principles and empiricism. He cut me off. ‘That’s not the way the world really works anymore,’ he continued. ‘We’re an empire now, and when we act, we create our own reality. And while you’re studying that reality — judiciously, as you will — we’ll act again, creating other new realities, which you can study too, and that’s how things will sort out. We’re history’s actors ... and you, all of you, will be left to just study what we do.’<sup>6</sup>

For a ‘problem-solver’, this is an intolerable attitude — and resistance to it is to be found in the realist writers as well as from liberal internationalists such as Ikenberry. Indeed, if there is one maxim which all realists could join all liberals in giving assent to, it is that *no* state is or could be actually in a position to create its own reality in the way that is suggested here. The revival of ‘classical realism’ in the last decade is, arguably, based not simply on the non-inconsiderable merits of the work of Morgenthau et al. but also on the sense that the Bush administration was exhibiting precisely the traits that the classical realists most deplored — the belief that the US was morally superior to other states, that it could act without considering the wider context of power and interests, and that the notion of absolute security made any kind of sense. These were attitudes that many believed had been successfully defeated in the 1940s and 1950s and their revival called forth a corresponding revival of the thinking that was successful in the earlier period.

One of the tropes of the classical revivalists has been that Waltzian ‘structural realists’ have done little to contest these pernicious ideas, but the work of John Mearsheimer at least refutes this accusation. From his *Tragedy of Great Power Politics* through to his work with Stephen Walt on the ‘Israel lobby’, his version of offensive realism has had clear policy implications and has incorporated an explicit critique of the Bush administration’s pursuit of the ‘War on Terror’ (Mearsheimer and Walt, 2008). Mearsheimer acknowledges the vital interest of the US in securing Middle East oil, but believes this is best achieved by the deployment of ‘over the horizon forces’, and, most important, argues that the US should be oriented towards off shore balancing of the rising power of China. Whether or not his view that US policy has been distorted by the Israel lobby is accepted, it is clear that, from the structural as well as the classical realist perspective, this policy has been misguided — although perhaps structural realists are less likely than their classical confreres to associate this distortion with Republican as opposed to Democrat administrations.

To summarize, the argument is that liberal and realist theorists in their different ways have been part of the so-called ‘reality-based community’ in the 2000s, and that, in an attempt to combat the leanings of those who believed themselves to be capable of making up the rules as they went along, these theorists have actually refined and developed their perspectives in IR in innovatory and progressive ways. They have been joined in this project by some adherents to other schools of thought, but, strangely, not by a great many adherents to the more obviously grand of the ‘Grand Theories’, the late modernists.

## Conclusion: The poverty of Grand Theory

Clearly, many late modern thinkers have been every bit as angry with George W. Bush as the liberal and realist theorists discussed above, and this anger has led to much research

focused on, for example, the War on Terror. This has produced some thinking that has, without doubt, illuminated our understanding of the last decade. For example, the Schmittian notion of a 'state of exception' has provided an interesting route into characterizing the War on Terror as a state of permanent exception — see, in particular, the work of Giorgio Agamben (Agamben, 2005). Similarly, Schmitt's analysis of the figure of the 'partisan' has been useful in understanding the nature of contemporary terrorist movements (Schmitt, 2007). Were I better informed, I could, I am sure, point to other branches of late modern theory where equally revelatory statements could be found. But 'illuminating our understanding', although of course valuable, in the end butters no parsnips. Once one has grasped the contours of the state of exception and the telluric nature of the figure of the partisan, what follows? To return to one of the themes of the introduction to this article, a great deal of late modern work is 'world-revealing' but very little is 'action-guiding'. Very few late modern writers have much time for systems theory, but the critique directed at Parsons by Wright Mills in *The Sociological Imagination* — that his work prioritized the organization of concepts over the investigation of the social world — is a cap that all too often fits.

One might argue that this is of no great consequence. After all, the realist and liberal problem-solvers have actually been successful; the Bush administration's attempt to create its own reality failed, partly because of the efforts of the 'reality-based' community. Even in Bush's second administration, things were slowly returning to normal, and the Obama Presidency has proved open to the work of that community, indeed has accepted many of its liberal members into the bosom of the foreign policy establishment. The value of multilateralism is now officially acknowledged, and, in another key, the administration is indeed disengaging from as many of its Middle East commitments as it can actually disengage from without suffering unacceptable political costs. The 'pivot' to the Pacific is a reality. Still, the absence of a contribution from the theorists who are not conventional liberals or realists is regrettable because of the obvious gaps in the work of those liberal and realist thinkers who have attempted to engage directly with the real world and are now getting a hearing. For example, Ikenberry's Liberalism 3.0 is an obvious improvement on the unilateralism of the first Bush administration and it is obviously in everyone's interest (including their own) that the emerging economies of India and China become stakeholders in the world economic system — but what is much less clear is how the 'bottom billion' of the world's poor will benefit from this new economic order, or, for that matter, how long it will be before the hundreds of millions of Indians and Chinese who are still surviving on low incomes start to see any real benefit from the new status of those countries. There are 'critical problem-solvers' in the International Political Economy community and amongst development economists who address these problems — see, for example, Paul Collier, *The Bottom Billion* (2008), or Jeffrey Sachs, *The End of Poverty* (2005) — but mainstream IR theory, conventional or late modern, has much less to offer.

Again, Mearsheimer's 'over the horizon' approach to security in the Middle East may have the negative value of avoiding Western adventures in the region, but it will do nothing to handle the problem of nuclear proliferation or to resolve the ongoing conflict between Israel and the Palestinians. Classical realists are good at pointing out the follies of neo-conservatism, but have less to offer when it comes to offering policy advice on removing

the oppressive regimes which still exist in many parts of the world — the realist opposition to any kind of ‘humanitarian intervention’ (as witnessed by their opposition to NATO’s campaign in Libya in 2011) may be good for burnishing anti-imperialist credentials but does not address the real problem that liberal interveners are at least trying to respond to. Once again, there are ‘critical problem-solvers’ who dig into the details of doctrines such as ‘Responsibility to Protect’ or who address the demands of humanitarianism in the 21st century — for example, Michael Barnett, *Empire of Humanity* (2011), Barnett and Thomas Weiss, *Humanitarianism in Question* (2008), or James Pattison, *Humanitarian Intervention and the Responsibility to Protect* (2010) — but, once again, these are scholars who work outside of the mainstream of IR theory, whether late modern or not.

In short, there are a range of ‘problems’ that the ‘problem-solving’ theorists are not addressing, and this is where the need for new thinking is pressing. The terminology employed above — ‘critical problem-solving’ — defines an ideal. What is needed is work that is based in the ‘reality-based community’, that is, is produced by scholars who believe that ‘solutions would emerge from their judicious study of discernible reality’ — but the reality with which they would be concerned would be oriented towards the needs and problems of the dispossessed, the ‘wretched of the earth’ as the old song has it, rather than with the problems of the masters of the universe. This would be ‘problem-solving’ theory in so far as it directly engaged with the pressing social problems, but it would also be ‘critical theory’ in so far as it did not take the definitions of such problems for granted. In short, it would compress the two modes of theory identified by Robert Cox into one; Cox’s formula made a kind of sense in the context of the 1970s and 1980s when the need was to combat the hegemony of establishment-oriented theories which made no attempt to problematize the status quo, but the original meaning of ‘critical theory’ was theory that contributed to human emancipation and for this task, problem-solving in the broader sense is essential. Returning again to an earlier formulation borrowed from Stephen White, the aspiration to produce Grand Theory should not be abandoned, but such theory must be action-guiding as well as world-revealing. Unfortunately, this is not a characteristic feature of most contemporary IR grand theorizing.

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## Notes

1. An exception might be Robert Axelrod, whose *The Evolution of Cooperation* (1984) has influenced biologists — although Axelrod himself drew on the work of John Maynard Smith (see, e.g., Smith, 1982) so the case is not clear-cut.
2. I use ‘late modern’ here as a less tendentious, albeit less informative, term than post-positivist, postmodern or post-structural.

3. I disregard here the issue of new techniques; I am concerned with substance, and books on methods do not make the cut unless they are linked to theories on the substance of IR, hence Glaser is in, but Patrick Jackson's recent book, and the literature on scientific/critical realism, is not (Jackson, 2010).
4. This is one reason why the set of books chosen under this category for the 2000s is necessarily idiosyncratic — any selection would be — this is a sign of the absence of a 'research programme' in this area, as a result of which, no books 'choose themselves'.
5. The aide is widely believed to have been Bush's *consigliere* Karl Rove. The full text of Susskind's article is online at: [http://www.nytimes.com/2004/10/17/magazine/17BUSH.html?\\_r=1&ex=1255665600&en=890a96189e162076&ei=5090&partner=rssuserland](http://www.nytimes.com/2004/10/17/magazine/17BUSH.html?_r=1&ex=1255665600&en=890a96189e162076&ei=5090&partner=rssuserland)

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