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Grand Theory in International Relations
An Investigation of a Chimera

Submitted by
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Submitted in partial fulfilment of the requirements for the degree of
Doctor of Philosophy, University of Kent,
June 2022

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To My Parents

Abstract

Grand theory is either understood as a particularly useful form of theory or a deeply problematic form of theory in the discipline of international relations (IR). Grand theory, for some, is useful as a “map” a “big picture” or a “framework” to orient and guide research. For others, grand theory either distracts attention from studying what matters in IR, or it is understood to have so many problematic effects that it should be abandoned. Despite the prominence of grand theory, there is no agreement on what constitutes a grand theory in IR. It is difficult to adjudicate whether grand theory is useful or problematic in general because theorists think that different types of grand theory are either useful or problematic. To gain a better understanding of whether grand theory is useful or problematic, I investigate three particular grand theories: Buzan’s social structural approach (2004), Lebow’s cultural theory (2008) and Ripsman et al.’s type III neoclassical realism (2016). I argue that Buzan (2004) and Lebow (2008) abandon their projects of grand theory while Ripsman et al. (2016) retain their commitment to grand theory. By answering why Buzan (2004) and Lebow (2008) abandoned grand theory, and why Ripsman et al. (2016) have retained their committed to grand theory, I aim to make a contribution as to whether grand theory can be viewed as useful or problematic in IR.

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Acknowledgments

This project is the result of a nine-year intellectual exchange which began at the University of Kent in January 2015. In the second year of my undergraduate degree, I enrolled on a module entitled ‘PO667: War and Peace in International Society’ for which Dr Seán Molloy was the module convenor. Over the course of twelve weeks, Seán introduced me to the English School, its three traditions, solidarism and pluralism, and its eventual reconvention led by Barry Buzan. Later that summer, when pondering a potential dissertation topic for the following academic year, I thought: “Buzan’s idea of grand theory was pretty cool”, “has anyone investigated Buzan’s reconvention to examine whether Buzan successfully created a grand theory?”, “if Buzan created a grand theory was it any good?”, “what would the creation of a grand theory mean for the English school?”. After some initial research, I discovered that such questions remained unanswered. I contacted Seán to ask for a meeting to discuss the possibility of completing a dissertation on Buzan’s reconvention to which he agreed. After listening patiently to my ideas, he said: “well ... I hope you like to work because Buzan wrote a lot” to which I responded: “my parents are Irish, so I’ve been raised to work”. Seán replied: “ah, you’ll be grand so”. My undergraduate dissertation won the School of Politics and International Relations’ Colin Seymour-Ure prize for best joint dissertation, and it was later published as ‘The English School: a new triad’ (2019) in *International Politics*.

The following academic year, I decided to stay at Kent to complete an MA in International Relations for which Seán was once again my dissertation supervisor. Suitably bitten by some sort of grand theory bug, I completed my MA dissertation on Ripsman et al.’s *Neoclassical Realist Theory of International Politics* (2016) titled ‘Neoclassical Realism: The Worst of Both Worlds’. My dissertation won the School of Politics and International Relations’ *Global Society* prize for best MA dissertation. Shortly before the completion of my MA, I was awarded a scholarship to pursue a PhD. When completing research for my PhD, it became clear that Lebow and Buzan

abandoned their projects of grand theory while Ripsman et al. remained committed to grand theory. ‘Grand Theory in International Relations: An Investigation of a Chimera’ investigates why Lebow and Buzan abandoned grand theory, and why Ripsman et al. remain committed to grand theory. It is the end product of my intellectual development as a student.

Looking back over past achievements here is an easy exercise which hides the formidable amount of work and patience they required. Mentoring a student through three degrees, three dissertations, professional publication, teaching in higher education and the trials and tribulations they endure from a nineteen-year-old through to a twenty-nine-year-old man is no easy task. Whatever the outcome of this project, I thank Seán for his mentorship over the past ten years. Any errors contained in this project are, of course, mine alone.

I would be remiss if I did not also thank the School of Politics and International Relations at the University of Kent for their support throughout my intellectual development. Special thanks must go to Dr Harmonie Toros for her help in securing my first co-authored publication, Dr Govinda Clayton (now at ETH Zurich) and Dr Nadine Ansorg for their help in securing my first research assistant post, Dr Andrea Den Boer for her help and guidance through the professional world of academic publishing, Professor Richard Whitman for his support as my undergraduate academic adviser, Professor Trine Flockhart (now at the University of Southern Denmark) for her sound advice and lastly Mrs Diane Arthurs and Dr Edward Morgan-Jones for their administrative support.

On a personal note, I must thank my parents, Gary and Ann Dunleavy, without whom my protracted education would not have been possible. Their continued financial support provided the possibility not only to embark on a PhD but also to complete an MA and BA. I consider myself privileged to have received an education unavailable to most. It is to them my dissertation is dedicated. Finally, I must thank Cerys Jones for her support over the past ten years without which the submission of this project would not have been possible.

Permissions

'Where Has Grand Theory Gone? On Barry Buzan, Grand Theory and the English School' builds on research conducted for my BA dissertation which answered the following question: 'Did the English School lose more than it gained through Barry Buzan's reconvention?'. My BA dissertation did not have a title. I submitted my BA dissertation in April 2015 in partial fulfilment of the requirements for the award of Bachelor of Arts (Honours) in Politics and International Relations at the University of Kent. 'Caught Between Two Stools? Neoclassical Realism, Grand Theory and Paradigms' builds on research conducted for my MA dissertation titled: 'Neoclassical Realism: The Worst of Both Worlds'. I submitted 'Neoclassical Realism: The Worst of Both Worlds' in August 2016 in partial fulfilment of the requirements for the award of Master of Arts in International Relations also at the University of Kent.

Any potential areas of similarity between 'Where Has Grand Theory Gone? On Barry Buzan, Grand Theory and the English School' and my BA dissertation would be contained to the introduction and first section of the paper subtitled 'Buzan's Grand Theory'. Potential areas of similarity would be of a descriptive nature only. The purpose of my BA dissertation was to investigate whether the English School lost more than it gained as a result of Buzan's reconvention. The purpose of my paper herein is to investigate why Buzan has abandoned grand theory. Other than descriptive areas of similarity which reasonably emerge when working with the same theory, the analysis of Buzan's social structural approach provided in this dissertation is original and does not replicate my previous work. Any potential areas of similarity between 'Caught Between Two Stools? Neoclassical Realism, Grand Theory and Paradigms', and my MA dissertation: 'Neoclassical Realism: The Worst of Both Worlds', would be contained to the introduction and first section of the paper subtitled 'Why Ripsman et al. Are Committed to Grand Theory' and would be of a descriptive nature only. The purpose of my MA dissertation was to demonstrate that the latest version of Ripsman et al.'s neoclassical realism, type III

neoclassical realism, failed to mark an advance on either classical realism or neorealism. The purpose of my paper herein is to investigate why Ripsman et al. remain committed to grand theory. Other than descriptive areas of similarity which inevitably emerge when working with the same theory, the analysis of type III neoclassical realism provided in this dissertation is original and does not replicate my previous work.

Pursuant to regulation 8.2 of the University of Kent's Academic Regulations for Research Courses of Study, 'Where Has Grand Theory Gone? On Barry Buzan, Grand Theory and the English School' and 'Caught Between Two Stools? Neoclassical Realism, Grand Theory and Paradigms', present 'wholly' my 'own work' and 'embody the results of' my 'research during the period of' my PhD registration at the University of Kent (University of Kent 2021, p. 6). I have, in a likewise pursuant manner to regulation 8.2, identified 'any material which' may be read as similar to work previously 'accepted for the award of an academic qualification at' the University of Kent (University of Kent 2021, p. 6).

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List of Abbreviations and Acronyms

IR: international relations

EJIR: European Journal of International Relations

BISA: British International Studies Association

ISA: International Studies Association

APSA: American Political Science Association

Introduction

Why Do Theorists Move Away From Grand Theory and Why Do They Remain Committed to Grand Theory?

I matured in a time when scholars in IR relied on “grand theory” for their sense of what the field was for, and about, and “great books” set forth grand theory for successive generations. I have always believed IR makes no sense as a claimant discipline or pedagogical undertaking in the absence of grand theorising ... As IR grows ever shaggier, its foundations ever shakier, its reason for being ever more shadowy, we need grand theory; we need great books to give shape to what we do’ (Onuf 2017, p. 1).

Introduction to the Research

Grand theory is either understood as a particularly useful or a deeply problematic form of theory in the discipline of international relations (IR). For some, grand theories provide useful “maps” (Mearsheimer and Walt 2013, p. 435), “big pictures” (Snyder 2013, p. 558) or “frameworks” (Alder 2019, p. 9) which enable and facilitate the intellectual inquiry of IR. For others, grand theory either distracts attention from studying what matters in IR (e.g., Lake 2013, p. 568; Qin 2018, p. xx; Solomon and Steele 2017, p. 268) or it is understood to have so many problematic effects that it is not worth pursuing at all (e.g., Der Derian, 1995 p. 179; Neacsu 2009, p. 11; George 1994, p. ix; Bleiker 2017, p. 258). Although grand theory is a live and important issue in the discipline of IR, there is no agreement among either its proponents or critics on what constitutes a grand theory. When theorists in the discipline of IR argue that grand theory is either useful or problematic, they claim that different kinds of grand theory are either useful or problematic. I briefly illustrate how this is the case below.

Proponents of Grand Theory

According to Mearsheimer and Walt, ‘a body of grand theories – or what are sometimes called the “isms” – has long shaped the study of international politics’ the ‘most prominent’ of which ‘are constructivism, liberalism, Marxism and realism’ (2013, p. 428). For Mearsheimer and Walt, ‘Grand theories such as realism or liberalism purport to explain broad patterns of state behaviour’ (Mearsheimer and Walt 2013, p. 432), and provide the ‘mental maps’ required to navigate the ‘terrain’ of IR (2013, p. 435). Grand theories, like all ‘Social science theories are not universal’, however, because ‘they apply only to particular realms of activity or to specific time periods’ (Mearsheimer and Walt 2013, p. 432). For theorists such as Snyder (2013, p. 558), Lebow (2008, p. 114) and Buzan (2004, p. 10), however, grand theory is a broader approach which aims to account for and go beyond IR’s mainstream paradigms or “isms”. Snyder, for example, claims that ‘Grand-theorizing should not be confused with the shopworn battle of the “isms” (2013, p. 558). He claims that his grand theory ‘while labelled “liberal”, does not fit comfortably within the field’s big “isms” (Snyder 2013, p. 558). Rather, Snyder’s grand theory is said to ‘synthesize elements of realist structural theory, liberal commercial theory, and constructivist ideational theory’ (2013, p. 558). Grand theory for Buzan and Lebow, in addition, is also universal because it should apply to all realms of activity and to all time periods (Buzan 2010, p. 208; Lebow 2008, p. 38). Grand theory is useful for theorists such as Snyder because ‘it gives us a “big picture” of the world: a way in which it can be seen and understood, directing our attention toward certain features and properties and away from others’ (2013, p. 558). For Adler, on the other hand, grand theory is constituted by neither a paradigm/”isms” nor by a broader theory which incorporates and goes beyond paradigms. Adler claims that ‘past grand theories’ can be found in the work of figures such as ‘Morgenthau 1949’, ‘Deutsch 1963’ and ‘Haas 1964’ (2019, p. 9). Such grand theories were useful for Alder because they offered ‘new ways of framing IR’, ‘suggested new research programs, elicited new debates, and showed the way to theorize at

the middle level' (2019, p. 9). Although there is no agreement on what constitutes a grand theory among its proponents, grand theory is nevertheless viewed as a useful "map", a "big picture" or a "framework" through which research can take place.

Critics of Grand Theory

Lake, contrary to Mearsheimer and Walt (2013), argues that grand theory 'was an evil tyrant' (2013, p. 568). While 'International Relations has a long tradition of Great Debates ... grand theory— 'a particular kind of theory' (2013, p. 581)—and clashes between competing grand theories now appear to be on the wane' (Lake 2013, p. 567). For Lake, clashes between competing grand theories 'insidiously ... took the form of paradigm wars' which 'perverted the discipline and turned inquiry into contests of a quasi-religious belief in the power of one or another "ism" (2013, p. 568). In fact, debates between competing grand theories are understood to have 'resolved little, and continue to this day' (Lake 2013, p. 567). Debates of grand theory for Lake are an unwelcome and problematic distraction from the core task of developing and testing mid-level theory which 'today forms the basis for a more progressive and eclectic approach to International Relations' (2013, p. 568). For Schweller, however, 'none of the field's various "isms" represent a fully articulated theory of international relations' (2003, p. 313) let alone a 'grand theory of international relations' (2003, p. 311). Paradigms in IR are 'vaguely conceptualized "perspectives" or political philosophies rooted in and guided by beliefs about "what matters" and what requires explanation in international affairs' (Schweller 2003, p. 313). Grand theories by contrast are 'unifying' (Schweller 2003, p. 311) in that they 'specify necessary causes of international outcomes and national behaviour, not merely sufficient ones' (Schweller 2003, p. 311). Schweller admits that he is 'not even sure what such a theory would look like' (2003, p. 311), although it is certainly not a paradigm. Researchers should avoid the distraction of grand theory by developing 'competing theories' to offer explanations at different 'levels of

analysis' (Schweller 2003, p. 311). Grand theory for Bleiker, on the other hand, neither has to be a paradigm nor a unifying theory. A grand theory for Bleiker is any theory which provides 'an overarching explanation of the world' (2017, p. 230). By 'directing our attention toward certain features and properties and away from others' (Snyder 2013, p. 558), the effects of grand theory are problematic for Bleiker because they facilitate 'practices of exclusion' (2017, p. 258). While there is no agreement on what constitutes a grand theory among its critics, grand theory is considered to be problematic due to its effects and because it distracts attention from studying what matters in IR.

An Investigation of Grand Theory in IR

Because there is no agreement either among proponents or critics on what constitutes a grand theory in IR, it is difficult to adjudicate whether grand theory is useful or problematic in general. Theorists of IR think that different kinds of grand theory are either useful or problematic. If one is to ascertain whether grand theory is useful or problematic, investigations of particular grand theories are required. By examining particular grand theories, I can ascertain precisely what a theorist means by a grand theory, why a theorist thinks their grand theory is useful and whether a theorist's grand theory holds up to critical scrutiny. While sustained investigations of particular grand theories have—to the best of my knowledge—not been undertaken in IR, critiques of particular grand theories have already been made (e.g., Little 2009; Adler 2005). How do I propose to go beyond such critiques? I claim that there are two figures in the discipline of IR who have abandoned grand theory: Barry Buzan (2018) and Richard Ned Lebow (2018). By answering why Buzan and Lebow have abandoned their projects of grand theory, I can address why grand theory was viewed by two of its former proponents to be useful and why it turned out to be problematic. There is, in addition, one group of theorists who remain committed to grand theory: Ripsman, Taliaferro and Lobell (2016). By answering why Ripsman, Taliaferro and

Lobell have remained committed to grand theory, I can address why grand theory continues to be viewed as useful by some of its proponents. By answering why Buzan and Lebow have abandoned grand theory, and why Ripsman et al. have remained committed to grand theory, I aim to make a contribution as to whether grand theory can be viewed as useful or problematic in IR.

Structure of Dissertation

The dissertation is structured in three papers. Paper one is titled 'Where has Grand Theory Gone? On Barry Buzan, Grand Theory and The English School'. Paper one investigates why Buzan has abandoned grand theory. Paper two is titled 'International Relations Should Beware Grand Theory, Not Lebow's Greek Gifts'. Paper two investigates why Lebow has abandoned grand theory. Paper three is titled 'Caught Between Two Stools? Neoclassical Realism, Grand Theory and Paradigms'. Paper three examines why Ripsman et al. have remained committed to grand theory. Although I aim for my papers to collectively make a contribution as to whether grand theory can be viewed as useful or problematic in IR, each paper is stand-alone and self-contained. Buzan, Lebow and Ripsman et al.'s respective projects of grand theory are distinct. Buzan saw the methodological pluralism of the English School as an ideal basis from which to construct and develop a grand theory of IR (2004, p. 10). Lebow, borrowing from Plato, uses what he considers to be the primary motivations of the human psyche, spirit, appetite and reason, as the basis from which to construct a grand theory of IR and politics in general (2008, p. 33; 52). Ripsman et al., systematise neoclassical realism as a school of diverse theories to offer a grand theory of 'foreign policy and international politics' (2016, p. 2). The historical development of Buzan, Lebow and Ripsman et al.'s respective projects are also distinct. Buzan's project of grand theory, I claim, began in 1993 with *The Logic of Anarchy* in which he attempted to bring together 'Neoliberal and Neorealist approaches to the study of the international system' (Buzan et al. 1993, p. 78).

Lebow's project of grand theory began with *The Tragic Vision of Politics* (2003) in which he sought to develop 'a new ontology for social science' (Lebow 2008, p. ix). I claim that Ripsman et al.'s project of grand theory began with *Neoclassical Realism, The State and Foreign Policy* in which they crafted neoclassical realism as 'an appropriate paradigm to construct theories of foreign policy' (Lobell et al. 2009, p. 287). While my papers can collectively speak to a broader debate of grand theory in the discipline, the grand theoretical projects of Buzan, Lebow and Ripsman et al. are distinct and they do not converge at any point either in their historical development or more contemporaneously. My analysis of each grand theoretical project is therefore self-contained.

Discussion of the Main results or Conclusions of the Papers

The overall conclusion of this dissertation is that grand theory, while partly useful as a heuristic device in the cases of Buzan and Lebow, is a deeply problematic form of theory in IR. I claim that my findings bring into question the utility of grand theory claimed by its proponents, and strengthen the case made by its critics that it is problematic. I discuss the main results and conclusions of each paper below.

Paper One: 'Where Has Grand Theory Gone? On Barry Buzan, Grand Theory and the English School'

The central finding of paper one is that Buzan has abandoned grand theory because it was no longer necessary to achieve his theoretical aims. I demonstrate that while grand theory was useful as a heuristic device for Buzan to think about particular phenomena and their interaction, including the insights of multiple paradigms within a single theory was not relevant to his theoretical aims in *Global International Society* (2018). I illustrate that while Buzan relentlessly

pursued the unification of IR theory since *The Logic of Anarchy* (1993), he has now abandoned this key epistemological and meta-theoretical pursuit and replaced it with an ontological one to better understand the ‘composition’ of international society (Buzan and Schouenborg 2018, p. 5). I claim that grand theory in the case of Buzan is therefore only partly useful as a heuristic. I also conclude that Buzan may have been motivated to pursue grand theory in order to obtain prestige. If Buzan could successfully bring together IR theory into a single approach, he would have found what Lake terms ‘the holy grail’ (2011, p. 466). The holy grail for Lake is a single theory for all IR (2011, p. 466). If Buzan was the architect of an approach which found the holy grail of IR theory, he would gain the prestige afforded to such a theorist.

Paper Two: ‘International Relations Should Beware Grand Theory, Not Lebow’s Greek Gifts’

The central finding of paper two is that Lebow has abandoned grand theory because he was able to achieve his new theoretical aims without grand theory. I demonstrate that while grand theory was also useful as a heuristic device in the case of Lebow, he did not require a grand theory which can be used to examine all aspects of politics in *The Rise and Fall of Political Orders* (2018). I claim that Lebow’s key epistemological move in *A Cultural Theory of International Relations* (2008) is to examine ‘all aspects of international relations’ by accounting ‘for all existing paradigms of international relations’ via ideal types of spirit, appetite and reason (Lebow 2008, p. 114). Such an epistemological move allowed Lebow to generate universal propositions of order/disorder in IR, and of their consequences for state behaviour. I illustrate that Lebow has abandoned this key epistemological and meta-theoretical move in *The Rise and Fall of Political Orders* (2018). Instead, Lebow uses his ideal types to analyse the ‘construction, evolution, decline and reconstitution of orders’ (2018, p. 9) but not to examine all aspects of politics by unifying

existing theory. In contrast to his claims in *A Cultural Theory of International Relations* (2008), Lebow now claims that universal ‘propositions about order are all but impossible’ (2018, p. 9). In the case of Lebow, I claim that grand theory is only partly useful as a heuristic device. I conclude that Lebow may also have been motivated to pursue grand theory in order to obtain prestige. If Lebow could bring together existing paradigms within a single theory to explain all aspects of IR, he would have found what Hoffmann describes as the ‘masterkey’ (1977, p. 52) or ‘a general theory for the discipline’ (1959, p. 352). If Lebow could provide ‘a single key’ (Hoffmann 1955, p. 352), or a theory for all IR, he would obtain the prestige afforded to such a theorist.

Paper Three: ‘Caught Between Two Stools? Neoclassical Realism, Grand Theory and Paradigms’

The central finding of paper three is that Ripsman et al. have remained committed to their particular grand theory because they purposefully do not engage in the relevant philosophy of science which would demonstrate its problems. I claim that Ripsman et al. construct a “multiparadigmatic” (2016, p. 164) paradigm as a grand theory which is caught between two inequivalent stools: a grand theory of the type constructed by Snyder (2013), Lebow (2008) and Buzan (2004), and a grand theory understood as a paradigm. Ripsman et al. base their understanding of grand theory as a paradigm on the work of Mearsheimer and Walt (Mearsheimer and Walt 2013, pp. 427-457 cited in Ripsman et al. 2016, p. 7). By constructing a grand theory as a “multiparadigmatic” paradigm, I find that Ripsman et al.’s grand theory can perform the tasks of neither type of grand theory. I claim, consequently, that Ripsman et al. cannot offer an overall better theory of international politics compared to structural realism, liberalism and constructivism (2016, p. 1). I also illustrate how Ripsman et al. arrive at their

“multiparadigmatic” paradigm. I find that Ripsman et al. constructed a “multiparadigmatic” paradigm of foreign policy in their earlier text of *Neoclassical Realism, The State and Foreign Policy* (2009). By building on this earlier work, I claim that Ripsman et al. build on flawed foundations. Once elevated to a grand theory of international politics, Ripsman et al.’s “multiparadigmatic” paradigm becomes caught, and ultimately falls, between two different types of grand theory. In the case of Ripsman et al., I claim that grand theory is problematic. I also conclude that Ripsman et al. may have been motivated to pursue grand theory in order to gain prestige.

Demonstration That the papers Provide Critical Exposition of Existing Knowledge

My findings should give theorists in IR pause for thought. Despite constructing grand theories, two out of the three prominent proponents of grand theory I examine have subsequently abandoned it in relation to their projects. While grand theory was partly useful for Buzan and Lebow, it was irrelevant to the next phases of their respective projects. The final proponents of grand theory I examine do not revise their position on grand theory because they do not question the epistemological basis of their theory. Ripsman et al. purposefully do not engage with the relevant philosophy of science which would demonstrate the problem with their grand theory. By deliberately avoiding difficult questions of epistemology, Ripsman et al. remain committed to grand theory through intellectual dogma. I claim that my findings provide critical exposition of existing knowledge in two ways; one, by contributing to particular literatures on Buzan’s English School, Lebow’s cultural theory and neoclassical realism; two, by contributing to IR’s more general grand theory literature.

Critical Exposition of Particular Literatures

Buzan, Lebow and Ripsman et al. claim that their respective grand theories give rise to research programmes (Buzan 2004, p. 270; Lebow 2008, p. 121; Ripsman et al. 2016, p. 1). Buzan and Lebow's abandonment of grand theory has immediate implications for anyone engaged in their respective research programmes, and the problematic nature of Ripsman et al.'s grand theory also has immediate implications for anyone engaged in their research programme. In the cases of Buzan (2004) and Lebow (2008), their abandonment of grand theory does not necessarily mean that their followers must also abandon their grand theories as the bases for research programmes in IR. But, at the very least, it should make Buzan and Lebow's followers stop and think about why their grand theories have been abandoned, and it should make them question whether they should also abandon them. The problematic nature of Ripsman et al.'s (2016) grand theory also does not necessarily mean that theorists engaged with its research programme must abandon type III neoclassical realism. It does, however, raise the question of whether Ripsman et al.'s grand theory is any 'better' (Ripsman et al. 2016, p. 88) than paradigmatic alternatives such as structural realism, liberalism and constructivism. If not, as I argue, it is not clear why researchers should choose Ripsman et al.'s grand theory over existing alternatives.

Buzan's abandonment of grand theory has, however, greater implications than it does for followers of either Lebow or Ripsman et al. Buzan's grand theory was crafted 'to attempt a reconvening of the English school' (2004, p. xiii). For Buzan, while the English School was an 'underexploited' or 'an underutilized research resource' (2001, pp. 471-472), it lacked a 'discernible sense of direction' (2001, p. 471) and required a 'sharper intellectual focus' (2001, p. 479). Buzan claimed that the English School lacked 'any identifiable leading figure or core forum' (2001, p. 479), and that the English School was in 'danger' of 'fragmentation' which would 'lead to the cumulative opus of the English School becoming less than the sum of its parts' (2001, pp. 479-480). By developing the English School as a grand theory, the English School could gain

a discernible sense of direction, a sharper intellectual focus and a core forum (2001, pp. 472-488). As the architect of such a grand theory, Buzan could also become the English School's leading figure. Buzan went so far as to claim that English School's claim to 'theoretical standing' hinges on its importance as a self-conscious location for the practice of a methodologically pluralist approach to the study of international relations, and therefore as a potential site for grand theory' (2004, p. 25). If Buzan has abandoned grand theory, as I argue, where does this leave Buzan's reconvention and what does this mean for the English School? It certainly leaves Buzan's reconvention project, and his wing of the English School (2014, pp. 78-80), in need of a profound and serious rethink. If the purpose of Buzan's reconvention was to craft the English School as a grand theory, it is not clear how one should understand the contemporary significance of his reconvention or his wing of the English School in the absence of grand theory. When theorists make a contribution to Buzan's wing of the English School today, it is not entirely clear what it is they are contributing to. In the absence of grand theory, it is not apparent what Buzan's wing of the English School even is. Furthermore, if Buzan has abandoned grand theory, it could be argued that he has thoroughly distracted attention from studying what mattered to the traditional/classical English School such as normative theory. Smith argued in 1992, for example, that 'the dominance of positivism' resulted in 'International Relations' taking a 'bizarre detour; a detour during which the goal of general theory was to be achieved by value free social science; a detour when it was simply old-fashioned, and very academic, to introduce normative concerns into analysis' (1992, p. 489). By abandoning grand theory, it could be argued that Buzan has taken the English School on a 'bizarre detour' (1992, p. 489) in which normative analysis was eschewed for no real purpose. In fact, Dunne, following the publication of *From International to World Society* (Buzan 2004), warned against taking such a bizarre detour (2005, pp. 157-167). Dunne argued that while Buzan's social structural approach 'is remarkable both in its ambition and in its theoretical innovation' (2005, p. 159), he privileges 'analytical rigor ... over normative

evaluation and critique' (2005, p. 167). Dunne emphasised that the classical English School's 'understanding of international society and world society is intimately connected to what moral values and purposes we ascribe to social relations' (2005, p. 167), and he stressed that one cannot 'be agnostic about the moral purposes of international [and world] society [parenthesis added]' (2005, p. 167). As some critics of grand theory in the wider discipline argue (Lake 2013, p. 568; Schweller 2008, p. 311), grand theory may well have distracted attention from studying what mattered to the traditional/classical English School in the case of Buzan (2004). In the absence of grand theory, Buzan's reconvention project and his wing of the English School are somewhat bereft of purpose, whereas the wider English School may have a renewed sense of purpose in opposition to the problematic effects of his reconvention.

Critical Exposition of Grand Theory Literature

My findings bring into question the degree to which grand theory is useful and strengthen the case that it is problematic in IR. My first and second papers provide critical exposition of existing grand theory literature by questioning the degree to which grand theory is useful as a "map" (Mearsheimer and Walt 2013, p. 435), "big picture" (Snyder 2013, p. 558) or "framework" (Adler 2019, p. 9) to guide research. While grand theory is partly useful in the cases of Buzan and Lebow as a heuristic, it was ultimately unnecessary as a type of theory which aims to bring together existing theory. Buzan was able to generate knowledge of IR in *Global International Society* (2018) without such a grand theory. Lebow was also able to generate knowledge of political order/disorder in *The Rise and Fall of Political Orders* (2018) without such a grand theory. While perceived as a particularly useful form of theory by theorists such as Mearsheimer and Walt (2013), Snyder (2013) and Adler (2019), grand theory is only partly useful in the cases of Buzan (2004) and Lebow (2008).

My third paper provides critical exposition of existing grand theory literature by demonstrating that grand theory is problematic in a manner distinct to that already identified. While grand theory is considered by its critics as either directing attention away from what matters (e.g., Lake 2013) or problematic due to its effects (e.g., Der Derian 1995), nobody—to the best of my knowledge—has argued that grand theory is problematic because some grand theorists do not question the bases of their theories. While theorists such as Hamati-Ataya, for example, argue that a grand theory cannot be achieved for epistemological reasons (Hamati-Ataya in Kaplan 2014, p. 24), this is a distinct argument from claiming that some grand theorists are not reflexive about issues of epistemology. Ripsman et al. are not just mistaken in their understanding of the philosophy of science, they *purposefully* do not engage with issues of the philosophy of science. Ripsman et al. retain their commitment to grand theory through dogma.

An Account of How the Papers Extend the Forefront of the Discipline

My papers extend the forefront of the discipline in three ways; one, by illustrating that the ambition to develop grand theory continues to be a major feature of IR; two, that my findings open new avenues of inquiry into grand theory than those currently pursued by either its proponents or critics; three, that the motivation to develop grand theory may be to obtain prestige in the discipline. I address each point in turn.

Grand Theory Is a Major Feature of IR

My three cases demonstrate that the ambition to develop grand theory continues to be a significant feature of IR. Although Buzan (2004) and Lebow (2008) abandoned their projects of grand theory, there were nevertheless motivated to develop grand theories. Buzan (2004) and Lebow (2008) developed grand theories because; one, they each sought to establish a single and

universally applicable theory of IR; two, because they may have sought the prestige that such a theory would confer among those who value what it offers. The fact that two of the field's most prominent theorists developed grand theories illustrates that grand theory is a significant and major feature of the discipline. In contrast to Buzan (2004) and Lebow (2008), Ripsman et al. (2016) remain committed to grand theory. Ripsman et al. constructed a grand theory to offer what they claim is a superior theory of international politics compared to paradigmatic alternatives (structural realism, liberalism and constructivism), and to arguably gain prestige by presenting what they hoped would be recognised as a leading paradigm of IR. The ambition to develop grand theory continues to be a major feature of IR because of its professed explanatory power, and because of the arguable prestige that it can confer.

New Avenues of Inquiry Into Grand Theory

My findings open new avenues of inquiry into grand theory than those currently pursued by its proponents such as Mearsheimer and Walt (2013), and by its critics such as Lake (2013) and Der Derian (1995). In the cases of Buzan (2004) and Lebow (2008), I claimed that grand theory was only in part useful as a heuristic. It may be the case with other grand theories that while they provide “maps” (Mearsheimer and Walt 2013, p. 435), “big pictures” (Snyder 2013, p. 558) or “frameworks” (Alder 2019, p. 9), they do so only as heuristic devices. A new avenue of inquiry is opened to investigate, for example, whether Snyder (2013) has abandoned grand theory and whether he continues to use grand theory in the form of a heuristic device. In the case of Ripsman et al. (2016), I found that grand theory is retained because Ripsman et al. did not question the epistemological basis of their theory. It could also be the case that the theorists from whom Ripsman et al. gained their understanding of grand theory, Mearsheimer and Walt (2013), also remain committed to grand theory because they too do not question the basis of grand theory.

My findings and conclusions extend the forefront of the discipline by opening two new avenues of inquiry into grand theory.

The Prestige of Grand Theory in IR

I argue, in the cases of Buzan (2004) and Lebow (2008), that they may have been motivated to construct grand theories to obtain a particular kind of prestige. The prestige Buzan and Lebow sought, I argue, is that of a theorist who had discovered ‘the holy grail of a universal theory of international politics’ (Lake 2011, p. 466). Buzan and Lebow did not seek to emulate the prestige of figures such as Waltz, Wendt or Keohane because they were already recognised as two of the discipline’s most significant figures. Buzan and Lebow sought the prestige that a theorist would obtain if they became the leading figure of the field. By constructing grand theories as universal theories of international politics, I claim that Lebow and Buzan sought to become the leading figures of IR. Ripsman, Taliaferro and Lobell, in distinction, although significant figures in IR—particularly in the study of foreign policy—were not leading names of the field prior to the publication of *Neoclassical Realist Theory of International Politics* (2016). Ripsman et al., rather than seeking the prestige bestowed upon IR’s leading figure, sought the prestige conferred to one of the discipline’s leading figures. I claim, that by constructing a grand theory as a “multiparadigmatic” paradigm, Ripsman et al. sought to occupy an analogous position to IR’s leading figures by becoming synonymous with what they hoped would become a leading paradigm in IR. Ripsman et al. describe structural realism, liberalism and constructivism as the ‘leading approaches’ of the field to which they claim their approach is ‘analogous’ (2016, p. 9).

Identifying prestige as a potential motivation to develop grand theory extends the forefront of the discipline because it suggests that grand theory in general could be partly understood as a desire to obtain prestige. Despite the problems with grand theory, theorists may nevertheless continue to propose and construct grand theories to obtain prestige in IR (and the study of

politics in the case of Lebow). Although Buzan (2004) and Lebow (2008) abandon grand theory, and Ripsman et al. only remain committed to grand through dogma, such findings may have no impact on a theorist's decision to construct a grand theory if they think they can obtain prestige. Ripsman et al., despite the significant problem with their grand theory, are synonymous with what is being increasingly recognised as a leading approach in IR. Neoclassical realism's strongest critics even admit that it has attracted an impressive number of adherents (Narizny 2017, p. 186). Grand theories are not simply constructed for the benefit of a discipline, they are also arguably constructed for the benefit of their author/s.

Where Has Grand Theory Gone? On Barry Buzan, Grand Theory and the English School

Abstract

Barry Buzan is a prominent Professor Emeritus of International Relations. In a career spanning more than three decades, Buzan has made substantial contributions to the English School, security studies, non-Western IR theory and the international relations of China. The purpose of this paper is to address, in particular, why Buzan has abandoned grand theory. I claim that Buzan constructed a grand theory in *From International to World Society: English School Theory and the Social Structure of Globalisation* (2004) which he has now abandoned in *Global International Society: A New Framework for Analysis* (2018). I argue that Buzan has abandoned grand theory because he has advanced his work to the extent that he no longer requires grand theory to achieve his theoretical aims. The paper contributes to wider debates of grand theory in the discipline by questioning the degree to which it is useful in conducting research. It also brings into question the nature of Buzan's contribution to the English School.

Key Words

Grand theory, Barry Buzan, English School, global international society, reconvention, prestige

Introduction

Barry Buzan is a prominent Professor Emeritus of International Relations (IR) at the London School of Economics and Political Science (LSE). Buzan ‘has written, co-authored or edited over twenty-five books, written or co-authored nearly one hundred and fifty articles and chapters, and lectured, broadcast or presented papers in twenty countries’ (LSE 2021). While Buzan is perhaps most known for his work on the English School (2000; 2001; 2004; 2010; 2018), such work has been influenced by the development of his other projects. Other than for his work on the English School, Buzan is known for his work on security studies, in particular, the Copenhagen School with which he is also synonymous. *People, States and Fear: The National Security Problem in International Relations* (1983) is cited in over two thousand texts (Google Scholar 2021), and his work with Ole Waever and Jaap de Wilde, *Security: A New Framework of Analysis* (1998), is cited in over twelve thousand (Google Scholar 2021). More contemporaneously, Buzan has worked with Amitav Acharya on non-Western IR theory (2007; 2010) while maintaining a particular interest in the international relations of China (Buzan 2010a; Buzan and Cox 2013; Buzan 2014a). Buzan’s recent work with George Lawson, *The Global Transformation: History, Modernity and the Making of International Relations* (2015), won the ISA’s Francesco Guicciardini Prize for best book in historical international relations (ISA 2021). In 2016, Buzan won BISA’s Distinguished Contribution Prize for making a substantial contribution to the discipline of IR and its prestige globally (BISA 2021). Buzan is certainly a leading and significant figure in the discipline of IR.

The purpose of this paper is to address why Buzan has abandoned his project of grand theory (2001, pp. 31-33; Buzan and Little 2001, pp. 480-488; 2004, p. 10; 2010, p. 208; 2014, p. 23). I claim that Buzan constructed a grand theory in *From International to World Society: English School Theory and the Social Structure of Globalisation* (2004), and that he has now abandoned his project of grand theory in *Global International Society: A New Framework for Analysis*

(2018). The question of why Buzan has abandoned grand theory is important to answer because it will contribute to a general debate of grand theory in the wider discipline. Although grand theory is either understood as useful to conduct research (e.g., Mearsheimer and Walt 2013, pp. 428-435) or problematic in IR (e.g., Lake 2013, p. 568), I claim that grand theory was only partly useful as a heuristic device and arguably became problematic in the case of Buzan. The question of why Buzan has abandoned grand theory is also important to answer because it brings into question the nature of Buzan's contribution to the English School.

A grand theory for Buzan is 'a holistic' and 'synthesising approach' (2004, p. 10) in 'which many of the fragmentations and diversities of IR' can be brought together in a single 'framework' (2001, p. 480). One purpose of grand theory for Buzan is to reintegrate 'the fragmented world of IR theory' (Buzan 2001, p. 488). For Buzan, 'IR prefers fragmentation into the anarchy of self-governing and paradigm-warring islands of theory rather than integration into the imperial or federative archipelago of theoretically pluralist grand theory' (2001, p. 31). The paradigm wars have 'narrowed down the terms of the debate' (2001, p. 31) in IR by staging the discipline 'as choices amongst irreconcilable opposites' leaving IR 'divided' and 'directionless' (Buzan 2001, p. 32). By bringing together 'the diversities of IR' into a grand theory, Buzan hopes to bring unity and direction to the study of IR. A second purpose of grand theory for Buzan is to provide a universally applicable theory of IR throughout time and space. For Buzan, a grand theory is a 'grand theory in the sense that it can be used to structure world historical accounts covering all of recorded history and all of the imaginable future' (2010, p. 208). A grand theory for Buzan is a single and universally applicable theory of IR which successfully captures the insights of IR's paradigms. The key epistemological claim of grand theory for Buzan is that it unifies existing theory into a single approach which in turn can be used to explain all aspects of history and the future. Grand theory is a single theory for all IR.

Buzan clarifies that *From International to World Society* ‘has deeper roots in my earlier attempts to link English school ideas to American IR theory ... and in my world historical writings with Richard Little, which point strongly towards the English school as an excellent site for developing grand theory’ (2004, p. xiii). Buzan’s earlier works of *International Systems in World History* (Buzan and Little 200) and *The Logic of Anarchy* (Buzan et al. 1993) are significant texts in the development of Buzan’s grand theory to which I will return later in the paper.

I argue that Buzan has abandoned grand theory because he has progressed his work to the extent that he no longer requires grand theory. I demonstrate how this is the case in five key steps. Firstly, I show how Buzan constructed a grand theory in *From International to World Society* (2004) by illustrating how he crafted his ‘social structural approach’ (2004, p. 25) to provide the basis of a single and universally applicable theory of IR. Secondly, I examine how Buzan arrived at his grand theory by examining his other key texts such as *International Systems in World History* (2000). I examine how Buzan arrived at his grand theory for two reasons; one, to illustrate how *From International to World Society* (2004) was the culmination of Buzan’s construction of grand theory to which he was committed from 1993; two, to demonstrate, that the purpose of Buzan’s project, throughout all stages of its development, was to arrive at a theory which could provide the basis of a single and universally applicable theory of IR. Thirdly, I illustrate that the purpose of Buzan’s social structural approach in *Global International Society* (2018) is to theorise the ‘composition’ of what Buzan terms “global international society” (Buzan and Schouenborg 2018, pp. 1-5). I claim that the purpose of Buzan’s social structural approach in *Global International Society* (2018) is not to bring together ‘the diversities of IR’ (2001, p. 480) into a single framework, nor is it an effort to contribute to a universally applicable theory of IR across time and space. In *Global International Society* (2018), Buzan does not continue the epistemological and meta-theoretical pursuit to unify existing theory. Buzan’s new theoretical aims are more ontological in nature for which the meta-theoretical pursuit of grand theory is not

required. Fourthly, I trace the disappearance of grand theory from Buzan's texts to demonstrate that grand theory is not something Buzan has temporarily put to one side. Finally, I illustrate that Buzan has abandoned grand theory because he only requires it as a heuristic device to make what are predominantly ontological claims about the composition of global international society.

The paper is structured in three sections. Section one addresses how Buzan constructed his social structural approach as a grand theory, and how Buzan arrived at his grand theory. Section two examines how Buzan abandoned grand theory, and it traces when grand theory began to disappear from his texts. In section three, I examine how Buzan's social structural approach provides a heuristic device.

Buzan's Grand Theory

Buzan presents his social structural approach as a 'triad' (2004, p. 133; 159) comprising three distinct domains. Each domain refers to a particular group of societies: interhuman, interstate and transnational societies (see figure 1 on the following page). Interstate societies for Buzan are 'about the (degree of) institutionalisation of shared interest and identity among states' (2004, p. xvii). Such societies range from those that are more pluralist 'with a relatively low degree of shared norms, rules and institutions' (Buzan 2004, p. xvii), to those that are more solidarist: societies 'with a relatively high degree of shared norms, rules and institutions' (Buzan 2004, p. xviii). Buzan's domain of interstate societies is a revision of the classical English School's concepts of international system and international society (Buzan 2004, p. 109; 133). Transnational societies are 'composed of non-state collective actors' (Buzan 2004, p. xviii) while interhuman societies are 'based on interactions amongst individual human beings' (Buzan 2004, p. xvii). Buzan's transnational and interhuman domains are a revision of the classical English School's concept of world society (Buzan 2004, p. 109; 133). The more particular societies of which each domain is comprised give rise to distinct social structures held together by 'binding forces' of 'coercion,

calculation and belief’ (Buzan 2004, p. 132). Interstate and transnational societies are ‘second-order’ in that their ‘members are not individual human beings, but durable collectivities of humans possessed of identities and actor qualities that are more than the sum of their parts’ (Buzan 2004, p. xviii). Interhuman societies are ‘first-order’ in that their members are human beings (Buzan 2004, p. xvii). Each domain, and its particular societies, emphasise the significant

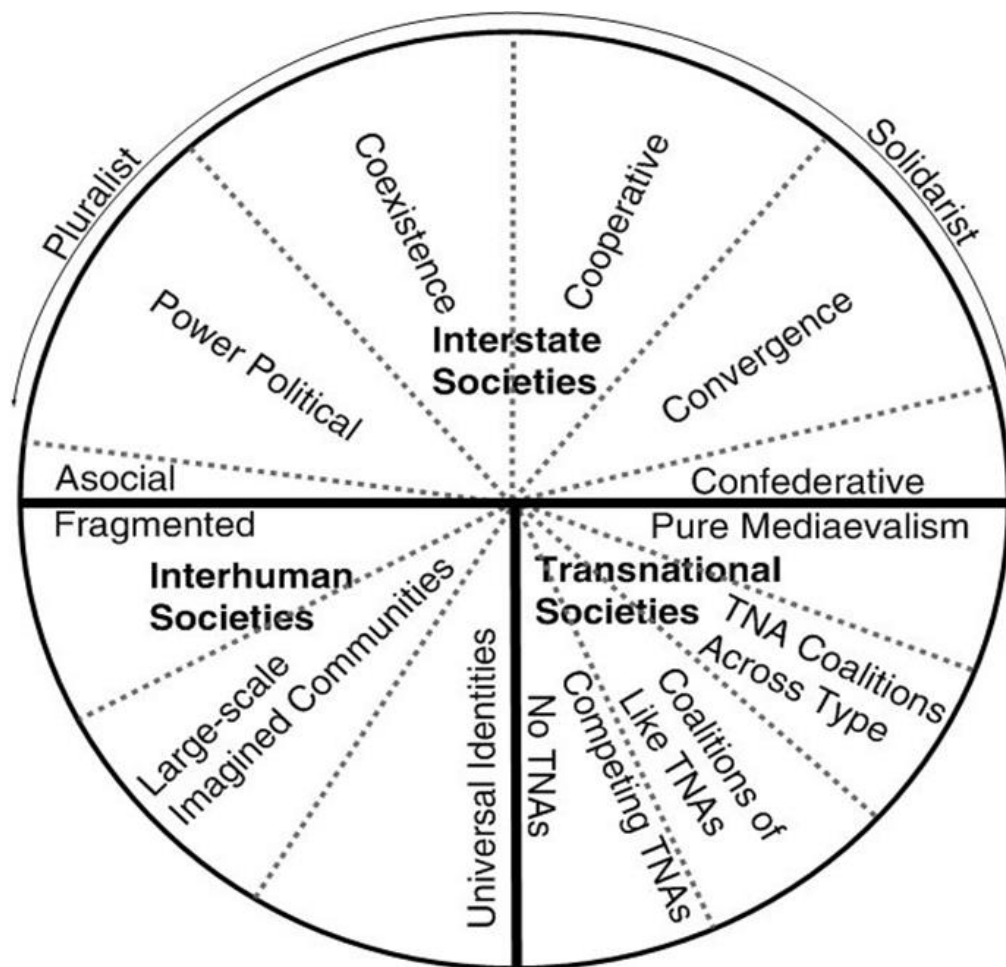


Figure 1: Buzan’s Social Structural Approach (Buzan 2004, p. 159)

role of ‘primary institutions’ which differ from one society to another (Buzan 2004, p. 190). Buzan acknowledges that ‘Exactly what the primary institutions of any given international society are is a matter for close empirical enquiry’ (2004, p. 190). Primary institutions not only help to describe a particular society, but they also help to explain how it operates. For example, in an

interstate society of coexistence, ‘diplomacy, great power management’ and ‘international law’ are primary institutions which help to explain how states avoid war (Buzan 2004, p. 191). Geography, understood as territoriality, is also a significant factor (Buzan 2004, p. 218). In interstate societies, for example, the role of geography is important to understand ‘the relationship between the global and sub-global levels’, especially where the relationship is ‘antagonistic’ (Buzan 2004, p. 218). An example of such an antagonistic relationship is the tension between ‘Western interstate society on the one hand, and the more Westphalian pluralist norms of global interstate society on the other’ (Buzan 2004, p. 218).

Buzan clarifies that his social structural approach is ‘about finding sets of analytical constructs with which to describe and theorise about what goes on in the world, and in that sense it is a positivist approach’ (2004, p. 14), one through which ‘social formations involving the three types of unit are always expected to be present in international systems to some degree’ (2004, p. 134). By acknowledging ‘that all three elements always operate simultaneously’, Buzan aims to challenge ‘the assumption ... that realist, liberal and Marxist approaches to IR theory are incommensurable’ (2004, p. 10). Realism and liberalism find expression in Buzan’s classification of interstate societies (Buzan 2004, p. 160), and Marxism via Buzan’s classification of transnational societies (Buzan 2004, p. 7). Realism, for example, occupies a role in Buzan’s interstate society of coexistence. By emphasising ‘the realist side of Grotius’ (Buzan 2004, p. 160), Buzan aims to understand an interstate society of coexistence by reference to its peace management system achieved, for example, via ‘core institutions’ of ‘sovereignty’ and ‘great power management’ (Buzan 2004, p. 160). When propelled inductively through history, Buzan’s social structural approach—in principle—provides an analytical structure to world history through which a succession of particular international societies can be identified, described, and explained. Buzan claims that ‘There are almost no limits for this type of social structural theory’ because ‘It provides a basis for grand theory in the sense that it can be used to structure world

historical accounts covering all of recorded history and all of the imaginable future' (2010, p. 208). Buzan aims to establish the basis of a single and universally applicable theory of IR in which the insights of multiple paradigms find expression. Buzan makes a meta-theoretical move to unify existing theory into a single approach which in turn can be used to explain all aspects of history and—so he claims—the future. Buzan concludes *From International to World Society* by claiming 'that the English school does indeed have the potential for grand theory that I suspected at the beginning' (2004, p. 270).

Importantly, however, Buzan's social structural approach presents an embryonic theoretical apparatus, 'an opening rather than a closing', about which 'more (probably much more) needs to be said' (2004, p. 268). This is to say that Buzan aims for his grand theory to establish a research programme through which his grand theory can unify the existing theories of IR's particular sub-disciplines, and which can be used as a guide to orient research in those sub-disciplines. Buzan's intention is clear:

'there is scope for an English school research programme that takes the particular qualities and characteristics of second-order societies as its subject, the pluralist-solidarist spectrum as its basic benchmark, and primary institutions as its principal object of investigation' (2004, p. 270).

Such a research programme 'would take systematically into account the role of socio-political geography, the inter-play among the interstate, interhuman and transnational domains, and the effect of binding forces' (2004, p. 270). By examining the interplay of interstate, interhuman and transnational societies within particular sub-disciplines, such as international security studies (Buzan 2015), Buzan can unite existing theories of those sub-disciplines and thereby provide a single theory of them. By considering the role of geography, and by identifying the primary institutions of selected second order societies, their position on the pluralist-solidarist spectrum and how they are held together by binding forces, researchers can also use Buzan's grand theory as a guide to orient research within particular sub-disciplines. Buzan's construction of grand

theory in *From International to World Society* (2004) provides the basis for grand theory through which unification within IR's various sub-disciplines is possible. If Buzan can provide a single theory of each IR sub-discipline via the domains of his grand theory, he can also bring together his theories of those sub-disciplines within his grand theory. Although Buzan constructs the basis for grand theory in *From International to World Society* (2004), the full unification of IR can only be achieved via the research programme embedded in his grand theory.

How Buzan Arrived at Grand Theory

From International to World Society (2004) is the culmination of Buzan's construction of grand theory which began with *The Logic of Anarchy* in 1993. Buzan began his construction of grand theory with realism and ended it with the English School. I argue, that throughout all stages of its development, Buzan's construction of grand theory has been about; one, bringing together 'the fragmentations and diversities of IR' into a single framework (Buzan 2001, p. 480) to bring unity and direction to IR; two, providing a framework through which all systems/societies can be identified and explained through world history (2010, p. 208). Because I have begun with Buzan's construction of grand theory in *From International to World Society* (2004), I work backwards through Buzan's key texts to illustrate how he arrived at his particular construction of grand theory.

Buzan, in an article titled 'Why International Relations has Failed as an Intellectual Project and What to do About it', asks how the English School can 'bring more coherence to IR theory when its "international society" approach can be seen as just another competitor in the paradigm wars?' (Buzan and Little 2001, p. 35). The English School's concept of international system is said to be 'broadly parallel to mainstream neorealism and uses structural modes of explanation and a positivist methodology' (Buzan and Little 2001, p. 36). While, for Buzan, 'parallels can be drawn between' the concept of international society 'and regime theory, it [international society]

has a constitutive rather than merely instrumental implications, using agency-based models of explanation, and hermeneutic methodology [parenthesis added]' (Buzan and Little 2001, p. 36). The English School's concept of world society, on the other hand, is said to be 'similar to transnationalism, but carries a much more foundational link to normative political theory and critical methodology' (Buzan and Little 2001, p. 37). By therefore 'acknowledging multiple rather than competing paradigms' (Buzan and Little 2001, p. 37), the ES can help bring coherence to IR theory by subsuming existing approaches into a single and distinct theoretical framework. An English School framework marked by its 'explicitly theoretically pluralist position', and by its refusal to accept the discipline as a 'war between incommensurable approaches' (Buzan and Little 2001, p. 37). In 2001, Buzan's project of grand theory was not simply about bringing together the mainstream paradigms of realism, liberalism and Marxism, it was also about embracing 'all meta-theoretical positions in Habermas's triad of cognitive interests (hermeneutics, positivism and critical theory)' (Guzzini 2001, p. 498).

Buzan's intent to develop the English School into a grand theory marked by an attempted 'synthesis' of existing approaches also found expression in Buzan's earlier monograph with Little, *International Systems in World History* (2000, p. 415). The approach developed in *International Systems in World History* (Buzan and Little 2000) is said to have 'been heavily influenced by our contact with the English School' (Buzan and Little 2000, p. 415). In particular, Buzan 'endeavoured to make progress' on 'a synthesis between the American and English (School) approaches to systems thinking' by 'associating the former with the military-political sector and the latter with the socio-cultural sector' (Buzan and Little 2000, p. 415). For Buzan and Little: 'A central feature of our project, therefore, is to find ways of drawing together the mode of systems thinking associated most closely with Waltz, to the mode of systems thinking most closely associated with the English School' (2000, p. 415).

While Waltz is therefore said to provide ‘the starting point’ for Buzan and Little’s theoretical framework, the English School nevertheless played a significant role in the development of their work (2000, p. 415). Buzan and Little also pursued ‘the synthesis of IR theory and history’ that he Little and Jones ‘had begun in the earlier book’: *The Logic of Anarchy* (1993) (Buzan and Little, 2000, p. vii). For Buzan, ‘such a synthesis’ showed ‘great potential for a novel approach to IR’ (Buzan and Little 2000, p. vii), one which would set ‘down a future research agenda’ (Buzan and Little 2000, p. 415).

The Logic of Anarchy (Buzan et al. 1993), while pursuing the synthesis of IR theory and world history, also sought a synthesis distinct from that of *International Systems in World History* (Buzan and Little 2000). Structural realism was said to provide the optimum ‘basis for synthesising the Neoliberal and Neorealist approaches to the study of the international system’ (Buzan et al. 1993, p. 78) rather than the English School. In an article titled ‘Structural Realism and Regime Theory Meet the English School’ published in the same year, however, Buzan argued that the ‘fitting together of the English School, structural realism and regime theory’ was said ‘to contain no substantial drawbacks’ (Buzan 1993, p. 352), and to offer ‘many advantages in constructing a coherent theoretical foundation for a wide-ranging and policy-relevant research agenda’ (Buzan 1993, p. 352). By working backwards beginning with *From International to World Society* (Buzan 2004), through to *International Systems in World History* (Buzan and Little 2000), *The Logic of Anarchy* (Buzan et al. 1993) and to smaller contributions such as ‘Structural Realism and Regime Theory Meet the English School’ (Buzan 1993), it is clear that Buzan has been engaged in an effort to construct a grand theory in which the fragmentations and diversities of IR can be brought together to explain world history since 1993.

Buzan’s project of grand theory can, however, be traced back to his encounters with Kal Holsti as an undergraduate student. Buzan states that Holsti’s ‘initial inspiration’ as his teacher made

such ‘a difference’ to his life that the work of Holsti stimulated and guided his own (Buzan 2016, p. ix). Most importantly for Buzan:

‘Kal showed his students that Realism was a good place to start building an understanding of international relations, but he left room for the idea that history matters, and that Realism may not be such a good place to end up’ (Buzan 2016, p. ix).

Such a view for Buzan leads to Holsti’s ‘second main engagement, which was the “classical” or “English school” tradition’ (2016a, p. viii). Holsti’s engagement with the English School was said to reflect his ‘pluralist outlook, and his commitment to the importance of history’ (Buzan 2016, p. viii). Buzan began his project of grand theory by examining how structural realism can provide the basis of a synthesis with neoliberal and other neorealist approaches (Buzan et al. 1993, p. 78) alongside consideration of a similar synthesis of the English School with structural realism and regime theory (Buzan 1993, p. 352). Structural Realism was the ‘starting point’ for Buzan and Little’s theoretical framework in *International Systems in World History* with which a synthesis with the English School was later developed (Buzan and Little 2000, p. 415). Buzan’s social structural approach retains a realist position via a power-political interstate-society (2004, pp. 159-160), and includes the insights of other paradigms via further classifications of interstate and transnational societies (2004, p. 159). The theoretical frameworks of *From International to World Society* (Buzan 2004), *International Systems in World History* (Buzan and Little 2000) and *The Logic of Anarchy* (Buzan et al. 1993) are each designed to be applied to world history (Buzan 2010, p. 208; Buzan and Little 2000, p. vii).

For Holsti, theoretical fragmentation in the discipline meant that the construction of a single framework in which the diversities of IR could be brought together is a difficult, if not an impossible, task. Holsti claims that it is:

‘difficult to organize a coherent debate, much less a dialogue leading to a constructive synthesis or to the emergence of a “super-paradigm” that will once again authoritatively guide inquiry, help

organize research agendas, be substantively accurate, and to provide criteria for developing reading lists for undergraduate and graduate studies' (Holsti 1985, p. 5).

By taking a 'methodologically pluralist rejection of the view that paradigms in IR are incommensurable' (2004, p. 3), by attempting to offer a 'framework within which many of the fragmentations and diversities of IR can be synthesized' (Buzan 2001, p. 480) and by 'acknowledging multiple rather than competing paradigms' (Buzan and Little 2001, p. 37) within a single theoretical framework, Buzan is attempting to create a 'constructive synthesis' of IR's paradigms leading to a "super paradigm" (Holsti 1985, p. 5). A "super-paradigm" which begins with realism but ends with a 'theoretically pluralist grand theory' (Buzan and Little 2001, p. 31), one which he hopes will 'authoritatively guide inquiry' (Holsti 1985, p. 5). Buzan has arguably been committed to grand theory since he was an undergraduate student. Buzan's abandonment of grand theory is therefore all the more significant. Why, despite the influence of his mentor, and why, despite pursuing a project of grand theory since 1993, has Buzan now abandoned grand theory? I address how Buzan has abandoned grand theory in the next section.

How Buzan Has Abandoned Grand Theory

In *Global International Society* (2018), Buzan and Schouenborg maintain 'primary institutions' and Buzan's 'domains' of 'interstate, transnational and interhuman' societies, and 'add' to their approach 'a differentiation theory from sociology' (p. 15). Buzan and Schouenborg also continue to use 'Wendt's (1999) ideas about binding forces that hold social structure together' (2018, p. 15). The central aim of Buzan and Schouenborg is to address 'the composition of' global international society (2018, p. 5) which they claim is a 'particular social structure' (p. 226). Buzan's new theoretical aims are primarily ontological in nature. His new theoretical aims do not include an epistemological or metatheoretical pursuit to unite existing theory. Buzan and

Schouenborg state that ‘What exactly is meant by phrases such “international society at the global level” or “global international society (GIS)” ... is far from clear’ (2018, p. 1). A global international society can be understood, however, as a regional international society which reaches ‘global scale’ (Buzan and Schouenborg 2018, p. 1). Buzan and Schouenborg give the example of the ‘ES’s international society expansion story, whereby what started as a European social form expands to global scale’ (2018, p. 1). Buzan’s primary object of investigation in *Global International Society* (2018) is not how existing theories can be brought together, it is global international society.

The approach offered by Buzan and Schouenborg gives rise to four models of global international society and its historical development: the ‘Like-Units Model’ (2018, p. 75), ‘The Regions/Subglobal Model’ (2018, p. 96), ‘The Hierarchy/Privilege Model’ (2018, p. 123) and ‘The Functional Differentiation Model’ (2018, p. 162). Buzan and Schouenborg ‘have chosen to integrate the three domains (interstate, transnational and interhuman) into all four models’ (2018, p. 225). The like-units model ‘focuses on the interstate domain’ (Buzan and Schouenborg 2018, p. 75), the regions/subglobal model ‘is largely located in the interstate domain, with some spillover on matters of identity into the interhuman one’ (Buzan and Schouenborg 2018, p. 96), the hierarchy/privilege model is positioned ‘not only in the interstate domain, but also in the transnational and interhuman ones’ and the functional differentiation model, although still located in the interstate domain, ‘opens up to the transnational and interhuman domains much more than the other three models’ (Buzan and Schouenborg 2018, p. 162). Each domain for Buzan and Schouenborg identifies the phenomena with which each model is engaged to better understand the composition of global international society. Buzan does not use his social structural approach to continue his pursuit of unifying existing theory. Instead, Buzan and Schouenborg replace such an epistemological and metatheoretical task with an ontological one. In fact, Buzan and Schouenborg are pre-occupied with Buzan’s interstate domain on ontological

grounds. Buzan and Schouenborg acknowledge that ‘World Society ... has been a kind of silent passenger on the theoretical journey in this book’ (2018, p. 226). While ‘we have made mention of the concept in passing several times’, they have ‘not subjugated it to any sustained discussion’ (Buzan and Schouenborg 2018, p. 226). Buzan and Schouenborg are ‘honest’ in that their ‘neglect of world society’ is a ‘consequence of our conscious and direct pursuit of global international society and models for differentiating this particular social structure’ (2018, p. 230). Buzan’s interstate (or international society) domain takes analytical primacy and is partly supplemented and supported by his transnational and interhuman (or world society) domains in the construction of Buzan and Schouenborg’s four models. Buzan’s interstate domain takes precedence because the dominant units of global international societies are primarily states (Buzan and Schouenborg 2018, p. 75; 96 123; 162).

The task of Buzan’s social structural approach in *From International to World Society* (2004) was to provide the basis for a single and universally applicable theory of IR. The purpose of such a theory was to provide a sense of unity and direction to the discipline by incorporating the insights of multiple paradigms, and to provide an account of state behaviour through recorded history. Buzan’s construction of grand theory contained an embedded research programme through which the unification of IR theory within IR’s sub-disciplines could take place, and through which IR’s sub-disciplines could be eventually united within Buzan’s construction of grand theory. *Global International Society* (Buzan and Schouenborg 2018) is the next phase of Buzan’s project, but it neither develops the apparatus of Buzan’s grand theory constructed in *From International to World Society* (2004), nor does it contribute to the unification of IR theory. *Global International Society* (2018) also does not contribute to a universally applicable theory of IR. Because Buzan’s interstate (or international society) domain takes analytical primacy, the framework contained in *Global International Society* (2018) cannot be used to explain ‘world historical accounts covering all of recorded history and all of the imaginable future’

(Buzan 2010, p. 208). According to Buzan, all three domains are required (Buzan 2010, p. 208). In addition, Buzan and Schouenborg do not present *Global International Society* (2018) as developing the theoretical apparatus of a grand theory, as attempting to unite existing theory, or as developing a theory universally applicable to world history. In fact, the terms “grand theory” and “research program/me” do not appear once in *Global International Society* (2018). There is no evidence that Buzan and Schouenborg intend to do anything other than understand the composition of global international society.

When Did Grand Theory Begin to Disappear?

Although I argue that Buzan has abandoned grand theory in *Global International Society* (2018), I claim that such an abandonment is the culmination of a gradual lack of engagement with grand theory since 2014. Buzan is not temporarily putting grand theory to one side. Buzan was engaged with a project of grand theory from *The Logic of Anarchy* (1993) through to *International Systems in World History* (2000), *From International to World Society* (2004), ‘The English School as a New Systems Theory of World Politics’ (2010) and to *An Introduction to the English School of International Relations* (2014). In *An Introduction to the English School of International Relations* (2014), Buzan claims that ‘The English School never accepted the argument about incommensurable paradigms’ which unnecessarily ‘separated liberal, realist and Marxian approaches to IR’ (2014, p. 23). Buzan is clear that ‘In its search for *raison de système*, it [the English School] always retained its potential as a site for synthesizing grand theory [parenthesis added]’ (2014, p. 23). Following the publication of *An Introduction to the English School of International Relations* (2014), I argue that while Buzan did not abandon grand theory until 2018, there is a traceable lack of engagement with grand theory in his texts between *An Introduction to the English School of International Relations* (2014) and *Global International Society* (2018).

In 2015, Buzan maintains a commitment to grand theory but does not use the term “grand theory” in his work. Buzan argues that ‘International society seen as a social structure of institutions can serve as the general framing for’ international security studies (2015, p. 129). Buzan’s general frame ‘shares ground’, for example, with ‘neorealism’ and ‘neoliberalism’ by being able to see ‘a world of enemies and rivals’ but also one of ‘shared interests and joint gains’ (2015, p. 129). Buzan is clear that the ‘ES incorporates both the realist and liberal framings’ (2015, p. 129) to the study of international security. Although Buzan applies the grand theory he constructed in *From International to World Society* (2004) as a means of studying international security, he neither uses the term “grand theory” to do so nor does he elaborate on what the findings of his work mean for the development of his grand theory. Buzan’s application of his grand theory to the particular sub-discipline of international security studies is consistent with the broader research programme he established in *From International to World Society* (2004). Upon intended application of his grand theory, it is curious that he makes no reference to his original research programme and how the application of his grand theory to international security studies develops it. While Buzan has not abandoned grand theory in 2015, he is neither presenting his work as grand theory nor does he present his work as contributing to a grand theory.

In a 2017 article with Albert titled ‘On the Subject Matter of International Relations’, Buzan differentiates between two different kinds of theory: “social theory” or “*Sozialtheorie*” and “theory of society” or “*Gesellschaftstheorie*” (Buzan and Albert 2017, p. 915). For Buzan, the former refers to the ‘fundamental characteristics’ and ‘assumptions’ which make up ‘sociality’ via a ‘macro-scale’ framework (Buzan and Albert 2017, p. 915). The latter is ‘only interested in parts of social theory that contribute to understanding society’ (Buzan and Albert 2017, p. 915). A social theory ‘does not, however, refer to the issue of whether we are talking about “grand theory”, it offers ‘a heuristic, but not a clear-cut definition of what is definitely “in” and what is definitely

“out” (Buzan and Albert 2017, p. 914). Buzan’s social structural approach (2004) does not offer ‘a clear-cut definition of what is definitely “in” and what is definitely “out” (Buzan and Albert 2017, p. 914) of any analysis. It offers a ‘Macro-scale’ framework (Buzan and Albert 2017, p. 915) from which any social structure can be explained through world history. Buzan’s four models of global international society, for example, use only ‘parts’ (Buzan and Albert 2017, p. 915) of Buzan’s social structural approach to theorise the composition and historical development of global international society. Buzan’s social structural approach in *Global International Society* (2018) is a “theory of society” and a ‘heuristic’ whereas his models of global international society are “theories of society” (Buzan and Albert 2017, p. 915). Buzan has shifted from a meta-theoretical claim to bring together the diversities of IR into a single theory, to a heuristic which can be used to identify relevant phenomena and their interaction when analysing particular social structures such as global international society. Buzan moves from a meta-theoretical and epistemological task to an ontological one. Buzan uses his construction of grand theory as a heuristic device, but he is no longer trying to unify existing theory. Buzan was trying to unify existing theory up until 2015. Buzan was trying to unify, for example, neoliberal and neorealist approaches to international security studies via his grand theory (if not the entire sub-discipline of international security studies). The task of unifying existing IR theory is abandoned in *Global International Society* (2018) and replaced with a heuristic. Buzan has relentlessly pursued the unification of IR theory from 1993 (*The Logic of Anarchy*) to 2015 which he has now abandoned in *Global International Society* (2018). Although Buzan was still engaged with the task of grand theory in 2015, he began his retreat from grand theory by neither presenting his work as grand theory nor as contributing to a grand theory. I claim that in 2017 Buzan moved away from grand theory by claiming that macro-scale frameworks constituted heuristics rather than grand theories. I argue that Buzan’s social structural approach (2004) constitutes a macro-scale framework which he and Schouenborg use as a heuristic device in *Global International*

Society (2018) to inform the analysis of global international society as a particular social structure. In what follows, I extend my analysis of how Buzan and Schouenborg use Buzan's grand theory as a heuristic device.

Why Buzan Has Abandoned Grand Theory

Buzan has abandoned grand theory because it is no longer relevant to achieve his theoretical aims. It is not necessary for Buzan to pursue the unification of IR theory to address 'the composition of' global international society (2018, p. 5) as a 'particular social structure' (2018, p. 226). Buzan does not require the grand theory element of his previous work in *Global International Society* (2018). This is not to say, however, that elements of Buzan's construction of grand theory were not useful. Without Buzan's initial construction of grand theory in *From International to World Society* (2004), Buzan and Schouenborg would not have been able to use Buzan's domains as a basis from which to fashion their four models of global international society. I argue that while Buzan's social structural approach was useful as a heuristic device to identify phenomena of relevance when constructing their four models, Buzan and Schouenborg did not require grand theory to achieve their aims in *Global International Society* (2018). I examine how Buzan and Schouenborg used Buzan's domain of interstate society to form their like-units model. I examine Buzan and Schouenborg's like-units model because it privileges Buzan's interstate domain.

Buzan and Schouenborg's like-units model is directly informed by Buzan's domain of interstate societies. It privileges 'the interstate domain in which the assumption is of a political world composed of modern, sovereign, legally equal, territorial states that are functionally alike in terms of what tasks they perform and how they are internally structured' (2018, p. 75). There are, however, 'shared values, norms and rules amongst the like-units that express themselves as the primary institutions of GIS' which 'compose the structure of GIS and play significant roles in

constituting both states themselves as legitimate members of international society and the rules defining legitimate behaviour' (2018, p. 76). Primary institutions 'include', for example, 'the classical ones of sovereignty, territoriality, diplomacy and international law, and also nationalism, human equality, development and the market' (Buzan and Schouenborg 2018, p. 76). As discussed in the previous section, Buzan conceptualised interstate societies in *From International to World Society* (2004) as those which identify 'the (degree of) institutionalisation of shared interest and identity among states' in particular (2004, p. xvii). In addition, Buzan and Schouenborg's primary institutions were also conceptualised by Buzan in *From International to World Society* (2004). For example, sovereignty, territoriality, diplomacy, international law and the market are all primary institutions of an interstate society of co-existence (2004, p. 160; 191). Territoriality and diplomacy are primary institutions of a power political interstate society (2004, p. 159; 191). Nationalism is a primary institution of a cooperative interstate society (2004, p. 193) while human equality is a primary institution of an interstate society of convergence (2004, p. 195). In addition to being informed by Buzan's domain of interstate societies, Buzan and Schouenborg's like-units model fully incorporates Buzan's domain of interstate societies. For Buzan and Schouenborg, 'The LUM provides the basic framing for Buzan's (2004a: 159-60) spectrum of types of international society ranging from power political ... to convergence' (2018, p. 81). Buzan's 'spectrum' of interstate societies is useful not only to track the historical development of global international society, but also to evaluate 'whether GIS is getting stronger or weaker' (2018, p. 81).

Buzan and Schouenborg use Buzan's domain of interstate societies to identify phenomena of relevance, appropriate primary institutions, and the interaction of such phenomena and institutions, to track the historical development of global international society as a particular social structure. Buzan's social structural approach is only relevant to the extent that it can help Buzan and Schouenborg theorise 'the composition of' global international society (2018, p. 5) through

time. Buzan and Schoenberg do not use Buzan's domain of interstate societies to unite existing theory. The continued pursuit of grand theory simply is not necessary to achieve Buzan and Schouenborg's new theoretical aims.

Conclusion: What About Buzan's Reconvension?

Buzan has abandoned grand theory because he no longer requires grand theory to achieve his theoretical aims. Despite Buzan's arguable commitment to grand theory from his encounters with Kal Holsti as an undergraduate student, Buzan abandoned grand theory in *Global International Society* (2018). Although Buzan's initial construction of grand theory in *From International to World Society* (2004) was useful to identify phenomena and their interaction within particular social structures, it was not necessary for Buzan to continue his pursuit of grand theory. Buzan did not need to unify existing theory to achieve his aims in *Global International Society* (2018). Buzan's social structural approach presents a sophisticated construction of grand theory by one of the discipline's leading figures. Buzan's abandonment of grand theory, and his possible reasons for his doing so, should give any prospective grand theorist pause for thought. Buzan's abandonment of grand theory also suggests that grand theory is only partly useful as a heuristic device contrary to the claims of its proponents in the wider discipline (e.g., Mearsheimer and Walt 2013). The analysis of Buzan's grand theory herein questions the degree to which grand theory may be useful in conducting research. It also suggests that grand theory is problematic as its critics argue because a prominent proponent of grand theory and a leading figure of the discipline has abandoned it.

Buzan's abandonment of grand theory, in addition, furthers our understanding of Buzan's contribution to the English School. Buzan's contribution began as grand theory and progressed to the analysis of global international society as a particular social structure. This is, however, significant news for the English School. Buzan's construction of grand theory was partly designed

to ‘reconvene’ the English School (1999, p. 9). For Buzan, ‘although impressively active in terms of people writing within or about it’, the English School ‘displays no discernible sense of direction’ (1999, p. 2), and ‘had its designation as a school given to it by someone calling for its closure’ (Jones 1981 cited in Buzan 1999, p. 2). The time was ‘ripe’ for Buzan to develop ‘what might be called its multi-paradigm, or methodologically pluralist, approach to IR theory’ (1999, p. 2). While the English School ‘definitely needs some development of its theory to sort out unresolved contradictions and underdeveloped concepts’, it ‘offers the best available basis on which to synthesise quite a few of the main lines of IR theory, and thus to revive a “grand theory” project’ (1999, p. 11). In fact, Buzan states that the English School’s claim to ‘theoretical standing’ hinges on its importance as a self-conscious location for the practice of a methodologically pluralist approach to the study of international relations, and therefore as a potential site for grand theory’ (2004, p. 25). Grand theory for Buzan offered a way of giving the English School a sense of direction, and as a way of developing its theory when its scholarship had ‘atrophied’ (Buzan 1999, p. 2). Buzan claimed that grand theory could also give the English School an ‘improved sense of self-awareness’ as a ‘coherent framework within which many of the fragmentations and diversities of IR can be synthesised’, and by emphasising its ‘potential to short-circuit much of the rather fruitless dispute of the inter-paradigm debate by showing how the realist, rationalist and revolutionist/liberal strands can (and must) be integrated’ (1999, p. 11). Grand theory was imperative for Buzan not only theoretically but to give the English School a new sense of identity. If Buzan as the architect of the English School’s reconvention has abandoned grand theory as its central mission, it leaves Buzan’s wing of the English School without a clear theoretical direction and somewhat bereft of identity. Buzan’s wing of the English School, in the absence of grand theory, needs a profound and serious rethink. Furthermore, the English School is a major approach in IR of which Buzan’s wing is a significant contingent. By introducing a structural interpretation of English School theory, and by bringing together the

paradigms of realism, liberalism and Marxism, the English School partly spoke to American IR theory. In the absence of grand theory, the English School may no longer speak to American IR theory. While many within the English School would be delighted to jettison any relationship to American IR theory, Buzan's abandonment of grand theory partially impacts how the English School views its overall identity to the rest of the discipline. Some English School theorists may even view this as an opportunity to reassert the traditional or classical English School of Bull or Wight (e.g., Hurrell 2007).

A significant question, however, has been left unanswered and it is not a question that I am able to address in this paper. While Buzan clearly sought to address theoretical fragmentation in IR, and while he looked to identify and explain social structures through world history, what would Buzan gain by crafting such a single and universally applicable theory of the field? I claim that the answer could be prestige. If Buzan could gain a consensus that his social structural approach provided a single and universally applicable theory of IR, Buzan would have found 'the holy grail' (Lake 2011, p. 466). Although Buzan did not achieve a theory which brought together the diversities of IR (only some of its diversities), he nevertheless crafted a single and universally applicable theory which included the insights of IR's mainstream paradigms. Buzan accounted for the insights of realism, liberalism and Marxism and his approach can, in principle, be applied to world history in an inductive manner to explain a succession of social structures. While Buzan was certainly credited with making an impressive theoretical contribution to English School literature (e.g., Adler 2005), why didn't Buzan achieve the prestige one would expect for a theorist who found or at least constructed the holy grail of IR theory? I conclude the paper with this question for further research.

International Relations Should Beware Grand Theory, Not Lebow's Greek Gifts

Abstract

Richard Ned Lebow is a leading Professor of International Political Theory. He has made substantial contributions to fields diverse as political theory, political psychology, Greek literature and philosophy, political science and international relations. He has been the recipient of multiple awards including the International Studies Association's distinguished scholar award. The purpose of this paper is to address why Lebow has abandoned grand theory. I claim that Lebow constructed a grand theory in *A Cultural Theory of International Relations* (2008) which he has now abandoned in *The Rise and Fall of Political Orders* (2018). I argue that Lebow has abandoned grand theory because he has advanced his work to the point where he no longer requires grand theory. The paper contributes to a debate of grand theory in the wider discipline by bringing into question the utility of grand theory to conduct research in IR. The paper also furthers our understanding of Lebow as one of the discipline's most significant figures.

Key Words

Grand theory, Richard Ned Lebow, order, chimera, prestige

Introduction

Richard Ned Lebow is a leading Professor of International Political Theory at Kings College London and professor emeritus at Dartmouth College. In a career spanning more than five decades, Lebow has published well over ‘19 scholarly authored or co-authored books, 12 edited books’, and ‘more than two hundred author or co-author articles’ (Reich 2017, p. ix). With interests covering fields diverse as ‘political science, international relations, political psychology, political theory, and Greek literature and philosophy’ (Reich 2017, p. ix), Lebow has been able to ‘push forward the frontiers of several research programs’ (Reich 2017, p. xiv). In the study of politics and IR, Lebow has been the recipient of multiple awards. *The Tragic Vision of Politics* (2003), for example, ‘was awarded the Alexander L. George Award for the best book in political psychology’ (Reich 2017, p. xi) while ‘*A Cultural Theory of International Relations* (2008) won two awards: the Paul Schrodinger and Robert Jarvis Award ... for the best book in international relations and history and the Susan Strange Award of’ the British International Studies Association (BISA) (Reich 2017, p. xi). Lebow, ‘Long overdue’, also won ‘the Distinguished Scholar Award from the International Security Section of the’ International Studies Association (ISA) (Reich 2017, p. xiv) for such contributions as *A Cultural Theory of International Relations* (2008). Lebow is certainly a significant figure in IR—as well as other disciplines—and a ‘A Pioneer’ as many have claimed (Reich 2017).

While any investigation of Lebow’s work cannot do justice to the breadth of his published texts, the purpose of this paper is to address, in particular, why Lebow has abandoned his project of grand theory (2008, pp. 6; 33-34; 38; 120-121). I claim that Lebow pursued a project of grand theory in *A Cultural Theory of International Relations* (2008) which he has now abandoned in *The Rise and Fall of Political Orders* (2018). The question of why Lebow has abandoned grand theory is important to answer because it will contribute to a more general debate of grand theory in the discipline. Grand theory is either considered useful to conduct research in IR (e.g.,

Mearsheimer and Walt 2013, pp. 428-435; Onuf 2017, p.1) or it is considered problematic (e.g., Lake 2013, p. 568; Der Derian 1995, p. 179). By examining how and why Lebow abandoned grand theory, I claim that grand theory was only partly useful and arguably problematic in the case of Lebow. The question of why Lebow abandoned grand theory is also important to address because it furthers our understanding of one of IR's most important figures and their work.

Lebow's cultural theory (2008) is the second part of a project that began with *The Tragic Vision of Politics* (2003) and continued with *The Rise and Fall of Political Orders* (2018). *The Tragic Vision of Politics* (2003) sought to develop 'a new ontology for the social science(s) [parenthesis added]' (Lebow 2008, p. ix) while, in *A Cultural Theory of International Relations* (2008), Lebow attempts to build 'a theory of international relations embedded in a proto-theory of order' (p. ix). Lebow states that his project therefore has a 'double theoretical focus: order and international relations' (2008, p. 6), the agenda for which he presents as follows:

'I begin with the problem of order, and propose a framework for its study, but not a theory. This framework provides the scaffolding for a theory of international relations, the major part of which I construct in this volume. In a planned follow-on volume, I intend to use this theory and additional evidence to transform my framework of order into a theory of order, and use that to further develop my theory of international relations' (2008, p. 6).

Lebow asks 'Do we need another grand theory?' (2008, p. 33). Lebow's answer is that we need another grand theory in the social sciences because 'Social scientists have been working away at the problem of order for a long time', and none of them have been able to address 'Plato's paradox: if true knowledge is holistic, we need to know everything before we can know anything' (2008, p. 33). Although 'Scholars have worked from the bottom up - tackling small and more manageable pieces of the puzzle - and from the top down - in the form of grand theories in the tradition of Hegel and Marx', it is 'difficult to do' one 'in the absence of the knowledge generated by the other' (Lebow 2008, p. 33). Lebow proposes a project of grand theory which attempts to

overcome Plato's paradox by explaining 'the particular by reference to the general' (2008, p. 4). Lebow aims to explain the particular case of IR by reference to a general framework of order. In a follow-on volume, Lebow will use his particular theory of IR to construct a theory of order which, in turn, will inform the development of his IR theory. Lebow adopts a 'layered' (2008, p. 6) approach to grand theory building through which he will eventually arrive at both a theory of IR, and a theory of order for social science in which his theory of IR is embedded. It is clear that Lebow offers his 'theory of international relations as a special case of political order (2008, p. 4), and 'that Lebow's new book does not propose a grand theory by itself; this is the professed goal of his larger project' (Onuf 2009, p. 144). Indeed, *A Cultural Theory of International Relations* (2008) 'is the second in a series of three books of breathtaking ambition whose purpose is no less than to present a grand theory of politics' (Welch 2010, p. 446) applicable across the various domains of the social sciences.

While Lebow's cultural theory (2008) is not by itself a grand theory of politics for social science (that is the goal of his larger project), it is nevertheless presented by Lebow as a grand theory of IR. For Lebow, grand theories in general 'provide frameworks for conducting research and suggest propositions that are amenable to empirical research' (2008, p. 33), and they are 'distinguished by the generalizations' they make 'across cultures and epochs' (2008, p. 38). Grand theories, 'by necessity, deploy conceptions that arose in one cultural context to describe behaviour in others' in a 'universal' manner (Lebow 2008, pp. 38-41). Lebow elaborates on what he means by a grand theory in general by outlining how he constructs a grand theory in IR. Lebow presents criteria for a grand theory of IR. For Lebow, 'A general theory of international relations is a grand theory' (2008, p. 120). He claims that 'It [grand theory] should have something to say about all aspects of international relations, but not necessarily in the form of testable propositions [parenthesis added]' (Lebow 2008, p. 121). Grand theory should also 'establish a research

program, or at least the foundation for one' (Lebow 2008, p. 121). Lebow claims that his cultural theory meets his necessary criteria for grand theory:

I believe I meet these criteria. I propose a general framework for studying politics in terms of dynamic status hierarchies. I derive several theories from this framework, having to do with the rise of the state, the kinds of states that are most likely to be aggressive, the causes, character and frequency of cooperation and warfare, and the propensity of actors to seek or eschew risk' (2008, p. 121).

A grand theory for Lebow is a framework which can be used to examine all aspects of a particular domain, and a theory which gives shape and meaning to a research programme. A grand theory is a general theory in that it can be distinguished by the universal nature of the generalisations it makes across cultures (space) and epochs (time).

I argue that Lebow has abandoned grand theory because he has advanced his work to the extent that he no longer requires grand theory. I demonstrate how this is the case in three key steps. Firstly, I illustrate how Lebow constructed his grand theory in *A Cultural Theory of International Relations* (2008). I demonstrate how Lebow's cultural theory provides a framework which can be used to examine 'all aspects' of IR, and how it gives shape and meaning to a research programme. Secondly, I demonstrate that Lebow in *The Rise and Fall of Political Orders* (2018) does not present a grand theory of politics. In fact, Lebow does not use the term "grand theory" once in *The Rise and Fall of Political Orders* (2018). I illustrate that while Lebow identifies 'some general reasons for the construction, evolution, decline and reconstitution of orders', his theory of political order is not 'universal' (2018, p. 9). Thirdly, I demonstrate that while Lebow's construction of grand theory was useful as a heuristic device, Lebow did not require a theory which examines 'all aspects' of a particular domain, nor a theory which facilitates a research programme, to achieve his theoretical aims in *The Rise and Fall of Political Orders* (2018). Finally, I conclude my analysis by asking whether Lebow was correct to abandon grand theory. I

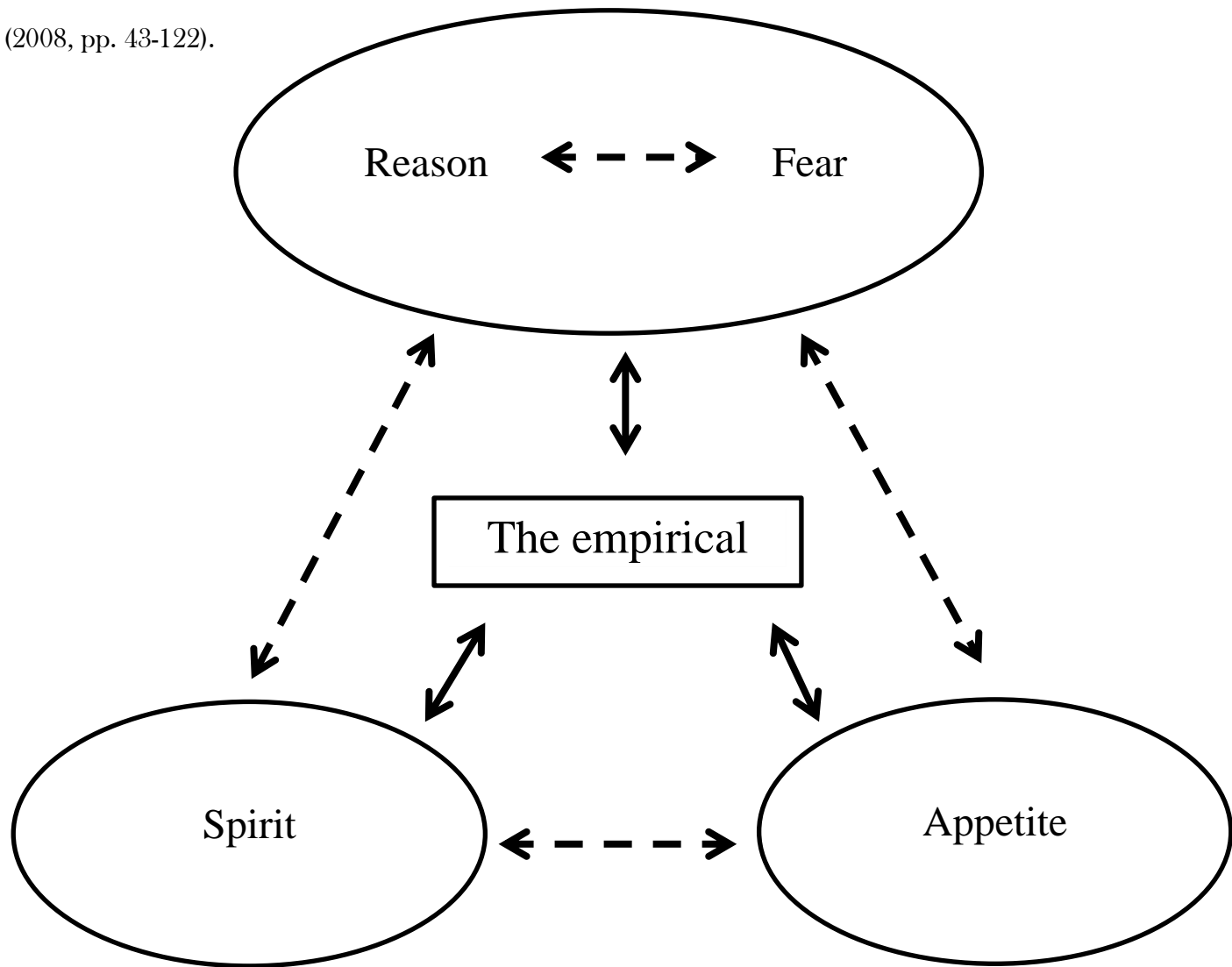
structure my analysis in two substantive sections. Section one addresses how Lebow constructed his cultural theory as a grand theory (2008), and how he moved away from his project of grand theory (2018). Section two examines how Lebow's construction of grand theory was useful as a heuristic device.

How Lebow Has Abandoned Grand Theory

Lebow opens *A Cultural Theory of International Relations* (2008) by stating that 'there are few general theories of international relations' (2008, p.1). Lebow establishes the theoretical apparatus of his grand or general theory by creating four Weberian ideal type worlds: spirit¹, appetite, fear and reason (2008, p. 26. See figure 2 on the following page). Each of Lebow's worlds are characterised by particular hierarchies based on respective principles of justice, and each give rise 'to distinctive forms of behaviour that have different implications for cooperation, conflict and risk-taking' (2008, p. 26). Borrowing from Plato, Lebow begins with the motivations of the human psyche: spirit, appetite and reason which are conceptually extrapolated from the individual to the local, regional and international levels of analysis via the analogy of an upward moving fractal (2008, p. 52). Fear operates in the same manner but is considered an emotion rather than a motivation. For Lebow, all four: 'Worlds qualify as ideal types according to Weber's first definition' because they are analytical constructs, useful to understand the behaviour of societies, but without direct correspondence to reality' (2008, p. 93). Reason, however, 'as an end in itself operates at another level of abstraction' by 'constraining spirit and appetite' to 'reshape

¹ Spirit is a crude translation of the Greek *thumos* which refers to a universal drive for self-esteem (Lebow 2018, p. 4). Lebow swapped the term spirit for *thumos* in *The Rise and Fall of Political Orders* (2018, p. 4). I will use the term spirit when referring to the ideal type as expressed in *A Cultural Theory of International Relations* (2008), and *thumos* when referring to the ideal type as expressed in *The Rise and Fall of Political Orders* (2018).

Figure 2: Lebow's Cultural Theory (2008, pp. 43-122).



and redirect them to enable a happier, ordered and more just life' (Lebow 2008, p. 94). Lebow restricts his analysis to three ideal type worlds (spirit, appetite and fear) as 'abstractions of societies that exist, or have existed', with reason working 'in the background as kind of ideal or platonic form' (2008, p. 94).

Lebow argues that his ideal types 'capture universal attributes of human nature that find expression in all cultures at all times, with the very important caveat that they are manifested and described in a wide variety of ways' (2008, p. 41). Lebow claims that 'appetite, spirit and reason have the potential to generate orders' (2008, p. 505) which he clarifies via the illustration of two triangles² (2008, pp. 510-511). One triangle 'has reason at its apex and appetite and spirit at its base' (Lebow 2008, p. 210). The second 'is an inverse triangle' which maintains appetite and spirit at its base but places 'a fear-based world at the bottom' (Lebow 2008, p. 510). Each triangle, Lebow claims, allow us 'to represent order; the closer to the reason apex any society resides, the more ordered it is' (2008, p. 510). The closer to fear any society resides, the potential for 'disorder' arises (Lebow 2008, p. 510). Although Lebow's ideal types find expression in a variety of different ways within particular societies, Lebow nevertheless establishes universal propositions to explain international order and disorder throughout world history. He found, for example, that 'Greece during the Peloponnesian War, the Mediterranean and Middle East during the Hellenistic age, Italy and Western Europe for much of the duration of the Roman Republic and medieval Europe were largely unordered' and closer to fear-based worlds because 'warfare was frequent, often unlimited in ends and means, [and because] cooperation was difficult and usually short-lived [parenthesis added]' (2008, p. 558). In contrast, 'fourth century Greece, eighteenth-century Europe and East-West relations during the last decades of the cold war' were moderately ordered (Lebow 2008, p. 558) because reason constrained 'and educated appetite and spirit' (Lebow 2008, p. 559). By examining, 'the ways in which all three motives found

² Figures 10.1 and 10.2 in *A Cultural Theory of International Relations* (2008, pp. 510-511).

expression' in societies throughout world history, and by demonstrating that 'all three motives are to varying degrees present' in real worlds (2008, p. 26), Lebow claims to examine the most significant aspects of IR throughout time. Lebow makes such a claim because the 'Conventional paradigms of politics and international relations are rooted in appetite' (Lebow 20018, p. 15). For Lebow, 'Liberalism and Marxism describe politics as driven by material interests, and realism acknowledges their primacy after security' (Lebow 2008, p. 15). Lebow can account for the leading paradigms of IR and the crucial aspects they cover in explaining behaviour. In fact, Lebow goes beyond such paradigms by illustrating how they work together with 'a paradigm of politics based on the spirit' (2008, p. 16) which 'has not been made the basis for any paradigm of politics and international relations' (Lebow 2008, p. 60). Lebow makes a key metatheoretical move to bring together the leading paradigms of the discipline to provide a single and universally applicable theory of IR. Such a move is directly comparable to that of Barry Buzan's in *From International to World Society: English School Theory and the Social Structure of Globalisation* (2004). Buzan also aimed to bring together the leading paradigms of IR via his social structural approach because he rejected the view that 'paradigms in IR are incommensurable' (2004, p. 3). Buzan also aimed to explain behaviour throughout world history in a universal manner (2010, p. 208). Lebow's construction of grand theory is metatheoretically analogous to existing efforts to develop grand theory in IR, and it can be situated within an existing precedent in the discipline to develop grand theory.

The core task for Lebow, however, is to 'chart the courses of multiple worlds over time' to 'tell us more about the paths - past and future - of worlds that interest us' by laying 'the groundwork for a common research agenda for scholars working in different paradigms' (2008, p.95). For Lebow, such a research programme 'would focus attention on the ways in which elements of their respective paradigms combine [within the context of Lebow's cultural theory] to shape the character and politics of a unit or system [parenthesis added]' (2008, p. 95). It is not

only for Lebow to take up the task of applying his cultural theory to world history but also for other researchers. By applying the theoretical apparatus of Lebow's grand theory through history in an inductive manner, researchers, over time, will be able to demonstrate the universal applicability of Lebow's cultural theory (2008). The particular findings of researchers may also contribute to the theoretical development of Lebow's cultural theory (2008).

A Theory of Political Order

In *The Rise and Fall of Political Orders* (2018), Lebow states that *The Tragic Vision of Politics* (2003) and *A Cultural Theory of International Relations* (2008) are 'components of a broader project that seeks to reframe our approach to international relations, and social science more generally' (2018, p. 2). *The Rise and Fall of Political Orders* presents a theory of order drawn from 'literature in political theory, history, comparative and international politics, ethology³, psychology and literature' (2018, p. 1). As such, the project is said to 'go beyond economics to sociology, political science, philosophy and history' (Lebow 2018, p. 5), and 'embed the study of political behaviour in psychology, history and philosophy in particular (Lebow 2018, p. 2). Furthermore, Lebow offers 'my book as an example of how to repair the rift between political science and political philosophy' to the extent that 'it is the kind of political theory that is political science' simultaneously (Lebow 2018, p. 5). Lebow thus not only presents a theory of political order which is interdisciplinary in construction but one which shows the futility of disciplinary distinctions, and by so doing seeks to reframe the manner through which we approach the social sciences in general.

Building on *A Cultural Theory of International Relations* (2008), Lebow maintains that his 'ideal types' of spirit/*thumos*, 'along with appetite and the emotion of fear, generate distinct logics

³ Ethology is the study of animal behaviour.

of conflict, cooperation and risk-taking, and give rise to different kinds of hierarchies' (2018, p. 153). Lebow also maintains that 'The relative importance of these three motives is a function of the degree to which reason restrains and educates *thumos* and appetite' (2018, p. 153). There is, however, a subtle but significant difference in how Lebow presents and understands the purpose of his ideal types. While Lebow maintains 'the case for the universal applicability of core concepts of my theory of political order, so long as they are applied with appropriate sensitivity to context' (2018, p. 40), Lebow only claims to provide 'some general reasons for the construction, evolution, decline, and reconstitution of orders and some of the dynamics associated with these processes' (2018, p. 9). In fact, Lebow claims that universal 'propositions about order are all but impossible' (2018, p. 9). Lebow's key metatheoretical move of bringing together existing theory within his ideal types to establish universal propositions or order/disorder has disappeared. Lebow neither aims nor claims to account for existing political theory in *The Rise and Fall of Political Orders* (2018). Instead, Lebow simply uses 'Weberian ideal-type descriptions of societies' to identify some 'general reasons' for order/disorder which 'serve as starting points for narratives that analyse specific societies' (2018, p. 9). This is a markedly different claim from that found in *A Cultural Theory of International Relations* (2008). The universal applicability of Lebow's ideal types in *A Cultural Theory of International Relations* (2008) was not to identify 'some general reasons for' and 'some of the dynamics associated with' (2018, p. 9) international order, it was to provide 'a general theory of international relations' (2008, p. 120) which covered 'all aspects' of the subject by accounting 'for all existing paradigms of international relations' (Lebow 2008, p. 114). Lebow's ideal types were understood to provide the basis for a universally applicable theory of the subject of IR to which Lebow went some length to illustrate via his cases. *The Rise and Fall of Political Orders* (2018) is the next phase of Lebow's project which was supposed to contain a grand theory of politics (2008, p. 33; Onuf 2009, p. 144; Welch 2010, p. 446), but Lebow has abandoned the key metatheoretical move that would enable him to

construct a grand theory of politics. Lebow does not provide a general theory of politics which covers all aspects of politics by accounting for all political theory (Lebow claimed to do this for IR). Instead, Lebow simply identifies some general reasons for the rise, decline and mutation of political orders which he illustrates via three cases: the US (2018, pp. 175-203), the UK (2018, pp. 244-273) and China (Lebow and Dimitrov 2018, pp. 274-303).

Lebow, furthermore, simply does not present *The Rise and Fall of Political Orders* (2018) as the continuation of his pursuit of grand theory. Lebow states that ‘My book builds on the epistemological foundations and substantive arguments of the two previous volumes – *The Tragic Vision of Politics* (2003) and *A Cultural Theory of International Relations* (2008)’ (2018, p. 1). The epistemological foundations upon which Lebow builds are those provided by Weberian ideal types, not grand theory (Lebow 2018, p. 2). Lebow builds on the foundation that his ideal types, while ‘not often encountered in practice’, nevertheless provide ‘a template for analysis’ (2018, p. 349). Lebow’s ideal types provide useful templates for analysis to examine specific societies without bringing together or accounting for existing theory in *The Rise and Fall of Political Orders* (2018). Lebow’s ideal types are maintained, but not to construct a grand theory of politics. Lebow is clear that he will ‘address the problem of international order in a follow-on study that will draw on the theory and findings of this book’ (2018, p. 17). Lebow continues a ‘layered’ (2008, p. 6) approach to theory building by explaining ‘the particular by reference to the general’ (2008, p. 4), but not for the purpose of developing a grand theory of politics. Lebow’s revised purpose is to develop a theory of political and international order.

*When Did Grand Theory Disappear From Lebow’s Texts?*²

Lebow clarifies that his epistemology in *The Rise and Fall of Political Orders* (2018) is further ‘elaborated in *Why Nations Fight* (2010b), *Forbidden Fruit: Counterfactual and International Relations* (2010c), and *Max Weber and International Relations* (2017)’ (2018, p. 2). By tracing

the disappearance of grand theory across Lebow's key epistemological texts, I argue that grand theory is not a form of theory to which Lebow will return. I claim that Lebow's abandonment of grand theory is the culmination of a sustained lack of engagement with grand theory from *Forbidden Fruit: Counterfactual and International Relations* (2010).

Lebow remains committed to grand theory in *Why Nations Fight* (2010b). Lebow claims, 'following his argument in *A Cultural Theory of International Relations* ... that most, if not all, foreign-policy behaviour can be reduced to three fundamental motives: fear, interest and honour' (2010b, p. 14) which give rise to four ideal types of 'spirit, appetite, reason and fear' (2010b, pp. 15-16). For Lebow, 'To understand the causes of war we need to start with motives and the foreign policy goals to which they lead' (2010b, p. 14). Consistent with his claim to provide grand theory in 2008, Lebow maintains that 'Existing theories of international relations are rooted in appetite (i.e. liberalism and Marxism) or fear (i.e. realism)' (2010b, p. 15). In *Why Nations Fight*, Lebow 'draw[s]out the implications of my [cultural] theory for warfare and use[s] the data set I have assembled to evaluate propositions derived from this understanding [parentheses added]' (2010b, p. 16). Lebow, in his first key epistemological text, is committed to the grand theory he constructed in *A Cultural Theory of International Relations* (2008) and contributes to the research programme it establishes by focusing attention on the ways in which elements of 'respective paradigms combine' (2008, p. 95) to explain the occurrence of war.

In *Forbidden Fruit: Counterfactual and International Relations* (2010c)⁴ and *Max Weber and International Relations* (2017), Lebow's key epistemological and metatheoretical claim to provide grand theory in relation to his cultural theory (2008) has disappeared. Lebow neither uses the term "grand theory", nor does he claim that his cultural theory (2008) brings together existing theory to explain all behaviour. In fact, '*A Cultural Theory of International Relations*' is

⁴ Although not published until 2010, Lebow finished *Why Nations Fight* in 2009. See page xi in Lebow, RN. 2010b. *Why Nations Fight: Past and Future Motivations For War*. Cambridge: Cambridge University Press.

only useful for Lebow because it illustrates ‘that conceptions of reason and risk taking are culturally determined, making the character of strategic logic situationally specific’ (2010c, p. 261). In *Max Weber and International Relations* (2017), Lebow similarly claims that ‘*A Cultural Theory of International Relations*’ is useful only because the explanations of behaviour it generates are ‘actor and culture specific’ (2017, p. 65). In *Forbidden Fruit: Counterfactual and International Relations* (2010c) and *Max Weber and International Relations* (2017), the key epistemological and metatheoretical claim to bring together existing theory for the purpose of explaining all behaviour has disappeared. Instead, Lebow emphasises the more particular claims his cultural theory (2008) can be used to make. In *Max Weber and International Relations* (2017), Lebow claims that ideal types are useful because ‘they allow us to explain aspects of cultures of interest to us – for example, ancient Egypt or modern China even though we have no direct historical relationship with them as we do with the Romans and ancient Greeks’ (2017, p. 56). While ideal types can be universally applied to explain aspects of particular cultures across epochs, they should not be used to explain all aspects of cultures across epochs. In other words, ideal types should be used to identify some aspects of politics which give rise to ‘some general reasons’ for the rise, decline and reconstitution of orders across societies (Lebow 2018, p. 9). They should not be used to unify existing approaches for the purpose of establishing a theory which covers ‘all aspects’ of politics and IR (Lebow 2008, p. 121). *Forbidden Fruit: Counterfactual and International Relations* (2010c) and *Max Weber and International Relations* (2017) present a subtle but significant shift in Lebow’s epistemological position. Lebow remained committed to the epistemological and metatheoretical claims of the grand theory he constructed in *A Cultural Theory of International Relations* (2008) in *Why Nations Fight* (2010b), which have disappeared in *Forbidden Fruit: Counterfactual and International Relations* (2010c) and *Max Weber and International Relations* (2017), and which were eventually abandoned in *The Rise and Fall of Political Orders* (2018). The epistemological claims Lebow makes in *The Rise*

and Fall of Political Orders (2018) are consistent with the epistemological claims he makes for his cultural theory (2008) in *Forbidden Fruit: Counterfactual and International Relations* (2010c) and *Max Weber and International Relations* (2017).

I am not arguing that Lebow, or anyone else, cannot use ideal types to establish a theory which covers all aspects of a particular subject matter or intellectual domain. I am also not arguing that Lebow's work is any way incoherent or contradictory (because it most certainly is not). I am claiming that Lebow ultimately abandoned the epistemological and metatheoretical pursuit of grand theory to pursue a more particular investigation of political order. Lebow's abandonment of grand theory, I claim, is the culmination of a sustained lack of engagement with grand theory from *Forbidden Fruit: Counterfactual and International Relations* (2010), and it is not something to which Lebow will later return. My claim is that Lebow's abandonment of grand theory as one of the discipline's most significant figures and one of its most prominent proponents is significant, and it is something which should make any proponent of grand theory question its utility. It is also something, I claim, which has implications for any theorist engaged with the research programme facilitated by Lebow's grand theory (2008, p. 95). Such researchers must also question whether they should abandon Lebow's grand theory in line with its author. My contention is not that there is anything wrong with Lebow's construction of grand theory *per se*, but that his abandonment of grand theory is curious and important for followers of Lebow and for the wider discipline of IR.

Why Lebow Has Abandoned Grand Theory

I argue that while Lebow's project of grand theory was useful as a heuristic device to identify key processes in understanding political order and disorder, Lebow has abandoned grand theory because he no longer requires it achieve his theoretical aims. In what follows, I demonstrate that while Lebow's project of grand theory was useful to identify such key processes which he

maintains in *The Rise and Fall of Political Orders* (2018), the more epistemological and metatheoretical pursuit of grand theory was not necessary.

Three years prior to the publication of *A Cultural Theory of International Relations* (2008), Lebow presented a paper⁵ to the American Political Science Association (APSA) originally titled 'Fear, interest and honour: outlines of a general theory of International Relations' in which he presented an embryonic cultural theory for the first time. Lebow claims that 'when not held in check by reason', spirit and appetite can 'lead to a rapid unravelling of order' (2005, p. 2) in which 'fear becomes paramount' through 'violence or warfare' (2005, p. 2). The process by which his ideal types explain order and disorder is identical to that expressed in *A Cultural Theory of International Relations* (Lebow 2008, pp. 510-511). When fear prevails disorder can arise but when spirit and appetite are held in check by reason order can be established (Lebow 2008, p. 10).

In *The Tragic Vision of Politics* (2003), Lebow hopes 'to show that by getting outside of our customary language and related concepts we can develop a new understanding of the nature and sources of cooperation and order, domestic and international' (2003, p. xiii). By examining the realist authors of Thucydides, Clausewitz and Morgenthau (Lebow 2003, p. 19), Lebow aims to enable 'lucid, defensible readings of these texts' to highlight the 'threat within the academic community' of realism posed by 'Kenneth Waltz and his neorealist disciples' (2003, p. 39). It is Lebow's contention that classical realism has become 'impoverished' (2003, p. xiii) due to the dominance posed by neorealism, and that by 'recapturing the perspectives, emotions and

⁵ The official name listed for the paper in APSA's archive is 'Fear, interest and honour: outlines of a general theory of International Relations', but the title on the paper submitted to the archive is 'Fear, interest and honour: a theory of International Relations'. See: Lebow, RN. 2005. Fear, interest and honour: outlines of a general theory of International Relations. Paper presented at the annual meeting of the American Political Science Association, 01 September, Washington.

language of Thucydides, Clausewitz and Morgenthau’, we can ‘enrich our understanding of politics’ (2003. P. xii-xiii). By investigating the texts of Morgenthau, Thucydides and Clausewitz, Lebow concludes that ‘for Morgenthau, as for Thucydides and Clausewitz, communities and the identities and norms they help to create and sustain are the most critical determinants of order [and potentially disorder if change occurs], at home and abroad [parenthesis added]’ (2003, p. 264). Communities, whether domestic or international, thus obtain order by establishing norms changes in which can lead to disorder (Lebow 2003, p. 264). This finds more explicit and particular expression as changes from reason to fear in *A Cultural Theory of International Relations* (Lebow 2008, p. 510-511). For example, Lebow argues that ‘World War I took place in an international society whose *nomos* had been rapidly breaking down’ due to violations of the limitations surrounding war (2008, p. 421). Lebow confirms that ‘when reason loses control of the spirit or appetite and actors no longer feel constrained by the limitations governing warfare ... fear quickly becomes the dominant motive [rather than reason] and provides further incentive for violating *nomos* [parenthesis added]’ resulting in a breakdown of order (2008, p. 428).

The continuities between each discussed text are ontological⁶. Although useful to identify key processes in order and disorder, Lebow’s project of grand theory as an epistemological and metatheoretical pursuit to bring together existing theory is not necessary. Lebow simply does not require grand theory to understand the dynamics of either international or political order/disorder. In fact, Lebow is clear that the ‘third volume of my trilogy fulfils the rash promise I made in *The Tragic Vision of Politics* (2003) to produce a theory of order’ (2018, p. 1). As stated previously, Lebow maintains that ‘*thumos* along with appetite and the emotion of fear, generate distinct logics of conflict, cooperation and risk-taking’ (2018, p. 2), and confirms ‘that the relative importance of the three motives is a function of the degree to which reason restrains

⁶ See also page 9 in Lebow, RN. 2018. *The Rise and Fall of Political Orders*. Cambridge: Cambridge University Press.

and educates *thumos* and appetite' (2018, p. 2). Fear 'rises in importance as reason loses control of either'⁷ (Lebow 2018, p. 2). Lebow claims that two principles of justice can be found throughout human societies: fairness and equality (2018, p. 77). Fairness is associated with a *thumos* based world and equality with an appetite-based world (Lebow 2018, p. 2). Fairness and equality are said to give rise to different hierarchies which characterise *thumos* and appetite-based worlds respectively (Lebow 2018, p. 2). Shifts in the principles of justice which underpin *thumos* or appetite-based worlds (Lebow 2018, p. 147), and discrepancies between the practice of a society and the principle which justifies an existing hierarchy can lead to disorder (Lebow 2018, p. 132, 147). In the case of *Thumos* based worlds, elites fail to honour rule packages and others worry for their ability to gain honour—self-esteem—or even security which results in a decline to fear from reason as cooperation fails and the likelihood of conflict and disorder increase (Lebow 2018, pp. 18, 31, 147-159). In the case of an appetite-based world, those at the top of society decide that they no longer need to cooperate with established rules and behave in an egregious manner (Lebow 2018, pp. 18, 31, 147-159). In fear, lower members of society seek to emulate the egregious practices of those at the top to restore equality which in turn can lead to disorder (Lebow 2018, pp.147-159).

The operation of appetite, spirit/*thumos*, reason and fear in *The Rise and Fall of Political Orders* (2018) is ontologically similar to that that expressed not only in *A Cultural Theory of International Relations* (2008), but in Lebow's earlier text of 'Fear, interest and honour: outlines of a general theory of International Relations' (2005) which in turn also finds a more general expression in *The Tragic Vision of Politics* (2003). When not held in check by reason,

⁷ See also a recording of Lebow in which he presents *The Rise and Fall of Political Orders* (2018) at the London School of Economics and Political Science. See: Lebow, RN. 2019. The Rise and Fall of Political Orders. Talk delivered to the London School of Economics and Political Science, 21 January, London, <http://www.lse.ac.uk/ideas/podcasts/the-rise-and-fall-of-political-orders>

spirit/*thumos* and appetite-based worlds may descend into fear and disorder. Grand theory, as an epistemological and metatheoretical pursuit to bring together existing theory, was not necessary for Lebow to analyse the ‘construction, evolution, decline and reconstitution of orders’ in *The Rise and Fall of Political Orders* (2018, p. 9). Lebow’s project of grand theory, beginning with *The Tragic Vision of Politics* (2003), was useful as a heuristic device to identify key processes in understanding political order and disorder but not as a form of theory which accounted for existing theory. Lebow does not require a theory which examines all aspect of politics, nor of political order, nor one which can be applied to world history in a universal manner. He also does not require a theory which facilitates a research programme. Grand theory was no longer necessary for Lebow.

Conclusion: Was Lebow Correct to Abandon Grand Theory?

Lebow has abandoned grand theory because he has advanced his work to the point where he no longer requires grand theory. Despite constructing and illustrating the utility of a grand theory in *A Cultural Theory of International Relations* (2008), Lebow abandoned grand theory in *The Rise and Fall of Political Orders* (2018). While Lebow’s project of grand theory was useful to identify key processes in order and disorder, Lebow did not require a theory which examined all aspects of politics or of political order, nor did he require a theory which facilitated a research programme. By engaging in such an analysis, I have also furthered our understanding of Lebow’s work by illustrating how he began with a project of grand theory (2008) which he later abandoned (2018). Lebow’s abandonment of grand theory, while unexpected given his commitment to grand theory, is not entirely without notice. Lebow was clear that his work ‘remains a work in progress’ and that he would ‘think further about my theory ... to introduce changes in the course of writing the follow-on volume’ (Lebow 2008, p. ix). Lebow’s abandonment of grand theory should make any budding grand theorist hesitate. Lebow’s cultural theory (2008) is one of the most elegant

grand theories in IR constructed by a 'Pioneer' (Reich 2017) of the field. Lebow's abandonment of grand theory brings into question the utility of grand theory in conducting research. *The Rise and Fall of Political Orders* (2018) is an impressive piece of research which makes contributes to several disciplines including IR without the aid of grand theory.

I argue that Lebow was correct to abandon grand theory because it is a chimera. Der Derian, for example, argues that 'there is the matter of scale: considering that the Athenian polis was composed of about 30,000 citizens ... that is a pretty small base from which to extrapolate and to project a grand theory' (2010, p. 483). Scepticism 'is warranted when Lebow regifts the Greeks for IR' (2010, p. 484) because he derives 'universal principles from particular historical contexts' (2010, p. 482). Der Derian identifies an epistemological problem with constructing a theory universally applicable across the history of IR based on concepts which applied at their inception to only 30, 000 citizens of the Athenian polis. Such a problem is exacerbated in the context of grand theory because 'grand theory' is marked by the universal nature of 'the generalizations it makes across cultures and epochs' (Lebow 2008, p. 38). While Lebow has illustrated how spirit/*thumos*, appetite and reason are relevant in understanding behaviour across cultures and epochs via his cases, it is not clear how the motivations he identifies are primary in the sense that all other human motivations are reducible to them. It may be the case that spirit/*thumos*, appetite and reason are the primary motivations of behaviour in the Athenian polis, but other human motivations may take precedence in different cultures and epochs. Rengger argues in addition, however, that 'claims about what "the Greeks thought" should be taken with a pinch of salt' (2010, p. 458). Rengger emphasises that 'they differed amongst themselves hugely about the relative importance of various different kinds of human motivation, certainly including (but also not limited to) appetite, spirit and reason and a whole host of other things' (2010, p. 458). For Rengger, it is not entirely clear in Lebow's account how appetite, spirit/*thumos* and reason are the primary motivations to understand behaviour in the epoch from which they were drawn, let

alone how they are the primary motivations to understand all aspects of IR through history. While it may be possible for Lebow to identify some general reasons for the rise, decline and reconstitution of orders across a limited number of societies, a universally applicable theory to understand all aspects of IR and politics is a chimera. IR should beware grand theory, not Lebow's Greek gifts.

Little, in a review of *A Cultural Theory of International Relations* (2008) titled 'Still on the Long Road to Theory'⁸ (2009), claims that while 'the book will have a very significant impact on the discipline', it will not 'generate a consensus about the future direction of the field' (2009, p. 115). The problem for Lebow is said to be that 'existing attempts to build a general theory are fundamentally flawed' (2009, p. 108) because the theories of 'realism, liberalism, Marxism and constructivism, which are, arguably, the only approaches in the contemporary field that can plausibly claim to have constructed general theories' do not 'make meaningful comparisons across the whole course of world history' (2008, p. 108). According to Little, Lebow:

'considers that he has developed an approach that overcomes the difficulties he identifies with other grand theories, and moreover, he argues that this approach transcends these theories in the sense that he is able to locate them within the more encompassing framework that he establishes in his book' (2009, p. 109).

For little, it is therefore clear that Lebow is attempting to 'provide a general theory on which the study of international relations can build' (2009, p. 108) by developing a framework which is said to be 'unequivocally applicable across space as well as across time' (2009, p. 109). Little argues,

⁸ Little's review of *A Cultural Theory of International Relations* (2008) was included in a *Millennium* forum along with the reviews of Osiander (2009) and Shilliam (2009). The forum has not been directly mentioned because Osiander and Shilliam's articles have been excluded from analysis. Osiander and Shilliam do not directly engage with grand theory in their comments to Lebow. See, for example, Lebow, RN. 2009a. Culture and International Relations: The Culture of International Relations. *Millennium* 38(1): 153-159.

however, 'that an agreed general theory anywhere in the social sciences is a chimera, and, as a consequence, the road to theory will not just be long but never ending' (2009, p. 115). Grand theory is a chimera for Little because it is difficult, if not impossible, for any theory to gain the consensus of a discipline. With reference to Hoffmann's article, 'International Relations: The Long Road to Theory' (1959), Little emphasises 'that the discipline is still no closer to establishing an agreed general theory' (2009, p. 108), as did Hoffmann in 1959 noting that 'Hans J. Morgenthau's realism and Morton A. Kaplan's systems theory, fell far short of this goal' (Little 2009, p. 108). In the case of Lebow's cultural theory, realists, for example, 'are very likely to challenge the way that Lebow interprets his evidence' (Little 2009, p. 115). Lebow argues, for example, 'that after the Second World War, the United States was preoccupied with its relative standing in the world, and it was, as a consequence, humiliated by the "loss" of China as well as the Soviet success in launching Sputnik' (Little 2009, p. 115). For realists, however, 'this underestimates the genuine fear that was experienced by American officials throughout the Cold War' (Little 2009, p. 115). While Lebow's cultural theory is 'quite breathtaking in its scope and ambition' (Little 2009, p. 108), an agreed upon theory for the discipline is a chimera because it is difficult, if not impossible, for any single theory to gain consensus.

Lebow was beguiled by a chimeric form of theory the pursuit of which he was certainly correct to abandon in the development of his project. Lebow was correct to abandon grand theory because grand theory itself presents a problem, not his ideal types. Lebow, however, continued his pursuit of grand theory in *Why Nations Fight* (2010) despite the critique of Richard Little (2009). Little's critique of grand theory is particularly notable because he supported Barry Buzan's development of grand theory (Buzan, Jones and Little 1993; Buzan and Little 2000; Buzan and Little 2001). Little is therefore in a particularly advantageous position to assess the merits (or lack thereof) of grand theory in IR. Moreover, Buzan's *From International to World Society* (2004), in which Buzan is clear to state the support of Little (p. xiii), is cited in *A Cultural*

Theory of International Relations (Buzan 2004 cited in Lebow 2008, p. 591). Lebow remained committed to grand theory in *Why Nations Fight* (2010) despite Little's apparent *volte-face* on grand theory of which he was aware. Of course, while Lebow is under no obligation to agree with Little, it does raise the question of whether there is anything else motivating Lebow to pursue grand theory other than its perceived benefits.

One question that has been left unanswered, however, is what Lebow hoped to achieve by constructing a framework which covered all aspects of IR and which could be applied to world history in a universal manner. The answer, I claim, may be prestige. If Lebow, to quote Little, could 'generate a consensus about the future direction of the field' (2009, p. 115) via his cultural theory (2008), Lebow would have found the 'masterkey' (Hoffmann 1977, p. 52) or the 'philosophers stone' (Hoffmann 1977, p. 52). While it is clear that Lebow did not construct a theory which covered all conceivable aspects of IR, he did craft a theory which accounted for IR's mainstream paradigms (realism, liberalism and Marxism). In addition, Lebow illustrated the universal applicability of his theory empirically via an examination of the ancient world (2008, p. 165) through to the contemporary (2008, p. 439). Lebow largely achieved what he set out to do in *A Cultural Theory of International Relations* (2008). Why, despite producing a sophisticated grand theory, did Lebow not gain the prestige one would expect for a theorist who found IR's masterkey or philosopher's stone? It is curious that Lebow did not obtain such prestige given the magnitude of the theory he created. I leave this question as one for further research.

Caught Between Two Stools? Neoclassical Realism, Grand Theory and Paradigms

Abstract

Ripsman, Taliaferro and Lobell are noted Professors of International Relations. They have each been published in leading journals such as *Security Studies*, and they have been the recipients of multiple awards and research grants. The purpose of this paper is to address why Ripsman et al. remain committed to grand theory. I claim that Ripsman et al. have constructed a grand theory as a “multiparadigmatic” paradigm in *Neoclassical Realist Theory of International Politics* (2016). I argue that Ripsman et al. remain committed to their grand theory because they do not engage in the relevant philosophy of science which would demonstrate its problems. The paper contributes to an ongoing debate of grand theory in the wider discipline by illustrating why theorists may remain committed to grand theory in general. The paper also contributes to current debates in neoclassical realist literature by informing how some neoclassical realists may choose to develop their projects of grand theory.

Key Words

Grand theory, Ripsman, Taliaferro, Lobell, neoclassical realism, philosophy of science, epistemology

Introduction

Norrin Ripsman, Jeffrey Taliaferro and Steven Lobell are noted Professors of International Relations. While Ripsman et al. are most known for their work on neoclassical realism (2009; 2012; 2016), they are also known for their work on foreign policy for which they have been repeatedly published in leading journals such as *Security Studies* (e.g., Taliaferro 2006; Lobell 2002; Ripsman 2001). Furthermore, Taliaferro's *Balancing Risks: Great Power Prevention in the Periphery* (2004) 'won the American Political Science Association's Robert L. Jarvis and Paul W. Schroeder Award for the Best Book in International History and Politics (Tufts 2021). Taliaferro's *Balancing Risks* (2004) uses a neoclassical realist framework (Ripsman et al 2016, p. 27) to examine why great powers engage in peripheral conflicts (Taliaferro 2004, p. 2). Lobell, in addition, is the principal investigator for an Office of Naval Research and Minerva Research Initiative project worth \$1, 179, 073 (Minerva Research Institute 2021). The project has already begun to see associated publications such as Lobell's 'Preventative military strike or preventative war?' (2021) for which he uses neoclassical realist theory to form his conception of power (Ripsman et al. 2016 cited in Lobell 2021, p. 2). Ripsman, Taliaferro and Lobell are certainly prominent figures in the discipline of IR.

The purpose of this paper is to address why Ripsman, Taliaferro and Lobell have remained committed to the grand theory they constructed in *Neoclassical Realist Theory of International Politics* (2016). The question of why Ripsman et al. have remained committed to their grand theory is important to answer because it will contribute to a debate of grand theory in the wider discipline. Grand theory is either understood as useful to give shape and meaning to the academic study of IR (e.g., Onuf 2017, p.1; Mearsheimer and Walt 2013, pp. 428-435) or it is understood as a highly problematic form of theory which the discipline would be better off abandoning (e.g., Qin 2018, pp. xx-xxi; Lake 2013, p. 568). By examining Ripsman et al.'s commitment to grand theory, I identify why theorists may continue to rely on grand theory in general to conduct

research in IR. My question is also important to answer because it may inform how some neoclassical realists develop their projects of grand theory. Kitchen, for example, ‘outlines the ways in which NCR could become “a grand theory of international politics that integrates system and unit-levels” (Kitchen in Sterling-Folkner 2021, p. 289) while Desmaele and Onea ‘offer socialization and grand strategy, respectively, as the basis for NCR as a grand theory’ (Sterling-Folkner 2021, p. 289).

For Ripsman et al., grand theory is a paradigm (2016, p. 7). Ripsman et al. place their discussion of neoclassical realism in ‘paradigmatic terms’ (2016, p. 7). Ripsman et al. argue that ‘Paradigmatic approaches’, such as neoclassical realism, ‘can help us understand the dynamics of international politics and its regularities in a holistic manner, rather than simply focusing on largely disconnected empirical results’ (2016, p. 18). In agreement with Mearsheimer and Walt (Mearsheimer and Walt 2013 cited in Ripsman et al., p. 7), Ripsman et al. argue that such ‘grand theory can help us guide our empirical research’ (2016, p. 8). Initially, Ripsman et al.’s claims are clear: grand theory is a paradigm, and they seek to construct neoclassical realism as a paradigm.

Ripsman et al. also argue, however, in partial agreement with Lake, ‘that while we too agree that paradigmatic rigidity ... is unhelpful, we should not throw the baby out with the bath water’ (2016, p. 8). Instead, a ‘multiparadigmatic’ (2016, p. 164) approach within a single paradigmatic framework is most beneficial. Ripsman et al. claim that ‘As long as it is not impervious to the insights generated by other paradigms—and, by its very nature, neoclassical realism, is not—a powerful paradigmatic approach can inform a useful policy-relevant empirical research agenda’ (2016, p. 8). In fact, ‘neoclassical realist theory can elucidate the conditions under which each of the leading approaches [‘structural realist, liberal and constructivist alternatives’] are likely to be useful, as well as the limitations of these approaches [parenthesis added]’ (Ripsman et al. 2016, p. 9). Although Ripsman et al. understand grand theory in general to be a paradigm, their

particular neoclassical realist grand theory is a “multiparadigmatic” paradigm. For Ripsman et al., there is no reason why a paradigm in IR cannot take such a form. By establishing a “multiparadigmatic” grand theory or paradigm, ‘Neoclassical realist theory’ overcomes the ‘shortcomings’ (Ripsman et al. 2016, p. 7) of ‘other major international relations research programs—structural realism, liberalism and constructivism’ by elucidating ‘comparatively’ more ‘about foreign policy and international politics’ (Ripsman et al. 2016, p. 2).

Ripsman et al.’s understanding of grand theory is, for example, markedly different from that of Richard Ned Lebow’s (2008) and Barry Buzan’s (2004). For Lebow and Buzan, grand theory is a theory which accounts for, or incorporates the insights of, multiple paradigms into a broader approach. Lebow, for example, claims that his cultural grand theory ‘can account for all existing paradigms of international relations’ (Lebow 2008, p. 114) and Buzan claims that a grand theory acknowledges ‘multiple rather than competing paradigms’ (Buzan et al. 2001, p. 37). Although Ripsman et al. are on comparable ground with Lebow and Buzan by arguing that their grand theory is “multiparadigmatic” (Ripsman et al. 2016, p. 164), such a broader approach for Lebow and Buzan is not in and of itself a paradigm.

Ripsman et al. refer to their grand theory in *Neoclassical Realist Theory of International Politics* (2016) as type III neoclassical realism. Type II neoclassical realism, found in *The Challenge of Grand Strategy: The Great Powers and the Broken Balance between the World Wars* (2012), was said to offer a theory of foreign policy rather than a theory of foreign policy and international politics (Ripsman et al. 2016, p. 12). Type I neoclassical realism, found in *Neoclassical Realism, the State and Foreign Policy* (2009), was said to offer more limited explanations of foreign policy rather than a theory (Ripsman et al. 2016, p. 12). Type I neoclassical realism was particularly useful to explain idiosyncratic state behaviour which did not conform to the systemic imperatives of structural realism (Ripsman et al. 2016, pp.26-29). Type III neoclassical realism is said to build upon type II and I neoclassical realism (Ripsman et al.

2016, pp. 31-32) and is presented by Ripsman et al. as the envisaged end point of an intellectual collaboration which began in 2003 (2016, p. ix).

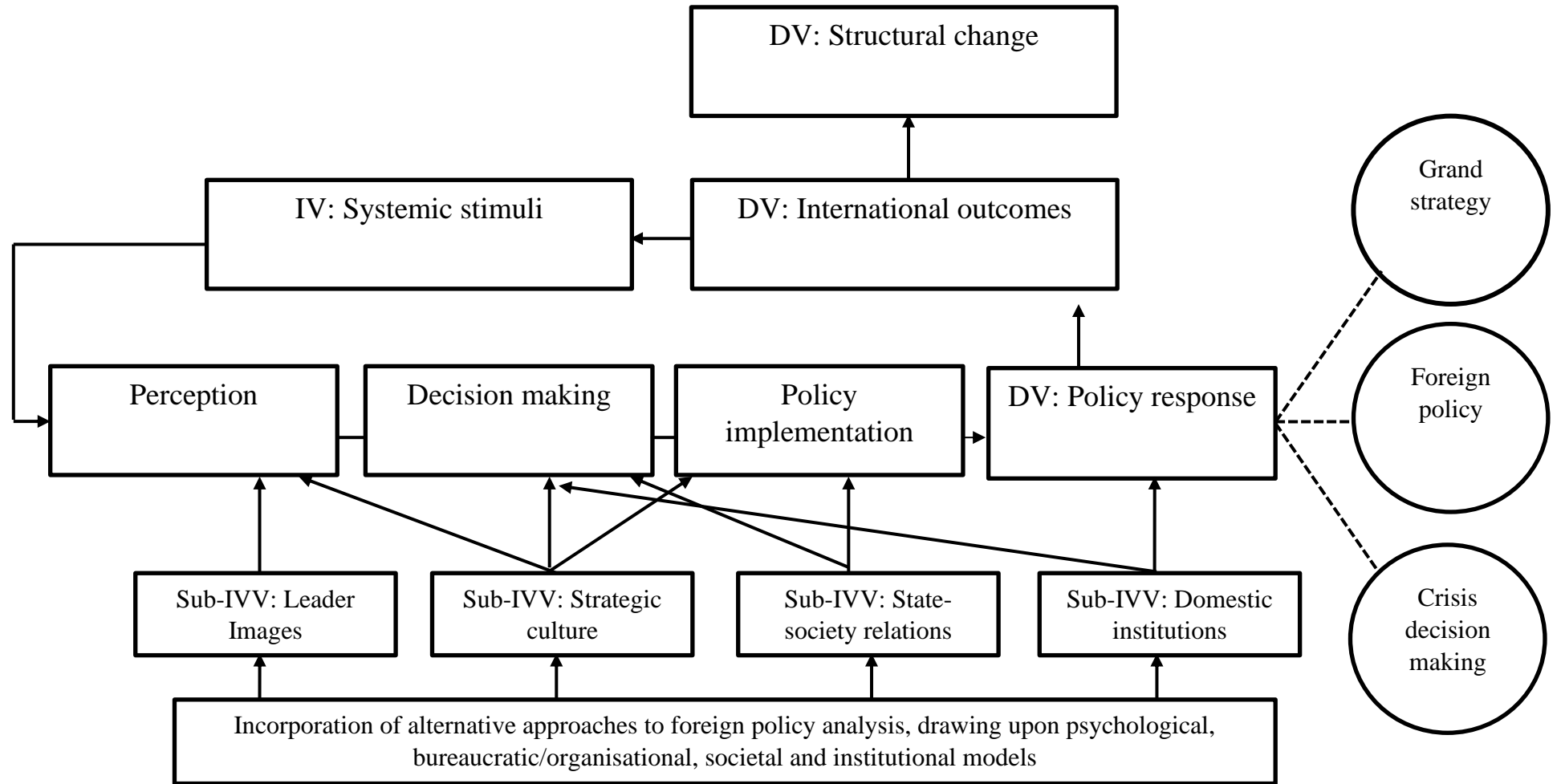
I argue that Ripsman, Taliaferro and Lobell have remained committed to their grand theory because they do not engage in the relevant philosophy of science which would demonstrate its problems. I illustrate how this is the case in four key steps. First, I address why Ripsman et al. are committed to grand theory by examining how they construct their grand theory and what they claim to achieve by constructing type III neoclassical realism as a grand theory. Second, I illustrate that the problem with Ripsman et al.'s construction of grand theory is that it is a "multiparadigmatic" paradigm. I argue, consequently, that type III neoclassical realism cannot perform the tasks claimed by Ripsman et al. Third, I investigate how Ripsman et al. arrived at their "multiparadigmatic" paradigm. I claim that types I and II neoclassical realism constitute a single "multiparadigmatic" paradigm for the study of foreign policy rather than international politics. Ripsman et al.'s type I-II "multiparadigmatic" paradigm, I argue, forms the foundation of type III neoclassical realism. Ripsman et al.'s neoclassical realist project has therefore, I claim, been problematic from the beginning. Finally, I demonstrate that Ripsman et al. purposefully do not engage in either debates of the philosophy science or questions of epistemology. I claim that Ripsman et al. do not fully appreciate the necessary limitations of a paradigm.

This paper is structured in four sections. Section one addresses how Ripsman et al. construct their grand theory and what they claim to achieve by constructing type III neoclassical realism as a grand theory. Section two illustrates the problem with type III neoclassical realism. Section three investigates how Ripsman et al. arrived at their "multiparadigmatic" paradigm. The final section demonstrates that Ripsman et al. do not fully appreciate the necessary limitations of a paradigm.

Why Ripsman et al. Are Committed to Grand Theory

Ripsman et al. present type III neoclassical realism as a ‘Research Paradigm’ (2016, p. 33) consisting of three different levels of analysis: the international, the domestic and the individual (Ripsman et al 2016, p. 34). The individual level of analysis for Ripsman et al. is constituted by various approaches to foreign policy analysis (FPA) including, for example, ‘psychological models’ (2016, p. 62). Such approaches are conceptualised via sub-unit level intervening variables drawn from already existing concepts in FPA such as leader images (Ripsman et al. 2016, pp. 61-66). The individual level of analysis for Ripsman et al., is designed to explain the behaviour of particular agents such as state leaders from the “bottom-up” (2016, pp. 61-66). The domestic level of analysis is constituted by three clusters of intervening variables: perception, decision-making and policy implementation (Ripsman et al. 2016, p. 60). Each cluster may include one or more sub-unit intervening variables (Ripsman et al. 2016, pp. 58-79). Intervening variables mediate information or ‘stimuli’ (Ripsman et al. 2016, pp. 33-34) from Ripsman et al.’s international unit of analysis, structural realism (2016, pp. 33-57), from the “top-down” (2016, pp. 58-79. See figure 3 on the following page). While, for Ripsman et al., ‘States construct foreign policy with an eye to the external environment ... as realists maintain’, how states interpret ‘international challenges may be affected by other variables, be they domestic political variables of the type emphasized by liberals or ideational or cultural variables advanced by constructivists’ (2016, p. 164). Type III neoclassical realism is therefore ‘multiparadigmatic’ (Ripsman et al. 2016, p. 164) by including ‘the insights of multiple approaches or paradigms’ (Ripsman et al. 2016, p. 9) as intervening variables. Ripsman et al. bemoan the ‘paradigmatic rigidity of debates in our field’ (2016, p. ix), and share ‘a concern that rigid coherence to paradigmatic approaches strips much of the nuance and complexity away from international politics’ and ‘distorts our understanding of the subject’ (2016, p. 163).

Figure 3: Type III Neoclassical Realism adapted from *Neoclassical Realist Theory of International Politics* (Ripsman et al. 2016, pp. 34, 59, 81).



Ripsman et al. present a sequence of causality to explain international behaviour ranging from the international to the individual levels of analysis (Ripsman et al. 2016, p. 106). Information or stimuli from within Ripsman et al.'s international level of analysis, structural realism, must be identified and selected by the researcher as an independent variable. Once identified, an independent variable (or information/stimuli), must be understood via the selection of particular intervening variables (crafted from rival paradigms) supported from the "bottom-up" by sub-unit level intervening variables informed by approaches to FPA. Via the identification and selection of particular variables, neoclassical realist researchers are able to offer explanations of crisis decision-making, foreign policy and grand strategy (Ripsman et al. 2016, p. 81). By mapping the identified behaviour of states in an inductive fashion over time, Ripsman et al. claim that type III neoclassical can explain broader interstate phenomena such as war and when such broader interstate phenomena can change the structure of the international system (2016, pp. 110-113).

Ripsman et al. claim that by conducting research within the framework they present, and by following their sequence of causality, 'there can be many [multiparadigmatic] neoclassical realist theories of international politics [parenthesis added]' simultaneously (2016, p. 10):

'Randall Schweller's theory of under balancing, Jeffrey Taliaferro's theory of resource mobilization, and Nicholas Kitchen's theory of grand strategy formation may use different variables to explain different phenomena and may even conflict partially, but they are still united by the assumptions about foreign policy and international politics that we identify in this book' (Ripsman et al. 2016, p. 10).

Type III neoclassical realism is therefore 'like other broad approaches or paradigms' which 'can encompass many, often competing, theories that nonetheless share the same set of core assumptions about the way states navigate their international and domestic environments' (Ripsman et al. 2016, pp. 9-10). Type III neoclassical realism, 'In this regard ... is analogous to the liberal paradigm' which is likewise said to unite various theories with common assumptions

(Ripsman et al. 2016, p. 9). For Ripsman et al., ‘Neoclassical realist theory addresses’ the ‘shortcomings’ (2016, p. 7) of other IR paradigms, namely: structural realism (2016, p. 2), *Innenpolitik* approaches/liberalism (2016, pp.2-6) and constructivism (2016, pp. 6-7) by ‘providing greater explanatory leverage’ (Ripsman et al 2016, p. 1). By including the variables of rival paradigms within its paradigmatic framework, type III neoclassical realism can ‘answer “big and important” questions, on which existing theoretical approaches—including structural realism, liberalism, and constructivism—cannot shed sufficient light’ (Ripsman et al 2016, p. 188). Type III neoclassical realism, as a “multiparadigmatic” paradigm or grand theory, is ‘a far more powerful explanatory tool than its competitors’ (Ripsman et al. 2016, p. 7).

The Problem With Type III Neoclassical Realism

The problem with type III neoclassical realism, I argue, is that it is a “multiparadigmatic” paradigm. As a “multiparadigmatic” paradigm, type III neoclassical realism is caught between two inequivalent kinds of grand theory. On one hand, type III ‘neoclassical realist theory’ can ‘shed light on all aspects of foreign policy and international politics’ (2016, p. 98) by ‘incorporating variables from other paradigms’ (2016, p. 196). On the other hand, type III neoclassical realism is understood as either its own paradigm (Ripsman et al. 2016, p. 10; Narizny 2017; p. 138) or a realist sub-paradigm (Ripsman et al. 2016, p. 164; Narizny 2017; p. 138). Lebow, for example, defines grand theory as a theory which covers ‘all aspects of international relations’ (2008, p. 121) which he partly achieves by accounting for ‘existing paradigms of international relations’ (2008, p. 114). Buzan defines grand theory as a theory which incorporates the insights of multiple as opposed to competing paradigms (Buzan 2001, p. 488). In distinction from Ripsman et al., and as briefly stated in this paper’s introduction, neither Lebow nor Buzan attempted to incorporate the insights of multiple paradigms within a single paradigmatic framework. Rather, Lebow and Buzan each constructed their grand theories as a dialogue

between three positions which reflect and incorporate the insights of IR's paradigms (Lebow 2008, p. 94; Buzan 2004, p. 159). For Lebow and Buzan, a grand theory is a theory which operates at a greater level of abstraction to that of a paradigm. A grand theory which incorporates IR's paradigms is therefore distinct from a grand theory that is a paradigm. By constructing a grand theory as a "multiparadigmatic paradigm", Ripsman et al. are caught between two inequivalent stools.

Ripsman et al.'s construction of grand theory as a "multiparadigmatic" paradigm is problematic. Narizny, for example, claims that type III neoclassical realism is a kind of "theory of everything" (2017, p. 185), and that by 'attempting to explain nearly everything, it excludes nothing' (2017, p. 185). The problem is that by trying to reconcile 'a top-down method in which security ostensibly matters most (per realism) with the possibility of varying preferences (per liberalism [and constructivism]), neoclassical realism sets up its adherents for error [parenthesis added]' (Narizny 2017, p. 187). Ripsman et al. violate the assumption that 'states ... are undifferentiated by function' (James 2002, p. 121 cited in Narizny 2017, p. 160) because type III neoclassical realism postulates that states 'vary in their [domestic] preferences [parenthesis added]' (Narizny 2017, p. 162). One's 'choice of a paradigm is not just an ontological bet; it is also a methodological one' (2017, p. 187). The error for Narizny is one of causality. He writes:

'In a chapter titled "Resolving Key Theoretical Debates with Neoclassical Realism," Ripsman, Taliaferro, and Lobell propose to explain Britain's turn toward protectionism as early as 1931-32 as a response to "scarce security". What about the Great Depression, the consequent collapse of the British economy, its effect on domestic interest groups, and the representation of their interests by the government?' (Narizny 2017, p. 187).

In this example, the key issue is that by attempting to construct a grand theory as a "multiparadigmatic" paradigm, it cannot be accurately established that the international system was the primary cause for Britain's turn toward protectionism. Narizny raises other significant

domestic factors which may also have had casual primacy to explain Britain's protectionist stance. If causal primacy cannot be established, Ripsman et al. cannot say that domestic variables drawn from other paradigms are in fact intervening in the causal mechanism they present. Information or stimuli, Ripsman et al.'s independent variables, may in fact be mediating the relationship between domestic variables and state behaviour. Consequently, Ripsman et al. can neither establish their independent nor intervening variables. But, If Ripsman et al. cannot identify their independent variables, they cannot explain what is causing observed outcomes in their dependant variables of crisis decision making, foreign policy, grand strategy, international outcomes and structural change. By combining the variables of multiple paradigms within a single paradigmatic framework, Ripsman et al. move from three rival explanations of state behaviour (found in structural realism, liberalism and constructivism) to a single but problematic account of state behaviour. Because Ripsman et al. cannot identify their independent variables, type III neoclassical realism is a descriptive rather than explanatory theory. Ripsman et al. provide a descriptively accurate account of particular international systems and the domestic factors relevant to state behaviour at particular points in time, but they do not provide an explanation of state behaviour. By providing a description of particular international systems in which idiosyncratic state behaviour operates, Ripsman et al. do not sufficiently 'shed light on all aspects of foreign policy and international politics' (2016, p. 98) nor do they provide a paradigm. Because type III neoclassical realism is a "multiparadigmatic" paradigm, it is caught between two inequivalent types of grand theory. Consequently, Ripsman et al. achieve neither a grand theory of the type constructed by Lebow (2008) and Buzan (2004) nor a paradigm 'analogous to the liberal paradigm' (2016, p. 9).

How Ripsman et al. Arrived at Their “Multiparadigmatic” Grand Theory

I argue that Ripsman et al.’s effort to develop neoclassical realism as a grand theory was flawed from the beginning. I claim that Ripsman et al. “paradigmised” neoclassical realism as an existing school thought in *Neoclassical Realism, The State and Foreign Policy* (2009). By building on type I neoclassical realism to construct a grand theory in type III neoclassical realism, Ripsman et al. build on flawed foundations. In what follows, I return to the Ur-source of neoclassical realism, Gideon Rose’s review article: ‘Neoclassical Realism and Theories of Foreign Policy’ (1998). I initially return to Rose’s article as a way of illustrating the school of thought Ripsman et al. problematically paradigmised. Next, I examine the flawed foundations of type I and type II neoclassical realism.

Rose identified the incorporation of ‘both external and internal variables’ (1998, p. 146) in the works of Brown (1995), Christensen (1996), Schweller (1998), Wohlforth (1993) and Zakaria (1998) which he claimed ‘collectively set out a fourth school’ of foreign policy ‘which I term “neoclassical realism”’ (1998, p. 146). Neoclassical realism’s ‘adherents’, for Rose, ‘argue that the scope and ambition of a country’s foreign policy is driven first and foremost by its place in the international system ... This is why they are realist’ (1998, p. 146), and second by internal state attributes such as the perception of state leaders (1998, p. 147) found at the unit level (1996, p. 146). Rose is clear that ‘*Innenpolitikers* preferred independent variables must be relegated to second place analytically’ as intervening variables (1998, p. 151). While neoclassical realism for Rose is “multiparadigmatic” by incorporating the variables of multiple paradigms, he does not

¹ In addition to *Innenpolitik* theories, offensive realism and defensive realism (Rose 1998, p. 146).

² Gideon Rose revealed at the International Studies Association’s (ISA) 2018 annual conference that he originally named neoclassical realism “little realism”. Schweller and Zakaria, however, did not want their scholarship to be referred to as “little”. Please see: International Studies Association (2018) San Francisco 2018 - Full Program (p. 96).

conceptualise neoclassical realism as either its own paradigm or a realist sub-paradigm. Rather, neoclassical realism is a broad school of thought useful for researchers to construct theories of foreign policy (Rose 1998, p. 154).

Although type I neoclassical realism is presented by Ripsman et al. as an approach to explain anomalous cases of foreign policy in structural realism (Ripsman et al. 2016, pp.26-29), they argue in *Neoclassical Realism, The State and Foreign Policy* that type I neoclassical realism ‘is not simply a refinement of Waltz’s balance of power theory nor an attempt to smuggle unit-level variables into the theory to explain anomalies’ (Lobell et al. 2009, p. 21). Type I neoclassical realism seeks to do more than explain anomalies by examining ‘variation in the foreign policies of the same state over time or across different states facing similar external constraints’ (Lobell et al. 2009, p. 21). Type I neoclassical realism constitutes an approach in its own right to the extent that it is considered to be ‘an appropriate paradigm to construct theories of foreign policy’ (Lobell et al. 2009, p. 287). In fact, the theoretical framework of type I neoclassical realism exists in the same manner to that of type III and adopts an identical casual logic. Lobell et al. begin with the international system in which events must be processed via the perceptions and calculations of state elites as intervening variables—drawn from other paradigms—to produce foreign policy outcomes (2009, p. 28). Lobell et al. construct type I neoclassical realism as a “multiparadigmatic” paradigm for foreign policy, and by so doing “paradigmise” what Rose initially crafted as a broad school of thought.

Type II neoclassical realism does not present a distinct theoretical framework. Taliaferro et al. use the theoretical apparatus of type I neoclassical realism to better understand the foreign policies of the United States, Great Britain, France and Russia between the World Wars³.

³ *The Challenge of Grand Strategy* (2012) is an edited volume containing the work not only of Taliaferro, Ripsman and Lobell, but also of Peter Jackson, Scott Silverstone, Andrew Webster, Dale Copeland, Jack Levy, Tsuyoshi Kawasaki, Timothy Crawford, Mark Hass and David Edelstein. Each author identifies respective dates to which

Taliaferro et al. state that ‘the general framework we use for this volume is a comparison of the permissive international environment of the 1920s and the less permissive (or more restrictive) environment of the 1930s’ for which *Neoclassical Realism, The State and Foreign Policy* (2009) is cited (2012, p. 23). Furthermore, Taliaferro et al. are careful to state that their general framework derives from the ‘emerging paradigm of neoclassical realism’ (2012, p. 33). Types I and II neoclassical realism present the same “multiparadigmatic” paradigm of foreign policy. Because type I neoclassical realism presents the same causal mechanism as type III, and because type I and II neoclassical realism present the same “multiparadigmatic” paradigm, the problem of causality discussed in the previous section originated in *Neoclassical Realism, The State and Foreign Policy* (2009). By building on types I and II neoclassical realism (Ripsman et al. 2016, pp. 31-32), or by building on a single “multiparadigmatic” paradigm, Ripsman et al. constructed a grand theory on flawed foundations. In fact, I claim that type III neoclassical realism is the second iteration of Ripsman et al.’s “multiparadigmatic” paradigm of foreign policy which they mistakenly present as a grand theory of international politics.

Type III neoclassical realism explains international politics only to the degree that it is relevant for foreign policy formation. While Ripsman et al. claim that the grand strategic choices of the major powers over time can affect international outcomes such as war and can reshape the structure of the international system, type III neoclassical realism explains only the particular (Ripsman et al. 2016, p. 95). Type III neoclassical realism examines the grand strategies of particular states within particular epochs, war between particular states in particular epochs and particular circumstances in which war may lead to structural change (Ripsman et al. 2016, pp. 95-98). War and structural change serve as the impetus for new stimuli from which new particular

their analyses refer. See pages v-vi in Taliaferro, J., Ripsman, N., and SE. Lobell. 2012. *The Challenge of Grand Strategy: The Great Powers and the Broken Balance between the World Wars*. Cambridge: Cambridge University Press.

state behaviours are formulated (Ripsman et al. 2016, p. 81). Ripsman et al. do not offer a theory of international politics which applies to the behaviour of all states (2016, p. 96), and their explanations of international level phenomena (war and structural change) only serve to identify stimuli for new explanations of foreign policy. The epistemologically positivist and methodologically inductive nature of type III neoclassical realism thus serves to establish similarity ‘across at least somewhat similar cases’ (Ripsman et al. 2016, p. 106) of foreign policy only. Ripsman et al. therefore offer a theory which can be applied to particular states in particular epochs through time which, when mapped in an inductive fashion, may lead to the creation of so-called ‘contingent’ (2016, p. 105) laws of foreign policy.

Why Ripsman et al. Remain Committed to Grand Theory

Ripsman et al. remain committed to their notion of grand theory because they do not engage in the necessary philosophy of science which would demonstrate its problems. Ripsman et al. are clear that they use the term paradigm:

‘To denote empirical theories that proceed from the same core assumptions and that identify related independent variables. In this book, we do not engage the debates about meta-theory (or philosophy of science) and the criteria that political scientists ought to employ to measure scientific progress’ (Ripsman et al. 2016, p. 8).

Ripsman et al. explicitly do not engage with necessary debates in the philosophy of science to fully appreciate the limitations of a paradigm. Ripsman et al. accept their notion of grand theory because they do not fully appreciate that a “multiparadigmatic” paradigm cannot meet the standards of a paradigm. In fact, Ripsman et al. purposefully leave ‘aside questions of epistemology’ by agreeing with Kitchen that “Much of IR theory is overly concerned with epistemology’ because ‘arguments over the status of subjects come to subsume the subjects”

(Kitchen 2012, p. 85 cited in Ripsman et al. 2016, p. 158). For Ripsman et al., questions of epistemology and the philosophy of science are not relevant. In their Response to Narizny, for example, Ripsman et al. do not engage with Narizny's critique. They simply disagree 'with Narizny's interpretation ... that realism requires states not to vary in their preferences' (2018, p. 197) by claiming that 'it would be foolhardy to suggest either that all states have identical interests or that the international system determines what these interests are' (2018, p. 198). They also question why Narizny 'settled' on 'the hard core of realism' postulated by Patrick James (Ripsman et al. 2016, p. 197). Narizny, in his reply to Ripsman et al., notes that Ripsman et al.'s response does 'not resolve any of' his identified problems; 'indeed, it does not even address them' (2018, p. 199). He also states, although 'The answer should be obvious', that 'James's approach is guided by the philosophy of science, the field of inquiry devoted to understanding the coherence and progressivity of theoretical paradigms' (Narizny 2018, p. 200). Ripsman et al. purposefully avoid, and do not engage in, questions of epistemology and debates of the philosophy of science in *Neoclassical Realist Theory of International Politics* (2016).

Although Ripsman et al. do not purposefully avoid difficult discussions of epistemology and the philosophy of science in *The Challenge of Grand Strategy* (2012) or in *Neoclassical Realism, The State and Foreign Policy* (2009), they nevertheless do not engage with such questions. In *The Challenge of Grand Strategy* (2012), Ripsman et al. discuss epistemology only to confirm that they 'are committed to a positivist epistemology' and 'that they take the existence of an objectively knowable world as a given' (Taliaferro et al. 2012, p. 22). In *Neoclassical Realism, The State and Foreign Policy* (2009), Ripsman et al. discuss epistemology only to clarify that neoclassical realism uses 'deductive theorizing' (Lobell et al. 2009, p. 20). While Ripsman et al., do not intentionally avoid difficult questions of epistemology and the philosophy of science, they certainly do not engage with them in either *The Challenge of Grand Strategy* (2012) or in *Neoclassical Realism, The State and Foreign Policy* (2009). Because Ripsman et al. do not engage

with such questions, they constructed neoclassical realism as a “multiparadigmatic” paradigm of foreign policy (2009) the next iteration of which came to be presented by Ripsman et al. as a grand theory of international politics (2016) replete with the problems I identify in the previous section. Ripsman et al. remain committed to their notion of grand theory, in part, because they uncritically accept the notion of a “multiparadigmatic” paradigm.

The general understanding of grand theory from which Ripsman et al. draw is also problematic. As discussed in the introduction to this paper, and following Mearsheimer and Walt (2013), Ripsman et al. understand a paradigm/grand theory to be an apparatus which allows one to understand the ‘dynamics of international politics and its regularities in a holistic manner, rather than simply focusing on largely disconnected empirical results’ (2016, p. 18). Paradigms/grand theories are helpful because they ‘guide our empirical research’ (Ripsman et al. 2016, p. 8). In such an understanding, there is no reason why Ripsman et al. cannot elevate their existing “multiparadigmatic” paradigm—types I and II neoclassical realism—to a grand theory ‘analogous to the liberal paradigm’ (2016, p. 9). The definition of grand theory provided by Mearsheimer and Walt (2013) and used by Ripsman et al. is problematic. Because Ripsman et al. do not engage in the philosophy of science or epistemology, they uncritically accept the notion of a “multiparadigmatic” paradigm, and uncritically accept a definition of grand theory that enables them to present the next phase of their “multiparadigmatic” paradigm as grand theory of international politics. The question, however, is why Ripsman et al. uncritically accepted the notion of a “multiparadigmatic” paradigm in the first place.

Ripsman et al. uncritically accept the notion of a “multiparadigmatic” paradigm because they have an underdeveloped perspective on what constitutes legitimate theory in IR. Legitimate theory for Ripsman et al. either takes form of a paradigm (2016, pp. 7-8) or a ‘mid-range’ theory (2016, p. 163). There is no evidence in *Neoclassical Realist Theory of International Politics* (2016), *The Challenge of Grand Strategy* (2012) or in *Neoclassical Realism, The State and*

Foreign Policy (2009) that legitimate theory can take any other form for Ripsman et al. Because Ripsman et al. aim to explain ‘broader patterns of international outcomes and structural change’ in *Neoclassical Realist Theory of International Politics* (2016, p. x), type III neoclassical realism cannot be crafted as a middle-range theory, it can only be crafted on ‘paradigmatic terms’ (2016, p. 7). The only legitimate way to construct a theory—with the explanatory scope Ripsman et al. seek for type III neoclassical realism—is as a paradigm comparable to others such as structural realism, liberalism and constructivism. In the case of types I and II neoclassical realism, Ripsman et al. constructed neoclassical realism as ‘an appropriate paradigm to construct theories of foreign policy’ (Lobell et al. 2009, p. 287). Again, legitimate theory for Ripsman et al. either took the form of a paradigm or middle range theory (e.g., Taliaferro et al. 2012, p. 33; Lobell et al. 2009, p. 73, 287). Because type I-II neoclassical realism explains ‘variation in the foreign policies of the same state over time or[/and] across different states facing similar external constraints [parenthesis added]’ (Lobell et al. 2009, p. 21), it works ‘upwards from empirical observation of state’s actions to generate new laws of behaviour’ (Quinn 2013, p. 178). As such, it goes beyond theory of the middle-range. Accordingly, type I-II neoclassical realism also needed to be constructed on paradigmatic terms. Ripsman et al. uncritically accept their notion of a “multiparadigmatic” paradigm because their multiparadigmatic approaches (type III neoclassical realism and type I-II neoclassical realism) had to take paradigmatic form by default. Ripsman et al. cannot think outside of a paradigmatic box. Ripsman et al. remain committed to grand theory because; one; they do not engage in the relevant philosophy of science or epistemological issues which would bring into question their “multiparadigmatic” paradigm and the definition of grand theory postulated by Mearsheimer and Walt (2013); two, Ripsman et al. cannot think of any other way to construct a wide-ranging multiparadigmatic theory either in terms of foreign policy (Lobell et al. 2009; Taliaferro et al. 2012), or of foreign policy and international politics (Ripsman et al. 2016).

Conclusion: Neoclassical Realism as a Tradition

Ripsman et al. remain committed to their grand theory because they do not engage in the necessary philosophy of science which would demonstrate its problems. By constructing a “multiparadigmatic” paradigm, Ripsman et al. cannot identify casual primacy in the “model” they present leading to a descriptive rather than an explanatory theory. Ripsman et al. constructed type III neoclassical realism on flawed foundations by building on an existing “multiparadigmatic” paradigm of foreign policy. It is arguable that type III neoclassical realism presents the next iteration of such a paradigm of foreign policy as opposed to one of foreign policy and international politics. Ripsman et al. purposefully do not engage in debates of the philosophy of science, or questions of epistemology, because they think that they are an unhelpful distraction from the task of explaining state behaviour. Overall, type III neoclassical realism is a grand theory that depends on nobody looking too closely at its foundations, and which relies on dogma that those foundations can support the weight of its claims about foreign policy and international politics. When subject to sustained scrutiny, the foundations of Ripsman et al.’s neoclassical realism are brought into question.

The above findings contribute to a debate of grand theory in the wider discipline because they illustrate that any theorist who attempts to construct a grand theory as a “multiparadigmatic” paradigm may experience the same problem as Ripsman et al. Grand theory in general can therefore partly be viewed as problematic because one of its types, a “multiparadigmatic” paradigm, has been demonstrated problematic in the case of Ripsman et al. My findings may also help to explain why theorists such as Mearsheimer and Walt (2013) remain committed to grand theory. Mearsheimer and Walt’s definition of grand theory can include any paradigm whether “multiparadigmatic” or not. Mearsheimer and Walt (2013) may remain committed to their particular notion of grand theory because they do not engage with the philosophy of science or questions of epistemology in sufficient depth. One cannot accuse Mearsheimer and Walt of

eschewing issues of the philosophy of science because their work contains a substantial engagement with their preferred epistemological position of ‘scientific realism’ (2013, pp. 432-434). The work of Walt, in particular, has also engaged with issues of epistemology in the past (e.g., 1997). For Mearsheimer and Walt to endorse a definition of grand theory which includes a “multiparadigmatic” paradigm, however, suggests that they do not engage with the philosophy of science to the extent of theorists such as Narizny (2017). If they did, Mearsheimer and Walt (2013) would either have to revise their understanding of grand theory or revise their commitment to grand theory. In addition, neoclassical realists who are currently attempting to develop grand theories—Kitchen (2021) and Desmaele and Onea (2021)—may want to avoid constructing their neoclassical realist grand theories as “multiparadigmatic” paradigms. In fact, Kitchen seems perilously close to developing a “multiparadigmatic” paradigm because he ‘outlines the ways in which NCR could become “a grand theory of international politics” by integrating “system and unit-levels” (Kitchen in Sterling-Folkner 2021, p. 289).

In view of the preceding analysis, I endorse Sterling-Folkner’s view that neoclassical realism should be understood from a ‘traditions perspective’ (2009, p. 213) rather than squeezed into an inadequate paradigmatic box. Sterling-Folkner claims that ‘providing a precise definition of neoclassical realism, or even describing its content and logic, is difficult’ (2009, p. 208), and that ‘The term was provided by the observer after the fact, and so it suggests a level of theoretical coherence and cooperation where little actually exists’ (2009, p. 208). From a traditions perspective, neoclassical realism is understood simply as a broad school of thought in which its ‘devotees debate what it is and whether they think it serves as a useful lens from which to examine and understand the world’ (Sterling-Folkner 2009, p. 208). From such a perspective, neoclassical realism is a ‘living thought’ community ‘in which analytical incoherence, disagreement, and struggles over the desires for analytical reconciliation ... will be present’ (Sterling-Folkner 2009 pp. 213-214). I claim that neoclassical realism exists as such a community today. Neoclassical

realism ‘is an ongoing, unsettled, and indeterminate dialogue between scholars over what realism is and can do’ (Sterling-Folkner 2021, p. 289), one which continues to question whether one should ‘prioritise midrange theorizing and empirically driven puzzles, or metatheory and grand debates’ (Sterling-Folkner 2021, p. 289). Neoclassical realism cannot provide ‘analytical certainty and clear boundaries’ because there is no single neoclassical realist ‘text to which we can turn for NCR’s roots, nor is there a single a priori NCR core that determines what does or does not count as its scholarship’ (Sterling-Folkner 2021, p. 290). Ripsman et al. attempt to assimilate a profoundly diverse group of contributions and scholars into a single paradigm resulting in the analytical tension discussed throughout. Neoclassical realism should be understood and left as a ‘tradition’ (2009, p. 213).

The analysis contained herein, I argue, also begs two questions for further research. One, are Ripsman et al. motivated to pursue grand theory for reasons other than its perceived benefits? Two, what is grand theory in IR? While I cannot answer these questions in full here, I hope to discuss them in enough detail to convincingly illustrate that others should take up the task of answering them. I address each question in turn.

What Motivates Ripsman et al. to Pursue Grand Theory?

I suggest that Ripsman et al. may pursue grand theory because of the prestige that it can confer. Ripsman et al. aim to craft a paradigm directly comparable to the leading paradigms of structural realism, liberalism and constructivism (2016, p. 2, 188). Structural realism is most associated with Waltz (Ripsman et al. 2016, p. iv), liberalism with Moravcsik (Ripsman et al 2016, p. 4) and constructivism with Wendt (Ripsman et al 2016, p. 157). Waltz, Moravcsik and Wendt are some of the leading names of IR and are synonymous with the aforementioned paradigms. By crafting a grand theory as a “multiparadigmatic” paradigm, it could be argued that Ripsman et al. are attempting to emulate the prestige of figures such as Waltz, Moravcsik and Wendt by becoming

synonymous with what they hope will become a leading paradigm of the field. Because type III neoclassical realism is a “multiparadigmatic” paradigm, Ripsman et al. claim that it can address “big and important” questions, on which existing theoretical approaches—including structural realism, liberalism, and constructivism—cannot shed sufficient light’ (Ripsman et al 2016, p. 188). But yet, as a single paradigm, type III neoclassical realism can stand alongside realism, liberal and constructivism and interlock into such a paradigmatic structure. By being synonymous with, and by being the architects of, what Ripsman et al. perceive to be a leading paradigm, it could be argued that Ripsman et al. attempt to obtain the prestige of IR’s most prominent figures. If this were the case, there would be a dual dynamic at play in a theorist’s motivation to pursue grand theory. On one hand, theorists would pursue grand theory because of its perceived benefits for the discipline. In Ripsman et al.’s case, a theory of greater explanatory power than conventional alternatives. On the other hand, theorists would pursue grand theory for themselves. In fact, if theorists such as Buzan (2004) and Lebow (2008) also constructed grand theories to obtain prestige, theorists in IR would be pursuing very different types of grand theory for the same reason. But if the only common characteristic between such different types of grand theory is the pursuit of prestige on the part of their respective authors, the pursuit of grand theory may simply be a pursuit of prestige in IR. While I have only begun to scratch the surface of this question here, I question the nature of grand theory in IR below.

What Is Grand Theory in IR?

Grand theory, in the cases of Lebow (2008) and Buzan (2004) discussed herein, includes the insights of multiple paradigms by operating at a greater level of abstraction to that of a paradigm. Lebow and Buzan achieve their grand theories by presenting dialogues between three mutually supportive and compatible positions. Grand theories for Lebow and Buzan are not paradigms. Grand theory for Ripsman et al. can be any paradigm including those which are

“multiparadigmatic”. A paradigm for Narizny, on the other hand, cannot be “multiparadigmatic” in the manner constructed by Ripsman et al. The “multiparadigmatic” paradigm of Ripsman et al. is therefore distinct from a mainstream paradigm such as structural realism. My analysis of Ripsman et al.’s type III neoclassical realism reveals that grand theory means three distinct and inequivalent types of theory at the same time: a dialogue, a “multiparadigmatic” paradigm and a paradigm. The meaning of grand theory across the cases discussed is unstable. The claim that grand theory is useful to conduct research by its proponents in the wider discipline becomes questionable. When theorists claim that grand theory is useful, they are claiming that different kinds of theory are useful. For example, Mearsheimer and Walt (2013, pp. 428-435) claim that paradigms are useful whereas Onuf claims that grand theories of the type constructed by Lebow are useful (Onuf 2013, p. 144; 2017, p.1). In fact, the paradigms emphasised by Mearsheimer and Walt can mean a “multiparadigmatic” paradigm presented by Ripsman et al. and they can mean a paradigm as endorsed by Narizny. Because the types of theory to which such theorists refer are inequivalent, it is questionable whether grand theory in particular is useful or just some kind of theoretical apparatus through which to construct one’s research is useful. Ripsman et al. aspire to a form of theory when nobody can agree on what that form of theory is, and when the integrity of that type of theory can be brought into question. It could be argued that Ripsman et al. have not proposed a grand theory, but simply a problematic paradigm.

If it can be argued that Ripsman et al. have not constructed a grand theory but a paradigm, what does this say about existing grand theories in the discipline and what does it say about the projects of grand theory with which theorists are currently engaged such as Kitchen (2021) and Desmaele and Onea (2021)? It suggests that theorists such as Lebow (2008) and Buzan (2004) may not have constructed grand theories but an apparatus which facilitates a dialogue between three positions. It also suggests that theorists such as Kitchen (2021) and Desmaele and Onea (2021) may not be engaged in projects of grand theory despite their claims to be so. Although

type III neoclassical realism is a problematic grand theory, the nature of grand theory itself must also be questioned.

Conclusion

The Ambiguity and Prestige of Grand Theory in IR

‘Theory is the lodestone in the field of International Relations (IR). Its theorists are the field’s most famous and prestigious scholars ... virtually all the classic IR books are theory-laden works like Hans Morgenthau’s Politics among Nations, Kenneth Waltz’s Theory of International Politics, Thomas Schelling’s The Strategy of Conflict, Hedley Bull’s The Anarchical Society, Robert Keohane’s After Hegemony, and Alexander Wendt’s Social Theory of International Politics’ (Mearsheimer and Walt 2013, p. 427).

Although grand theory is presented by its proponents in the discipline of IR as useful, grand theory in the cases of Buzan (2004) and Lebow (2008) was useful only as a heuristic device. Buzan constructed a grand theory in *From International to World Society* (2004) which he abandoned in *Global International Society* (2018), and Lebow constructed a grand theory in *A Cultural Theory of International Relations* (2008) which he abandoned in *The Rise and Fall of Political Orders* (2018). While grand theory was partly useful for Buzan (2004) and Lebow (2008), it was only partially relevant to the development of their projects. Buzan produced knowledge of global international society without grand theory, and Lebow produced knowledge of political order without grand theory. Out of the three cases of grand theory I examine, it is only Ripsman et al. (2016) who persist in their efforts to develop a grand theory and they do so because they do not engage with relevant issues of the philosophy of science and epistemology. While proponents of grand theory emphasise the utility of grand theory as a “map” (Mearsheimer and Walt 2013, p. 435), a “big picture” (Snyder 2013, p. 558) or a “framework”, such a map, picture or framework is only of limited use to Buzan (2018) and Lebow (2018), and manifests as incoherence in the case of Ripsman et al. (2016). In agreement with grand theory’s

critics in IR, I claim that grand theory is deeply problematic and possesses only limited utility as a heuristic device.

I conclude this dissertation with two final arguments. First, I make an argument concerning the nature of grand theory itself. I argue that grand theory has come to mean so many inequivalent referents simultaneously that it is better understood as a floating signifier. Using the work of Laclau (2007), I claim that grand theory is a floating signifier which absorbs rather than emits meaning. Second, I make a sociological argument that the pursuit of grand theory, in the cases of Buzan, Lebow and Ripsman et al., is an effort to obtain prestige in the discipline by each author. Following an engagement with literature on the sociology of IR, and using the work of Bourdieu, I claim that Ripsman et al. (2016) construct a grand theory in an effort to emulate the prestige of theorists such as Waltz, Wendt and Keohane (Mearsheimer and Walt 2013, p. 427). I claim, in particular, that Ripsman et al. aim to become synonymous with what they hope will become a leading paradigm of the field in the manner that Waltz, Wendt and Keohane are synonymous with the leading paradigms of IR. Continuing to use the work of Bourdieu, I also claim that Lebow (2008) and Buzan (2004) followed in a tradition of previous attempts to establish a single and universally applicable theory and thereby sought the prestige that such a theory would confer among those who value what it offers. I make each final argument in turn.

Grand Theory Is a Floating Signifier in IR

I argue that grand theory is a floating signifier which absorbs rather than emits meaning. I claim that grand theory is an unreliable, ungrounded and unstable type of theory without a particular and fixed form rather than a useful type of theory. For Laclau, floating signifiers do not possess a 'fully fixed' meaning (Laclau 2007, p. 36). Floating signifiers are characterised by 'ambiguity' because of 'either an overdetermination or underdetermination of signifieds' (2007, p. 36) of which they are constituted. Laclau gives the example of 'a political discourse asserting that

“Labour is more capable than the Tory Party to ensure *unity of the British People*” (2007, p. 94). The Tory Party provides one ‘concrete content of the unity’ while Labour provides another (Laclau 2007, p. 95). Without the provision of such concrete contents, ‘unity cannot exist’ (Laclau 2007, p. 95). The word “Unity” is a floating signifier because its signified is fixed only by the concrete contents’ provided by either the Tory party or Labour (Laclau 2007, p. 95). The signified which constitutes unity changes from one concrete content to the next. The signifier of “unity” moves from the account provided by the Tory party to the account provided by Labour, takes on different forms and consequently refers to different signifieds or notions of unity. “Unity”, however, ‘is not fully exhausted by any of these alternative concrete contents’ (Laclau 2007, p. 95). New concrete contents may emerge with their own signifieds which may give additional meaning to “*unity of the British People*” (2007, p. 94). “Unity” as a floating signifier cannot be defined in a clearly delineated and unequivocal sense because it has neither a fixed nor a permanent meaning. The meaning of “unity” is ambiguous because of the overdetermination of signifieds provided by its shifting substratum of concrete contents.

I apply the concept of a floating signifier as it is understood in the work of Laclau to the notion of grand theory in IR. By so doing, I argue that grand theory is a particular type of floating signifier without a fully fixed meaning in the discipline. I demonstrate how grand theory operates as a vehicle which absorbs meaning, and I illustrate why grand theory possess limited meaning in IR. Grand theory possesses an ambiguous meaning as a “map” (Mearsheimer and Walt 2013, p. 435), a “big picture” (Snyder 2013, p. 558) or a “framework” (Alder 2019, p. 9) which grounds the study of IR. Grand theory has a limited meaning as a type of theory which guides and orients research in one way or another. It is my claim that because grand theory has an ambiguous and limited meaning, it can capture a variety of inequivalent signifieds. The ambiguous and limited meaning of grand theory in IR has come to capture the signifieds of the following: a paradigm, a “multiparadigmatic” paradigm, a dialogue between three positions which embrace the insights of

IR's paradigms but which operate at a greater level of abstraction to that of a paradigm, an overarching explanation or a totalising and monological theory which can take the form of any of the aforementioned or not. As illustrated in my third paper, a "multiparadigmatic" paradigm is neither a paradigm as endorsed by Narizny, nor a grand theory of the type constructed by Buzan (2004) and Lebow (2008). The grand theories constructed by Buzan (2004) and Lebow (2008) are not paradigms. The signified which constitutes grand theory changes from one concrete content to another. The signifier of grand theory moves, for example, from the concrete content provided by Mearsheimer and Walt (2013) to the concrete contents provided by Buzan (2004) and Lebow (2008), takes on different forms, and consequently refers to different signifieds or notions of grand theory. Grand theory, however, 'is not fully exhausted by any of the mentioned 'concrete contents' (Laclau 2007, p. 95). New proposals or constructions of grand theory provide new concrete contents. Grand theory cannot be defined in a clearly delineated and unequivocal sense because it has neither a fixed meaning nor a permanent form. The meaning of grand theory is ambiguous and limited because of the overdetermination of signifieds provided by its shifting substratum of concrete contents. While other semiotic theorists have conceptualised further types of floating signifiers such as Levi-Strauss (1987), Derrida (2002) and Baudrillard (1994), Laclau's conceptualisation is advantageous to apply to grand theory in IR because it captures its precise role as a particular type of floating signifier. Grand theory in IR is a particular type of floating without a 'fully fixed' meaning' (Laclau 2007, p. 36).

I claim that grand theory itself cannot be useful because it is not possible to identify with any precision what a grand theory is. When proponents of grand theory argue that it is useful, they claim that different kinds of theory are useful. Consequently, the tasks such proponents claim a grand should perform are completely different and inequivalent. Grand theory cannot provide a stable or reliable basis for theory in the discipline of IR because it has come to mean different kinds of theory at the same time.

The Prestige of Grand Theory in IR

Research on the sociology of IR is a distinct and significant literature. While some argue that interest in the sociology of IR began with Wæver's 1998 article 'The Sociology of a Not So International Discipline' (Grenier and Hagman 2016, pp. 1-2), others have suggested that interest in the sociology of IR began as early as 1985 with Holsti's *Dividing Discipline* (Hamati-Ataya 2012, p. 627). Since the contributions of Holsti (1985) and Wæver (1998), literature on the sociology of IR has expanded to 'gendered and (post)colonial realities' (Hamati-Ataya 2018, p. 10) such as Tickner and Wæver's *International Relations Scholarship Around the World* (2009), to examinations of institutions and their impact on the production of scholarly knowledge (Hamati-Ataya 2018, p. 10; e.g., Turton 2016; Grenier 2015; Haggmann and Biersteker 2012). One could also add the work of, for example, Banks (1966, 1978, 1979, 1984, 1985, 1985a) who—similarly to Holsti (1985)—taxonomised IR's authors and theories, Hoffmann (1977) and Crawford and Jarvis (2001) who emphasised the centrality of so-called "American IR" throughout the wider discipline, Breuning et al. (2005) who evaluated IR journals, Parmar (2007) who investigated the role of the Rockefeller foundation in the evolution of realism, Hanafi (2011) who examined university systems in the Middle East and Michelsen who emphasised the public role IR scholars should play (2018) and who questioned the rise of critical theory as a category of theory in its own right (2021). Others have directly applied the work and key concepts of Bourdieu to the study of international history (Jackson 2008), European security studies (Mérand 2010) reflexive practices (Bigo 2011; Madsen 2011), the import and export of expertise (Dezalay and Garth 2011), the concept of the "international" (Leander 2011) and to inter- and transnational fields of power (Alder-Nissen 2011; Cohen 2011; Georgakakis 2011; Vauchez 2011). Although united by a concern for the sociology of knowledge—the relationship between the production of knowledge and the social context in which it arises—literature on the sociology of IR possesses distinct research agendas. Despite becoming a significant literature replete with

its own sub-agendas in IR, and despite the prominent role played by grand theory in the discipline, nobody (to the best of my knowledge) has examined the role of prestige as a motivation to pursue grand theory in IR. In addition, while authors have engaged thoroughly with Bourdieu's work and his key concepts in IR, and while Hamati-Ataya has also begun to conceptualise IR as a field in Bourdieusian terms (2012), none have exploited the potential of Bourdieu's work to understand the role of prestige in pursuing grand theory. In what follows, I use the work of Bourdieu, and the contributions his work has inspired in IR, to argue that the pursuit of grand theory is an effort to obtain prestige by Buzan, Lebow and Ripsman et al. By so doing, I contribute to IR's general literature on the sociology of IR and to IR's more particular Bourdieusian literature.

Intellectual Capital

Using the work of Bourdieu, I argue that a proposal of grand theory in the cases of Buzan (2004), Lebow (2008) and Ripsman et al. (2016) can be understood as an attempt to gain intellectual capital (Bourdieu 1988) and 'intellectual renown' (Bourdieu in Wacquant 1989, p. 12) within the discipline of IR. The more intellectual capital a scholar possesses, the more prestigious or renowned they become. I use the work of Bourdieu because he crafted a sociology of academia in which he demonstrated how scholars engage in particular strategies to accrue intellectual capital and gain prestige by either working against, or by working with, dominant approaches in a particular academic field (Bourdieu 1988). In Bourdieu's case, the French academy (Bourdieu 1988). While others in IR have already demonstrated that theorists in the discipline adopt particular strategies with reference to dominant approaches in the field, none have advanced the argument that grand theory is a strategy to obtain prestige. Hamati-Ataya, for example, argues that for 'dissidents of IR's "second debate', the strategy was to interpret the extant version of objectivity as "unscientific", and 'for newer generations of dissidents since the 'third debate', it is

about either denying the validity of ‘objectivism’, or portraying the core’s ‘objectivity’ as a hidden ‘parochialism’ (2012, p. 636). The pursuit of grand theory, I claim, is but one example of an already illustrated precedent in the discipline to adopt particular strategies with reference to dominant approaches in the field.

Bourdieu outlines the manner through which academics gain prestige in *Homo Academicus* (1988). Bourdieu claims that ‘One can and must read *Homo Academicus* as a programme of research on any academic field’ (Bourdieu in Wacquant 1989, p. 11). One can therefore read *Homo Academicus* (1988) as applicable and relevant to the academic field of IR. Bourdieu clarifies that the concept of intellectual capital is closely related to his other notions of habitus, field, and strategy (1988, pp. 8-10). Bourdieu first articulated his concepts of habitus, field and strategy in *Outline of a Theory of Practice* (2013, pp. 159-197; 1977). In *Outline of a Theory of Practice*, Bourdieu uses his concepts to analyse matrimony (2013, pp. 58-71) in particular. Bourdieu’s earlier work in *Outline of a Theory of Practice* (2013; 1977) formed the conceptual basis of *Homo Academicus* (1988). Bourdieu further develops his concepts in *Practical Reason: On the Theory of Action* (1998). For Bourdieu, ‘Such notions as habitus, field, and capital are definable, but only within the theoretical system they constitute, not in isolation’ (Bourdieu in Wacquant 1989, p. 5). The concepts of field, capital, habitus, and strategy acquire meaning relative to the field in which they are used as analytical devices. In broad terms, a field for Bourdieu is a bounded space in which ‘*Social agents*’ are:

‘bearers of capitals and, depending on the position that they occupy in the field by virtue of their endowment (volume and structure) in capital, they tend to act either toward the preservation of the distribution of capital or toward the subversion of this distribution’ (Bourdieu in Wacquant 1989, p. 8).

Hamati-Ataya confirms that the strategies of agents ‘depend on the position they occupy in the structure of the field ... which through the mediation of the constitutive disposition of their

habitus ... inclines them either to conserve or to transform the structure of this distribution' (Bourdieu 1994, p. 71 cited in Hamati-Ataya 2012, p. 632). The field to which Hamati-Ataya refers is the academic field of IR (2012, p. 631). By applying Bourdieu's concepts of strategy and habitus to the academic field of IR, I claim that social agents, or grand theorists, seek to gain intellectual capital in one of two ways; one, by emulating the success of those regarded to be in possession of such capital and by so doing act toward the preservation of IR's distribution of intellectual capital; two, by acting towards the subversion of this distribution. I apply Bourdieu's concepts of strategy and habitus to the academic field of IR as they are understood primarily in *Homo Academicus* (1988) and support my analysis where applicable using Bourdieu's other mentioned key texts (2013; 1998). I argue that Ripsman et al. (2016) seek to emulate the success of those regarded to be in possession of intellectual capital, and that Buzan (2004) and Lebow (2008) act towards the subversion of IR's distribution of intellectual capital. I begin by examining the case of Ripsman et al.

The Strategy of Ripsman et al.

Mearsheimer and Walt have noted (and as contained in this conclusion's epigraph) that 'Theory is the lodestone in the field of International Relations ... Its theorists are the field's most famous and prestigious scholars' (2013, p. 427). Indeed, 'virtually all the classic IR books are theory-laden works such as 'Wendt's Social Theory of International Politics' and 'Kenneth Waltz's Theory of International Politics' (Mearsheimer and Walt 2013, p. 427), and such theorists have become synonymous with particular paradigms in IR: the constructivist paradigm in the case of Wendt and the realist paradigm in the case of Waltz. Hamati-Ataya also notes that 'IR "classics" are always works in/on theory' and that 'paradigms' are predominant in the discipline (2012, p. 641). I claim that Ripsman et al. have engaged in a particular strategy to acquire intellectual capital within the academic field of IR. Ripsman et al. develop type III neoclassical realism as a

“multiparadigmatic” paradigm to emulate the existing intellectual capital of, for example, Wendt and Waltz as figures synonymous with leading paradigms in the discipline.

The development of a “multiparadigmatic” paradigm is necessary for Ripsman et al. for two reasons; one, to unite the work of neoclassical realists who use variables drawn from different paradigms via ‘the assumptions about foreign policy and international politics that we identify in this book’ (2016, p. 10); two, to provide ‘a better overall theory of international politics’ (Ripsman et al. 2016, p. 88) than structural realism, liberalism and constructivism (Ripmsan et al. 2016, p. 7). Ripsman et al. aim to develop a leading paradigm of IR by including, building upon and going beyond the variables of existing paradigms while becoming the standard bearers for a single neoclassical realist paradigm. Type III neoclassical realism, as a “multiparadigmatic” paradigm, is supposedly better than leading paradigms but, as a single paradigm, it can integrate into IR’s paradigmatic structure. Ripsman et al. develop type III neoclassical realism as a grand theory to offer a theory of comparable significance to those of Waltz and Wendt which can gain a place at IR’s leading table of theory. Ripsman et al. want a place alongside theorists such as Waltz and Wendt to become some of ‘the field’s most famous and prestigious scholars’ (Mearsheimer and Walt 2013, p. 427). Ripsman et al. act toward the preservation of IR’s distribution of intellectual capital by attempting to carve out a space within IR’s paradigmatic structure for their neoclassical realist paradigm. Ripsman et al., by conforming to IR’s disciplinary structure, are putting themselves ‘in the right’, and are attempting to beat IR ‘at its own game’ (Bourdieu 2013, p. 22). Ripsman et al., ‘In abiding by the rules’, by ‘falling into good form’ and by giving ‘apparent satisfaction to the [paradigmatic] demands [parenthesis added]’ of IR, attempt to gain ‘prestige’ (Bourdieu 2013, p. 22). For Bourdieu, as for Ripsman et al., there may be ‘a clear advantage that lies in abiding by the rules’ (Bourdieu 2013, p. 22). Although it is too soon to tell in the case of Ripsman et al. whether they will become some of IR’s leading names, and whether they will sit

with Waltz, Wendt and Keohane at IR's leading table of theory, their pursuit of prestige so far has been a 'smashing success' (Narizny 2017, p. 186).

The Strategies of Lebow and Buzan

In contrast to Ripsman et al. (2016), neither Buzan (2004) nor Lebow (2008) sought to establish a paradigm which could integrate into the discipline's paradigmatic structure. On the contrary, Lebow and Buzan were perturbed by the theoretical fragmentation paradigms facilitated. While, for Lebow, 'realism, liberalism, Marxism, constructivism' in addition to 'The English School, feminism, pragmatism, cognitive psychology, sociological institutionalism and philosophical realism' have brought about 'intellectual diversity and encourage[ed] intellectual honesty [parenthesis added]', such paradigms 'also brought about considerable fragmentation of the field' (2008, p. 35). Buzan, in a comparable manner, questions 'the prevailing tendency to assume that theoretical fragmentation constitutes an inevitable state of affairs that we should either endure or embrace' (Buzan and Little 2001, p. 19). For Buzan, 'scholars have taken too much pleasure in the pursuit of competing programmes that fragment theories into rival camps' (Buzan and Little 2001, p. 31).

By constructing grand theories which account for IR's mainstream paradigms by operating at a greater level of abstraction to such paradigms, I claim that Lebow and Buzan also engage in a particular strategy to acquire intellectual capital within the academic field of IR. Lebow and Buzan work against the distribution of intellectual capital enshrined across IR's paradigms by crafting approaches which include and go beyond them. Bourdieu claims that efforts of unification are 'all the more likely to succeed', and translate into the accrual of intellectual capital, 'if the social agents on which it is excreted are more inclined ... to mutually recognize each other and recognize themselves in the same project' (1998, p. 33). Both Buzan and Lebow, as illustrated in my respective papers, went to great lengths to demonstrate how theorists of IR's

paradigms could find a place within the grand theories they constructed, and to illustrate how they could all contribute to the research programmes they facilitated. I argue that Lebow and Buzan do not aim to emulate the prestige of Waltz or Wendt, but the intellectual capital of a theorist who found ‘the holy grail of a universal theory of international politics’ (Lake 2011, p. 466): a single and universally applicable theory for all IR. Although, to use the language of Bourdieu, it is overstatement to suggest that Lebow and Buzan aimed to “subvert” the discipline’s distribution of intellectual capital (Bourdieu in Wacquant 1989, p. 8), they certainly sought to undermine it.

As Hamati-Ataya has illustrated, ‘the narrative required to justify and legitimate anti-mainstream discourse is one of “crisis” (2012, p. 637). Bourdieu claims that ‘Crisis is a necessary condition for the questioning of’ the status quo (2013, p. 169). Buzan and Lebow have each claimed that ‘there is something fundamentally wrong with the discipline’ in the form of theoretical fragmentation ‘that needs to “urgently” be addressed’ (Hamati-Ataya 2012, p. 637). In the case of Buzan, theoretical fragmentation ‘is something that undermines’ the discipline’s ‘very identity and vocation’ (Hamati-Ataya 2012, p. 637). Nowhere is this more apparent than in Buzan and Little’s article, ‘Why International Relations has Failed as an Intellectual Project and What to do About it’ (2001). The task of Buzan and Lebow’s ‘narrative lies in presenting an alternative that is not merely “different” from, but somehow ‘superior’ to, the dominant view, whose flaws are therefore portrayed as resulting from some sort of epistemic deception’ (2012, p. 637). It is by refusing to recognise the epistemological possibility of unification encapsulated in grand theory that, for Buzan and Lebow, has led to theoretical fragmentation for which they each present their grand theories as the solution. By being the architects of such grand theories, Buzan and Lebow do not seek to become synonymous with a particular paradigm in IR, and thus become *one of* the field’s leading names, they each sought to become *the* leading name of

the field by attempting to construct the grail of a universal theory of international politics. In what follows, I illustrate in greater depth the respective strategies of Buzan and Lebow.

Lebow's Strategy

Lebow describes a grand or general theory 'as something akin to a unified field theory in physics' because 'A good theory of international relations presupposes a good understanding of politics' across 'multiple levels of social aggregation' (2008, p. 1). Lebow notes that 'Einstein devoted his mature decades to' the development of a unified field theory 'and failed, as anyone would in the absence of more knowledge about the individual forces that have to be subsumed by a general theory' (2008, p. 1). The forces to which Lebow refers are the fundamental forces of physics. In the manner that Einstein sought to unify the fundamental forces of physics or, more accurately, theories about the fundamental forces of physics into a single theory, Lebow aims to establish a good understanding of politics across all relevant levels of analysis by bring together existing theories at each level of analysis. Lebow aims to establish such an understanding through his project of grand theory. In my Lebow paper, I emphasised that Lebow adopts a 'layered' (2008, p. 6) approach by explaining 'the particular by reference to the general' (2008, p. 4). At the international level of analysis, Lebow's cultural theory 'account[s] for all existing paradigms of international relations' (2008, p. 114). Lebow aims to craft a theoretical framework which can be used to understand all aspects of politics in the manner that Einstein sought a theoretical framework for all aspects of physics (a contemporary contender of which, for example, is string theory). Although Lebow is not basing his cultural theory on the work of physicists, he is drawing an analogy between unified field theory and the kind of theory he is looking to establish (2008, p. 1). If Lebow's cultural theory could 'generate a consensus' (Little 2009, p. 115) as the 'general theory on which the study of international relations can build' (Little 2009, p. 108), Lebow would achieve for the study of politics what Einstein failed (and what his contemporaries are failing) to

do in physics. Lebow would have found, and he would have been accepted as finding, ‘the holy grail of a universal theory of international politics’ (Lake 2011, p. 466). Lebow would have accrued enough intellectual capital to not just be *one* IR’s leading figures (as he already was and is) but *the* leading figure of IR.

Little has illustrated (and as discussed in my Lebow paper) that Lebow is following in a tradition of previous attempts to establish an ‘agreed general theory’ by ‘endeavouring to provide a general theory on which the study of international relations can build’ (2009, p. 108). The tradition to which Little refers was originally conceptualised by Hoffmann as ‘The Long Road to Theory’ (1959). Hoffmann claimed that there were two prominent ‘general theoretical efforts’ in the discipline (1959, p. 349). The first was ‘Professor Morgenthau’s “realist” theory of power politics’ because ‘It tries to give us a reliable map of the landscape of world affairs’ (Hoffmann 1959, p. 349). Morgenthau’s ‘master key’ is ‘the concept of interest defined as power’ (Hoffmann 1959, p. 349) because ‘power relations reproduce themselves in’ a ‘timeless’ or universal manner (Hoffmann 1959, p. 350). Morgenthau claims that ‘power is universal in time and space’ (1948, p. 17). Hoffmann concludes that Morgenthau’s ‘map is inadequate’ because “power monism” does not account for all politics’ (1959, p. 350), and that ‘The consequence of this inadequacy of the map is that the theory’s usefulness as a general theory for the discipline is limited’ (1959, p. 352). In addition, Hoffmann also cites the work of Deutsch as inadequate for the basis of a general theory because ‘the map’ it produces ‘does not allow us to recognise the landscape’ of IR (1959, p. 359). Deutsch, as a systems theorist, builds on ‘shaky foundations of metaphors taken too seriously’ such as ‘communication systems’ which seeks ‘to interpret the behaviour of all systems’ (Hoffmann 1959, p. 359). Hoffmann recommends ‘a more modest and slow way of proceeding toward theory’ (1959, p. 359) in the form of “historical sociology” (1959, p. 367) and ‘a philosophy of international relations’ as normative theory (1959, p. 376). In distinction from a general theory, Hoffmann does not ‘claim that it is possible to squeeze the whole camel of

international relations through the eye of one needle' (1959, p. 359). Little shows that Lebow refuses to 'follow Hoffman's injunction to eschew the pursuit of general theory' (2009, p. 107). Lebow states that 'Hans Morgenthau' and 'Karl Deutsch' were two of his 'former mentors' (2018, p. 7). Lebow is following in the tradition of his former mentors to establish 'a universal theory of international politics' (Lake 2011, p. 466), and thereby seeks the prestige that such a theory would confer among those who value what it offers. Lebow attempts to gain a 'sufficient amount of ... capital to be in a position to dominate the' academic 'field' of IR (Bourdieu 1998, p. 34).

Buzan's Strategy

If Buzan could successfully bring together IR's 'paradigm-warring islands of theory ... into the imperial or federative archipelago of theoretically pluralist grand theory' (2001, p. 31), Buzan could 'authoritatively guide inquiry, help organize research agendas' and help 'to provide criteria for developing reading lists for undergraduate and graduate studies' (Holsti 1985, p. 5). I claim that Buzan is following in a previous tradition to establish a general theory of IR identified by Holsti in 1971. In his article 'Retreat from Utopia' (1971), Holsti writes that his 'main concern is to analyse the unifying characteristics, assumptions, and rise and decline of what might be called "grand theory" (1971, p. 165). For Holsti, "The utopia the authors of these works have sought is a general theory of international politics', the names of which include: David Easton (1965), Karl Deutsch (1963), Quincy Wright (1955) and Hans Morgenthau (1948) (Holsti 1971, p. 166). According to Holsti, 'An important epistemological assumption' of such "grand theorists" is 'that the diverse data of the field, the sources of state behaviour, and the international patterns and recurrences could be integrated, explained, or described in a single theory, model, approach, or framework' (1971, p. 170). The contributions of these grand theorists for Holsti 'have been (in most cases) inclusive, offering organizing devices for the entire field, not only portions of it', assuming 'that they could bring together the essential, if not all, the animals of world politics into

one theoretical ark' (1971, p. 170) or establish 'a single general theory or scheme for organizing the field' (1971, p. 171). Holsti clarifies, however, that 'no framework or theory which has aspired to acceptance as *the* approach to the field' has been successful as such¹ (1971, p. 171). By trying to fulfil an ideal for theory identified by his mentor, Buzan is following in an existing tradition to establish a general theory of IR and thereby gain the prestige than an accepted general theory would confer. Buzan's social structural approach (2004) would become the leading theory of the field not only in terms of research but for education. If Buzan was successful, he would accrue intellectual capital beyond that endowed to Waltz and Wendt by becoming the leading theorist of IR. Although Ripsman et al. (2016), Lebow (2008) and Buzan (2004) construct grand theories because they aim to gain prestige, Lebow and Buzan seek an entirely different kind of prestige to that of Ripsman et al.

The Habitus' of Ripsman et al., Lebow and Buzan

For Bourdieu, 'The way one accedes to a position is inscribed in habitus as a system of durable and transposable dispositions to perceive, evaluate, and respond to social reality' formed by one's experience within a respective academic field (Bourdieu in Wacquant 1989, p. 10). Bourdieu clarifies that 'Social agents are the *product of history*, of the history ... of the accumulated experience of a path within the specific sub-field' (Bourdieu in Wacquant 1989, p. 10). To 'understand what such and such a professor will do' one must consider 'what position he occupies

¹The remaining authors included in Holsti's list (1971, p. 166) are as follows (in no particular order): Snyder (1963), Liska (no text cited), Organski (1958), Kaplan (1957), Burton (1965), Aron (1966) and Rosenau (1971). Holsti lists the following authors as systems theorists who 'also belong to this group' (1971, p. 166): McClelland (1966), Scott (1967), Galtung (1967; 1964), Spiro (1966) and Rosecrance (1963). See Holsti, K. 1971. Retreat from Utopia: International Relations Theory, 1945-1970. *Canadian Journal of Political Science* 4(2): 165-177.

in academic space' and 'their accumulated experience within such space' (Bourdieu in Wacquant 1989, p. 10). Eagleton-Pierce reiterates that it is important to develop an 'awareness of how [IR] academics, like other cultural interpreters, owe something to their position in a social [academic] space [parenthesis added]' (2011, p. 816). Ripsman et al., Lebow and Buzan each occupied advantageous positions from which to gain the prestige they sought. Ripsman, Lobell², Lebow and Buzan were Professors when their respective texts containing their grand theories were published, and Ripsman et al., Lebow and Buzan had previously made substantial contributions to the discipline of IR (as the introduction to each of my papers makes apparent). Each theorist was already regarded to be competent (Bourdieu 1988, p. 63), and to be in possession of significant intellectual capital before they constructed their grand theories. Ripsman et al., Lebow and Buzan were able to use such existing positions 'in academic space' (Bourdieu in Wacquant 1989, p. 10) in an effort to accede to the higher positions of prestige they arguably sought.

The academic experiences of Ripsman et al., Buzan and Lebow were, however, markedly different. Buzan took as his mentor Kal Holsti (2016), Lebow took as his mentors Karl Deutsch and Hans Morgenthau (2018, p. 7) and Ripsman et al. took as their mentor Ben Frankel (2016, p. iv). Ben Frankel 'introduced' Ripsman, Taliaferro and Lobell, 'and encouraged' them 'to explore' their 'common interests in enriching realism' (2016, p. iv). Due to his experiences with Kal Holsti, Buzan followed in a tradition of previous attempts to establish a general theory of IR and thereby to gain prestige. Due to his experiences with Morgenthau and Deutsch, Lebow likewise followed in a previous tradition to establish a general theory of IR and thereby to gain prestige. By introducing Ripsman, Taliaferro and Lobell, and by encouraging them to enrich realism, Frankel was the arguable Ur-source of Ripsman et al.'s "multiparadigmatic" paradigm which I claim they constructed as a vehicle to obtain prestige. Ripsman et al. sought to enrich

² Jeffrey Taliaferro was promoted to full Professor in 2020. See, Tufts University. Jeffrey Taliaferro, <https://facultyprofiles.tufts.edu/jeffrey-taliaferro>

realism by including variables from other paradigms into a single neoclassical realist paradigmatic framework.

The habitus' of Ripsman et al., Buzan and Lebow were connected to 'their position' in academic space (Hamati-Ataya 2012, p. 640), and each met with the 'given' distribution of intellectual capital in IR (Hamati-Ataya 2012, p. 644). The respective habitus' of Ripsman et al., Lebow and Buzan gave rise to particular 'scholarly judgements and interests' (Eagleton-Pierce 2011, p. 815) to pursue grand theory as vehicle to obtain prestige. Grand theory, in the cases of Ripsman et al., Lebow and Buzan, is, in part, a 'production of theoretical knowledge [which] results from the meeting of different socio-academic habitus and their associated positions with the' [parenthesis added]' (Hamati-Ataya 2012, p. 621) given distribution of intellectual capital in IR. Ripsman et al., Lebow and Buzan formulated their strategies to gain prestige because of their particular positions and experiences within the discipline of IR relative to the field's distribution of intellectual capital.

Grand Theory Is a Chimera

In the three cases of grand theory I examine in this dissertation, two prominent theorists have abandoned their grand theories while the other prominent theorists have retained their commitment to grand theory through dogma. Buzan's abandonment of grand theory has significant implications for the English School. The English School is a major approach to the study of IR which Buzan "reconvened" in 1999. Buzan as the architect of the English School's reconvention has abandoned its central mission to construct the English School as a grand theory. Because Buzan has abandoned grand theory, Buzan's reconvention project is now somewhat bereft of purpose and leaves his wing of the English School in need of a serious rethink. Lebow's abandonment of grand theory is important because his cultural theory (2008) is a significant approach in IR. Lebow's abandonment of grand theory should also give everyone currently

engaged with the wider research programme facilitated by his grand theory pause for thought. If such researchers continue to orient their work in terms of Lebow's grand theory, they will do so when its author has put it to one side. Ripsman et al.'s retainment of grand theory through dogma has significant implications for neoclassical realism. Anyone engaged with the paradigmatic version of neoclassical realism promulgated by Ripsman et al. will also have to rethink their position, and they will have to question the nature of their contributions. As illustrated in my neoclassical realism paper, type III neoclassical realism does not offer an epistemologically sound basis from which to explain foreign policy or international politics. Consequently, anyone who uses Ripsman et al.'s theoretical framework to explain foreign policy or international politics cannot offer coherent explanations of either. In the three cases examined, grand theory is deeply problematic and brings into question the utility of grand theory in general. Despite the claimed usefulness of grand theory by its proponents, grand theory in the cases of Buzan (2004) and Lebow (2008) was only partly useful as a heuristic device and problematic in the case of Ripsman et al.

Furthermore, in addition to grand theory's supposed benefits, I argued that authors are motivated to pursue grand theory because of the prestige that it may confer. In the cases examined, I argued that theorists constructed grand theories in an effort to gain prestige which they either later abandoned or hung onto through dogma. If grand theory can be partly understood as an effort to gain prestige, it suggests that Buzan (2004) and Lebow (2008) did not gain the prestige they sought because they abandoned the very vehicles through which they hoped to obtain it. Buzan (2004) and Lebow (2008) constructed sophisticated grand theories in which IR's mainstream paradigms could be accounted and through which one could arrive at a universally applicable theory of IR. It is curious why Buzan (2004) and Lebow (2008) did not obtain the prestige they arguably sought because they could each be credited with constructing a

'holy grail of a universal theory of international politics' (Lake 2011, p. 466). This was a question I left for further research in my first and second papers but is now one I attempt to answer below.

Why Didn't Buzan and Lebow Obtain the Prestige They Sought?

I claim that Buzan (2004) and Lebow (2008) did not obtain the prestige they arguably sought because grand theory is a floating signifier. If it cannot be said with any degree of particularity or specificity what a grand theory is, it cannot be ascertained that either Buzan (2004) or Lebow (2008) found the 'holy grail of a universal theory of international politics' (Lake 2011, p. 466). There is no agreement on what the holy grail is, meaning, consequently, that it cannot be found. It is not possible to be accorded the prestige of a theorist who found the holy grail of international politics when nobody can say what such a holy grail looks like. Buzan and Lebow were, however, accorded the intellectual renown of authors who made substantial contributions to the discipline of IR (as the introductions to my first and second papers makes clear). Buzan, for example, won BISA's Distinguished Contribution Prize in 2016 (BISA 2021) for contributions such as *From International to World Society* (2004), and Lebow won the Distinguished Scholar Award from the International Security Section of the ISA (Reich 2017, p. xiv) for contributions such as *A Cultural Theory of International Relations* (2008). Buzan and Lebow have gained the prestige of theorists who constructed impressive theories rather than as theorists who found the grail.

The Continued Attraction of Grand Theory in IR

Despite the fact that grand theory is a floating signifier, prestige may nevertheless help to explain the continued attraction to develop grand theories in IR (e.g., Kitchen 2021; Desmaele and Onea 2021). In the cases of Buzan and Lebow, they developed grand theories to obtain a particular kind of prestige when there is no agreement on what a grand theory is, and therefore what the

holy grail of a universal theory of international politics looks like. Despite there being no agreement on what a grand theory is in IR, Buzan and Lebow's proposals of grand theory nevertheless (and as discussed above) gained Buzan and Lebow the prestige of theorists who constructed impressive theories. In addition, Ripsman et al. seem to be in the process of achieving the prestige they sought by becoming names synonymous with what is now being increasingly recognised as a paradigm of IR (Narizny 2017). Above all else, and despite the problematic nature of grand theory in IR, theorists may nevertheless be motivated to pursue grand theory because of the prestige that it can, and arguably does confer, if albeit not in the manner desired by Lebow and Buzan. While it is too soon to tell if type III neoclassical realism will become a leading paradigm of IR, the potential for it to do so is certainly visible. As Hamati-Ataya has suggested, the efficacy of theories is 'not solely' determined 'on the basis of their intrinsic logic', but also by the strategies of their authors (2012, p. 640). Any effort to problematise type III neoclassical realism by 'pointing out its incoherence' may never be fully 'sufficient' (Hamati-Ataya 2012, p. 640) because of the successful 'game' Ripsman et al. seem to be playing thus far within the field of IR's *given configuration* of intellectual capital (Hamati-Ataya 2012, p. 631). Even though grand theory is a floating signifier without a stable meaning, a grail for theory that can never be found, and in the case of Ripsman et al. (as my neoclassical realism paper illustrates), incoherent, grand theory may nevertheless beguile theorists because of the prestige that it can confer.

Overall, grand theory is a deeply problematic form of theory which has significant implications for neoclassical realism, followers of Lebow, the English School and the utility of which can be questioned across the academic discipline of IR. While Ripsman et al., Lebow and Buzan constructed grand theories because they in part sought the prestige a grand theory would confer, Buzan and Lebow sought a particular kind of prestige. Buzan and Lebow could never obtain the prestige they sought because the holy grail of grand theory cannot be found. Despite the

problematic nature of grand theory in IR, however, theorists may nevertheless be motivated to pursue grand theory because of the potential prestige that it can confer. Grand theory in the particular cases of Ripsman et al., Lebow and Buzan, and arguably in the wider discipline, is a chimera. One final set of questions, however, looms over this dissertation which I am frustratingly unable to answer. If nobody can agree on what the holy grail is, and if the holy grail of grand theory consequently cannot be found, was there ever a grail of grand theory to find in the first place? How did grand theory emerge in IR? Was grand theory always a floating signifier? I suspect that there never was a grail of grand theory to find, and that grand theory was always a floating signifier. I leave such questions, however, in the hands of those far more capable than I for further research.

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