ANALYTICAL ESSAY

Induction and Deduction in International Relations: Squaring the Circle Between Theory and Evidence

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International Relations (IR) scholarship represents the pursuit of theory about how the world works or ought to work. Yet despite this broad orientation of the field, the process of theory building itself remains challenging. There is abundant professional guidance on how to refine and test a theory once it has been created, but how do we get from a vague interest in some area of IR to a specific argument that explains or interprets a particular aspect of international politics? This article aims to refine the theorization challenge by taking stock of—and advancing—the debate over the relationship between inductive and deductive inference in IR theory building. It begins with an analysis of the short shrift still given to induction by many in the positivist IR community—a tendency that follows from the criticism of inductive theory building contained in Kenneth Waltz's *Theory of International Politics* (1979) and its enduring legacy, despite Waltz's own professedly nonpositivist approach—before progressing to consider the limits of a purely deductive approach to theorization. This article concludes that "creative-deductive" theory possesses key strengths, including its parsimonious and generalizable explanatory reach, and its truth-preserving quality. However, given the endogenous feedback relationship that exists between theoretical premises and empirical evidence, this article argues that it is only once we accept that induction is an intrinsic and unavoidable component of IR theory building that we can truly seek answers to the most important questions of international politics. Accordingly, IR scholars should strive to use the two in tandem as they set about developing new theory.

Keywords: international relations theory, induction, deduction

International Relations (IR) scholars are in the business of theorizing. Like all social scientists, our "value-added" vis-à-vis our more glamorous counterparts in international journalism lies in our ability to make sense of the political structures and social meanings behind the headlines (Hollis and Smith 1990, 1–2).[1] Theories can be positive, interpretive, normative, or even critical of "theory" in the sense that much of the IR community uses the term, yet the discipline is nevertheless united by its preoccupation with going beyond the mere description of

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[1]For a similar argument made in the context of a different academic field, see Baggini (2003).

international events.[2] Despite this consensus over the utility of theory in improving our knowledge of international affairs, however, it is far less clear how the process of building IR theory should actually proceed. Indeed, while literature on the methodology of theory testing has proliferated exponentially in the last two decades, theory *building* remains something of a black art. To be sure, there is abundant—and vigorously contested—professional guidance on what criteria good political-scientific theories should meet once they exist.[3] But how do we actually get from a vague interest in some area of IR to a specific argument that explains or interprets a particular aspect of international politics (the step prior to KKV's refinement process)? Addressing this question is the most difficult task faced by any IR scholar at the start of a research project, but it is also the task that determines the project's ultimate value.

The purpose of this study is to take stock of, and advance, the debate over how this challenge can best be handled, by scrutinizing the inference relationship that exists between empirical evidence (the "real-world" events that trigger our interest in an area of IR) and the theories that we use to explain them. This article argues that although deductive inference is a valuable truth-preserving component of the theory-building process, it is only once we recognize that a measure of induction from empirical observations is necessary as a method for finding a "starting point" for subsequent theorization that IR scholars can obtain answers to the most pressing questions in international politics.[4] Indeed, deductive scholars who deny the utility of induction as a component of the theorization process while advocating the empirical testing of theoretical premises actually open the door to induction, given that observation of prior empirical data—particularly data generated in the course of past empirical tests—functions as a selection rule for new theoretical ideas.[5] This article does not attempt to defend contemporary IR's preoccupation with data mining, hypothesis testing, and "flat-footed empiricism" (Monteiro 2014, 20) at the expense of theorization (Mearsheimer and Walt 2013)—research failings that are often justified as "inductive"—and nor is it anti-deduction. On the contrary, it seeks to afford theory the explanatory primacy that it merits, and to identify the proper places of inductive *and* deductive inference within that.[6]

Considering how IR theory should be related to evidence, and vice versa, allows us to demystify the theory-building process and thereby attain a more systematic

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[2]To be sure, there are empirical—particularly quantitative—scholars who declaredly eschew generating their own theories, but even they must test hypotheses (even if generated by other scholars) if they are to derive interesting findings from their data. Moreover, the avoidance of theoretical micro-foundations in choosing quantitative estimators when building statistical tests—always a questionable approach—now enjoys less methodological tolerance even among other quantitative scholars (Achen 2002, 440–41).

[3]For one seminal example, see King, Keohane, and Verba (1994, 99–114) (hereafter "KKV"); for a recent KKV's critical account, see George and Bennett (2005).

[4]Deduction is defined as "top-down" reasoning whereby, if the premises are held to be true (and the terms in use precisely specified), then the conclusion follows as a logical necessity, based on the reductive application of covering laws to a closed domain of discussion. Induction, by contrast, is defined as "bottom-up" reasoning, whereby a conclusion is reached via generalization or extrapolation from initial information. Thus, "all P are Q; R is P; therefore, R is Q" is a deductive argument; "most P are Q; R is P; therefore, it is highly likely that R is Q," by contrast, is an inductive argument. Further elaboration in the context of IR theorization follows later in this article.

[5]This contention is not intended to be the final word on the matter but rather a spur to further discussion.

[6]Note too that the focus is on explanatory theory—that is, the sort of theory that seeks to identify the causal processes that purportedly bring about certain international outcomes—although application to the field of interpretive theory could conceivably represent an extension of the argument. This article thus adheres broadly to the notion of a world about which things can be known external to the knower—mind-world dualism—and is thus aligned primarily with positivist and critical/scientific realist ontological positions (Jackson 2011, 196–201), although this is certainly not to claim that other approaches are less worthwhile. Indeed, to the extent that interpretive research is best understood as a continual circulation, starting from a puzzle and working back and forth between theoretical conceptions and empirical material, its approach corresponds closely to that advocated here—although its emphasis on exploring specific contexts rather than explaining generalizable patterns is not the focus of this article (Heaven 2012, 5).

ability to perform this important task, as well as shed light on the nature of theory and the metrics by which a theory's value should be assessed. The argument therefore sits at the nexus of pedagogy, methodology, substantive theory, and philosophy of science: it considers how to "do" explanatory IR theory and, in the process, provides at least a few thoughts on the nature of IR theory itself.[7]

The argument advanced in this article is centered around IR as a subfield of political science, beginning with the intellectual legacy of Kenneth Waltz's *Theory of International Politics* (1979), the impact that it has had on theoretical development in the discipline, and recent reformulations and rebuttals of the Waltzian position.[8] The particular focus is Waltz's argument that good theory must not rely on induction in its embryonic stage if it is to tell us what really drives international politics—a finding that is actually much more nuanced and circumscribed in Waltz's own work than many of his subsequent admirers have implied.[9] *Theory*'s argument has profoundly influenced the positivist-inclining IR community since its publication—ironically, given Waltz's self-declaredly non-positivist approach to theorization—and its legacy continues to cast a long shadow today. This is not least because other trends in the field, such as the influence of KKV's approach to methodology, continue to reinforce it, skewing theory toward false professions of "purely" deductive underpinnings. There is certainly merit in the position of induction's critics: by itself, induction can struggle to produce generalizable results and risks degeneration into a "laundry listing" approach (Achen and Snidal 1989, 155), in which a multitude of factors are assumed to have causal importance while failing to identify the relative salience of particular variables. At its worst, moreover, claims to induction risk being used as mere window dressing for atheoretical data mining. Yet despite this, the article goes on to demonstrate that even the most strident deductive IR scholars find that a large dose of concealed induction is unavoidable in their analysis, and the field is enriched when this is admitted openly. Indeed, as most scholars—Waltz included—have long recognized, theory and evidence exist in an endogenous feedback relationship in which our grasp of each is informed by the other. This leads to the conclusion that anyone hoping to build good IR theory must proceed with an awareness that they need to engage in both deduction *and* induction, and that each can precede the other: we need a sense of what is empirically interesting before we look at causation, but we need a sense of causal order before we assess the importance of specific events.

In short, IR theorists are free to work backwards from empirics to theory, provided that they also work forwards from the ensuing theory to reassess the significance of evidence, and vice versa: a true fusion of inductive and deductive inference. Moreover, Waltz's charge that induction should be treated skeptically because it cannot show us "objective reality" misses the point that the value of an IR theory should be assessed by how well it explains events in the real world.[10]

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[7]Rudra Sil and Katzenstein (2010) have recently made a case in favor of analytic eclecticism as a means of moving beyond the meta-theoretical rigidity of paradigm debates in a way that frees IR scholars to utilize an array of concepts in addressing important questions of international politics, navigating between the ideal-type "poles" of theory. The argument presented here echoes this call but at a higher level: while such poles must be identified, it is only once an appreciation of the utility of meta-theoretical pluralism is reached in the context of the induction-deduction debate that IR can connect most effectively to real-world issues worthy of analysis (see also Jackson 2011, 188–212)—"epistemic ecumenism" might be an equally appropriate characterization.

[8]Exploring such a field-defining exemplar gives practical meaning to the flexible and eclectic approach to combining induction and deduction described above; the rationale for paying yet more attention to an already extensively analyzed treatise will be explained at length subsequently.

[9]This is not to deny that the debate over the relative merits of inductive and deductive theorization has long resonated throughout the wider vista of social science. The point here, however, is to advance the debate insofar as it pertains to those engaged in *IR* theory building—and given the particular implications of the induction/deduction debate for contemporary IR scholarship, the subject merits consideration in its own right.

[10]A measure of value that Waltz himself subscribes to (1997, 913–17; 2004, 2–6).

The search for some objective truth criterion is thus likely to be fruitless, but it is also unnecessary.[11]

Neorealism's Creative-Deductive Legacy: Building Theory, Not "Reality"

Kenneth Waltz's *Theory of International Politics* has arguably defined the academic study of IR since its appearance in 1979, insofar as even his most strident critics have felt obliged to address—if only to refute—the model of the international system that he presents (Burchill 1996, 83; Wendt 1999, 2, 15). Yet as influential as the neorealist international theory that Waltz's text puts forth have been the book's conclusions on the broader philosophy of social scientific inquiry as it applies to IR (Waltz 1979, 1–17). *Theory* is as much a treatise on what an IR theory should look like and how it should be formulated as it is an expression of Waltz's own substantive argument. In particular, Waltz's influence within the IR community is owed largely to—and, in turn, further bolstered—the allure of analytically deductive scholarship. Such an approach has appeared to offer the prospect of turning the study of IR into "real" science, by generating logically coherent theories of how the world works and then testing their explanatory power via empirical observation, just as a natural scientist would do.[12]

The principal target of Waltz's critique of prior IR scholarship is its over reliance on inductive theory building (Waltz 1979, 2–5; Wæver 2009, 203). This is not to say that he denies a valuable role for induction in theory refinement, as will be discussed at greater length below, but he does not see it as an adequate starting point for theory. In this vein, he is critical of students of politics who simply study more and more cases in the hope that causal connections will eventually present themselves, on the grounds that "we can never say with assurance that a state of affairs inductively arrived at corresponds to something objectively real" (1979, 5). It is this finding that leads Waltz—echoing Milton Friedman's F-twist argument on the role of assumptions in economic theory—to one of his most striking conclusions: "Explanatory power…is gained by moving away from "reality," not by staying close to it" (1979, 7).[13] By this, he means that IR scholars should construct parsimonious, internally consistent theories "creatively"—that is, by relying on intelligently and intuitively derived starting assumptions—before tracing through the logical implications of these assumptions in an analytically deductive manner (1979, 9). Deduction alone is insufficient in generating a helpful theoretical lens through which to view the world, since the process of deduction requires a starting point, and it is this starting point that smart theorists must generate for themselves, hence the need for "creativity" in developing theory ("a picture, mentally formed") (1979, 8,11). Yet it is imperative that we resist the temptation to replace creativity with empirical data since, in the absence of a clear theoretical

[11]The argument accordingly echoes Bruce Russett's near-50-year-old call (1969, 87–94) for both deduction and induction to have a legitimate role in the IR theory-building process but goes further in arguing for an end to seeing them as alternative approaches to theorization (for the original counterpoint to Russett, making the case for the merits of deduction and the perils of inductive reasoning in IR, see Young [1969, 486–511]). Indeed, despite the visible circularity of the induction–deduction connection, this article contends that the interaction between theorizing and empirical observation represents an opportunity for us to further our knowledge of international politics rather than an insurmountable problem. All that is required is that scholars have the humility to see their theoretical contribution as part of an ongoing process rather than as a revelation of final truth. On induction and deduction's inherent compatibility in IR theory, see Wagner (2007, 2–8).

[12]On the legitimating appeal of the "science" label, see Wight (2002, 25) and Jackson (2011, 189). Note that, as will be discussed further below, Waltz's influence on this front owes more to how his work has been interpreted by the wider field than to his own actual approach to theorization, given that Waltz advocated measuring a theory's merits on the basis of its analytical and logical strength rather than by relying wholly on empirical tests.

[13]On Friedman, the so-called F-twist and the role of "unreal" assumptions in economic theory, see Musgrave (1981, 377–87). On the perils posed for the relevance of social science by the accumulation of unrealistic assumptions within a field of inquiry, see Kay (2012).

framework with which to organize our empirical study, the social world will remain too complex for us to know which data are most important (1979, 7).[14] In short, for Waltz, theory must be generated through a combination of the theorist's ideas and tracing the logical consequences of those initial assumptions. To be sure, the theory's subsequent rejection or acceptance may be conditional on its explanatory power (Waltz 1997, 913–17; Waltz 2004, 2–6), but if this is to be tested, it is tested only after the act of theorizing; induction from observed effects should not *itself* drive theory building.

Of course, Waltz concedes that theoretical models are abstract simplifications of the world that inevitably lose some richness of detail (2004, 2); indeed, this is their very value. Accordingly, his conceptualization of the way that theory should be utilized to elucidate and understand the real world appears to bear some similarity to that of another prominent structural realist thinker, who states that good theory should be "like a powerful flashlight in a dark room: even though it cannot illuminate every nook and cranny, most of the time it is an excellent tool for navigating through the darkness" (Mearsheimer 2001, 11).[15] In other words, according to Waltz and his intellectual progeny, IR theory should allow us to strip away the "white noise" of international politics—something that he would contend is impossible if we build theory only with reference to the very empirical events that constitute this "white noise"—allowing theorists to see the variables and factors that really matter.

It is important to note at this stage that—to use the labels properly—Waltz is a "rationalist" or a "systematist," rather than a "positivist," since his conception of theory necessarily gives causal importance to underlying structures and processes that are not directly observable (Hollis 1994, 25).[16] Indeed, the narrow empiricism traditionally associated with the term positivism would actually be more in keeping with induction than the deductive theory that Waltz advocates, and he himself eschews the "positivist" label (Halliday and Rosenberg 1998, 379–86; Waltz 2003, xii; Wæver 2009, 203–204). This is not least because he admits—and is not concerned by—the limited empirical testability of *Theory*'s version of neorealism (Guzzini 1998, 128–30). Furthermore, his emphasis on the role of the theorist's creative "flash of insight" bears more resemblance to the pragmatism of Grounded Theory (he even cites Charles Peirce) than to the strictly empiricist hypothesis testing of positivism (Waltz 1979, 4).[17] However, the important point is that much of the contemporary IR community—particularly much of the US-based IR community—has *taken Waltz to be* a positivist (Wæver 2009, 203–204, 216) and thus taken the guidance contained in *Theory*'s crucial first chapter to be a seminal manual for positivist IR theorization (for example, Monteiro and Ruby 2009, 17).[18]

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[14]See also Trachtenberg (2006, 17).

[15]Note here that because Waltz, like Mearsheimer, sees the international system as having little substance beyond the sum of its parts, he is in one sense an individualist—and therefore perhaps more accurately a "systemic realist"—than a structuralist (Guzzini 1998, 129; Wendt 1999, 15–16). However, insofar as Waltz's theory attributes causality primarily to the international-structural level, the assignation of the "structural realist" label remains justified.

[16]Indeed, following from the previous footnote, a key insight of *Theory* is that reductionist political science—theories that reduce systems and structures *solely* to their constituent agents and actors—cannot adequately account for international order and change (Goddard and Nexon 2005, 10).

[17]On grounded theory and the role it attributes to such flashes of insight, see Reichertz (2010, 6–9).

[18]This divergence between the traditional understanding of positivism and the way that contemporary IR scholars now tend to interpret the term leads Jackson (2011, 41–71) to dub the latter community "neopositivists." It is also worth noting that Waltz's view of the extent to which the merits of a theory should be assessed by empirical testing is not fully clear or consistent over time. Early in *Theory*, he identifies several requirements of theory testing (despite the rest of the book showing little interest in testing)—and lambasts many of his contemporaries for not meeting these requirements, principally because the theories that they are purportedly testing are not precisely enough specified in advance (1979, 13–16). Yet in his later work, he appears to come closer to following the "usual" (neo)positivist process of theoretical argument followed by empirical testing common to much contemporary IR

An obvious rejoinder to the discussion of Waltz's stance on induction conducted above is: "So what?" Does a treatise advocating creative-deductive IR theorization contained in a single, already extensively scrutinized, and nearing 40-years-old book merit renewed analysis in the second decade of the twenty-first century? Three features of the contemporary scholarly literature suggest that it does indeed. First, the impact of Waltz's creative-deductive approach is plain to see in foundational texts drawn from other leading schools of thought in IR theory. Neoliberal institutionalism, for instance, essentially subscribes to the Waltzian approach, illustrating that *Theory*'s influence is certainly not restricted to similarly inclined neorealists (Keohane 2005, X; Wendt 1999, 31: 137). Constructivism is more open to its inductive or abductive roots (Wendt 1999, 87) and, indeed, represents a broader theoretical church that includes declaredly non-explanatory approaches, which lie beyond the purview of this exchange. Yet even much constructivism—certainly of the explanatory, positivist flavored sort—adopts an essentially Waltzian view of theory building (Wendt 1999, 39).

Second, much of the best contemporary IR theory is still propagating the Waltzian deductive ideal, even when there is a visible inductive base. Charles Glaser's 2010 book, for instance—arguably one of the most sophisticated pieces of theoretical scholarship adopting a broadly neorealist perspective to date—is admirable in its fusion of material, informational, and motivational variables. Moreover, the very utility of Glaser's argument derives from its identification of a key explanatory gap in simpler power-centric versions of structural realism: specifically, the greater empirical prevalence of cooperative major power relations than competing realist accounts would seem to suggest is likely. Yet despite this inductive starting point, the book professes to offer only deductive theory (Glaser 2010, 6–13)—and, incidentally, criticizes Waltz for flawed deduction in the process. Another acclaimed recent work, Nuno Monteiro's *Theory of Unipolar Politics* (2014, 17–21)—to give but another high-profile example—does not *explicitly* claim the "pure" deductive mantle. Yet it certainly does so *implicitly,* by eviscerating induction at length (and lauding Waltzian deduction). Much of Monteiro's critique is well-founded and echoed by this article: particularly, its targeting of the poverty of mere hypothesis testing and data mining in the absence of creative theorization. It nonetheless means, however, that the inductive elements of Monteiro's excellent theory of unipolarity—particularly, its inferences from the evident yet not previously predicted proneness to war of the US' post-Cold War "unipolar moment"—are under-played.

Third, political science's broader concern for avoiding selection on the dependent variable—a key legacy of KKV's still profound impact on methodology—continues to reinforce IR's creative-deductive bent (King, Keohane, and Verba 1994, 108).[19] To be sure, the argument against generalizing from a biased set of cases that have been chosen because of their fit with a particular theory is compelling. But if taken too far, the mantra that selection on the dependent variable is the "work of the devil" can have a chilling effect, creating a situation whereby junior scholars begin their academic careers paranoid about the censure that may ensue from building theory by working backwards from interesting observed cases and empirical patterns (George and Bennett 2005, 12).[20] The result is that

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(1997, 913–17; 2000, 5–41; 2004, 2–6). For the charge that Waltz's critique of those who focus on testing rather than precise theory specification is unjust because his neorealism itself does not constitute a logically valid argument, see Wagner (2007, 17–33, 238).

[19]Qualitative methodology has certainly moved beyond KKV (Mahoney 2010), as will be discussed subsequently, but their rubric for rigorous testing continues to cast a long shadow over the field.

[20]Of course, the demand that scholars should avoid selection bias in testing—particularly when that bias risks the rejection of an otherwise-persuasive deductive theory following systematic mismeasurement—is an eminently reasonable one (Achen and Snidal 1989, 160–63; Downs 1989, 228) and one that this article endorses. The point here is simply that concern with avoiding such bias in *testing* all-too-easily spills over into a reticence to *begin*

professions of "purely" deductive theorization continue to abound, even in cases in which there is a substantial and visible inductive element (as in the Glaser and Monteiro cases cited above).[21]

Creative-Deduction's Shadow Lengthens: Overlaps and Parallels Elsewhere in the Field

A key legacy of Waltz's argument has been the primacy of parsimony and generalizability over case-specific detail in much IR scholarship. Adding more and more causal factors to our explanations of certain political phenomena has tended to win less favor in much of the political science community than reducing our explanations of a broader set of such phenomena to one or two pivotal variables.[22] The appeal of this latter approach is obvious: it creates the possibility of inference and extrapolation to an array of past or future cases, which in turn offers the prospect of wider explanatory and predictive power (King, Keohane, and Verba 1994, 7).[23]

Such an exchange has the potential to interact with what Martin Hollis and Steve Smith famously called the divide between "holism" and "individualism" in explanatory IR, but it should be stressed that there is no clear induction-individualism/deduction-holism connection (or vice versa), so this article should not be interpreted as simply equating these two debates.[24] However, there are stronger parallels between the deduction/induction debate and the debate between the nomothetic and idiographic approaches to scientific inquiry: the former seeks to draw generalizations about objective phenomena, while the latter seeks to understand specific and contingent events (Windelband 1998, 13; Lebow 2007, 10). The key implication of the Waltz-inspired perspective taken up by much of the IR community may be that the nomothetic approach is superior to the idiographic approach since the former offers the prospect of a parsimonious theoretical lens with which to make sense of the world while the latter can (critics might argue) be reduced to a succession of unconnected historical case studies.[25] The connections between these debates over types of IR scholarship deserve to be investigated further; but for now, they lie beyond the scope of this article.

Further parallels exist between the Waltzian focus on building theory prior to observation and the popular approach to scholarly inquiry in political science that James Mahoney and Gary Goertz (2006, 227–49) characterize as studying the

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theorization by investigating particular cases; that is, it can exert a chilling effect. And as George and Smoke (1989, 170–71) note, exploring such cases can often provide the starting point for theory development.

    [21]Note that because Waltz is a critic of narrowly empiricist positivism, his approach to theory building is actually at odds with that of KKV—and indeed, Waltz became an open critic of *Designing Social Inquiry*'s guidance after its publication (Wæver 2009, 205). However, because Waltz is often incorrectly *taken as* a positivist, the methodological influence of KKV has served to reinforce what many positivists *imagine* Waltz to say. KKV's argument is not an argument for deductive theorization, ironically, but it *is* an argument against selection on the dependent variable—and inductive inference from particularly interesting cases does open scholars to charges of such allegedly sinful selection.

    [22]The TRIP survey of the state of the IR scholarly discipline finds that 55 percent of scholars across ten countries classify themselves as adopting a "positivist" approach to research, with this figure rising to 65 percent in the United States (the single largest center for the academic study of IR) (Jordan et al. 2009, 38). As noted earlier, while Waltz is not actually a positivist—at least on any strict reading of the term—he is nonetheless often taken to be one by much of the rest of the positivist IR community, especially in the United States (Monteiro and Ruby 2009, 17; Wæver 2009, 216).

    [23]For a critique of such quests for single variable parsimony, see Glaser (2010, 6–7).

    [24]Individualism attributes causal importance to individual unit-level agents-as-variables alone, while holism sees an independent causal role for systems and structures that are more than the sum of their parts (Hollis and Smith 1990, 1–7; Hollis 1994, 15–16; Bunge 1996; Wendt 1999, 26–29). An individualist theory can be arrived at in a way that is more or less deductive, and the same can be said of holism, hence the lack of equivalence between the two exchanges.

    [25]Indeed, Waltz explicitly states that "Departing from reality is not necessarily good, but unless one can do so in a clever way, *one can only describe and not explain*" (1979, 7) [emphasis added].

"effects-of-causes;" that is, first identifying potentially causal independent variables and then measuring their impact on an array of dependent variables.[26] Mahoney and Goertz (2006, 229) identify this approach as more frequently synonymous with quantitative research, since a theoretical statement about the likely effects (say, peace or war) of certain causal conditions (say, democracy or authoritarianism) can then be tested against a large-N dataset to determine whether there is a statistically robust correlation. Yet there is nothing necessary about this link between quantitative methodology and the effects-of-causes approach to political science; indeed, the "easy" data mining via iterated regressions that has been made possible by modern computing power is antithetical to the Waltzian ideal of specifying theory in advance of "seeing" the data. Rather, it should be more broadly conceived as a particular epistemology of social scientific inquiry. Those scholars who seek to build a creative-deductive theoretical model through which to view the world, refining or rejecting their theory on the basis of its explanatory power, may all choose to adopt this approach—irrespective of whether their theory testing utilizes quantitative or qualitative methods.[27] Indeed, in their influential guide to the conduct of social scientific research, KKV (1994, 99–114) call explicitly for qualitative researchers to adopt the effects-of-causes approach in their methodological design (although they acknowledge the difficulty of including "pure" negative cases in a small-n dataset).

The "effects-of-causes" approach is posed as an antimony to the "causes-of-effects" approach that Mahoney and Goertz (2006, 299) equate with a large body of qualitative research; although note too that some of the best quantitative work is characterized by an identification of data that need to be explained (Achen 2002, 441–42). The causes-of-effects approach process-traces in exactly the opposite direction to the effects-of-causes approach, insofar as it starts with an important effect(s) that scholars consider important to understand (say, a particular major war, such as World War II) and then works back to consider its multifarious causes (multipolarity, hyper-nationalism, Hitler's character, offense-favorable military technology, territorial revanchism, and so forth). This approach yields great scholarly richness in that it offers a comprehensive, detailed, nuanced understanding of particular international events of major importance (Mahoney and Goertz 2006, 230–32). Yet as demonstrated above, it has also been eviscerated by many in the positivist political science community—often with good reason—for reducing causation to a "laundry list" of factors of possible causal importance while failing to specify the few variables of greatest salience, thus leaving the approach with little generalizable explanatory power or parsimony.[28]

How does the effects-of-causes versus causes-of-effects debate bear on the induction versus deduction debate? After all, the charge here is not that the two are directly equivalent.[29] Nonetheless, there is some visible common ground. If seeking

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[26]Of course, the effects-of-causes and causes-of-effects distinction was drawn long before Mahoney and Goertz approached the issue, by Paul Holland (1986, 945–60) in the context of statistical inference and, indeed—at least in terms of the conceptual intuition, if not in terms of the specific parlance—by John Stuart Mill (2012). Accordingly, the discussion here is not crediting Mahoney and Goertz with "discovery" of the dichotomy, but rather with the application of the distinction to the cultural polarization that has been prevalent between different methodological subfields of contemporary political science.

[27]Lebow (2007, 9) uses the label "hypothetical-deductive" (H-D) rather than creative-deductive. This article includes the "creative" label explicitly, due to Waltz's focus on the necessity for creativity in theorizing, but uses the term in essentially the same way as Lebow's H-D characterization.

[28]Achen's "ART" formulation represents one significant effort to control induction's "laundry listing" tendencies in empirical research (2002, 445–47). Nevertheless, on a conceptual level, the problems of case specificity and limited generalizability can remain.

[29]Linear additive quantitative work fitting an effects-of-causes mold can be inductive, for example, while applied formal models that seek to identify a set of causes that are jointly sufficient to produce an effect may be more deductive. That said, even here, once questions are asked over how to preserve truth when tracing additive quantitative hypotheses forward or how we get to analyzing a specific problem via an applied formal model, the neatness of the distinction again begins to break down.

to identify the causes of a particularly interesting empirical effect, working backwards to identify factors that played a causal role is likely to involve a substantial measure of induction. Conversely, if seeking to trace forwards in a truth-preserving manner to identify the various dependent results of a particular causal variable, deductive inference is likely to be a useful tool. Indeed, KKV make clear that a key research maxim for social scientific theorization in the effects-of-causes mold should be to obtain as much generalizable explanatory reach—that is, the sort of reach provided by a deductive covering law—as possible (1994, 113–114). And on the flip side, the causes-of-effects approach can of course—like scholarship utilizing inductive inference—be more readily accused of selecting on the dependent variable. Following from the discussion at the end of the previous section, therefore, this is another instance of a powerful methodological trend in the discipline serving to reinforce the influence of the creative-deductive Waltzian ideal.

The Problem for Creative-Deducers: The Endogeneity of "Interesting Questions"

Despite its undeniable parsimony, the ultimate weakness of the approach whereby theory is developed through intuition and deduction before being tested for explanatory power is that unless we make reference to what Waltz dismisses as "reality," we can have no idea of what is *actually worth theorizing*. We also have no idea where to set the boundary conditions on our theoretical targets or even how to generate the broad analytical concepts from which more precisely specified theories are ultimately drawn.[30] John Mearsheimer (2001, 6) observes that "The ultimate test of any theory is how well it explains events in the real world" and in doing so provides a neat and intuitively appealing summation of an essentially "mainstream" approach to predictive validity. More recently, Adam Humphreys (2011, 257–77) has argued that given the problems inherent in "quasi-deductive" theorizing, the true utility of IR theories lies in their role as heuristic tools that help us to understand the type of explanation and categorization needed for a particular empirical puzzle, rather than as explanatory covering laws in their own right.[31] But again, neither Mearsheimer's (broadly) positivist assessment nor Humphreys' critical perspective tells us anything about how to come up with a valuable theory that sheds light on *important* patterns or events in the first place. No matter how hard theory builders try to avoid induction from strings of observable events, and no matter how much creativity is employed (as it undoubtedly must be), the only way to derive interesting questions about the world that are in turn converted into IR theory is by observing empirical phenomena and deciding that there is a relationship at work significant enough to actually merit theorizing.[32] Just as IR scholars must resist the drive toward mere hypothesis testing and ensure that creative deduction remains at the heart of the theory-generation process (Mearsheimer and Walt 2013), so too we must continue to recognize and investigate the empirical puzzles that throw up the need to theorize.

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[30]Of course, empirical testing itself cannot proceed until concepts of explanation are in place, hence the need for a circulation of constantly improving theory and evidence (Wight 2002, 26).

[31]See also Kratochwil (2007, 35).

[32]For an early articulation of this basic intuition, see Rosenau (1966, 27–92). Note, however, that in applying this argument to the context of IR theory, the article is *not* attempting to pretend that these debates are either new or unique to political science. The two-decade exchange among Karl Popper, Wesley Salmon, Imre Lakatos, and Larry Laudan, among others has of course already explored the merits of various approaches to scientific inference and knowledge progression with far more sophistication and at a much higher level of abstraction than is feasible here (Salmon 1967; Lakatos 1970, 91–195; Laudan 1977; Popper 2002a, 2002b). It should therefore be made clear here that this article is not attempting to make "new" strides in the philosophy of science, but rather to bring an awareness of the importance of fusing induction *and* deduction back to IR theorization in the wake of the Waltzian brand of neorealism's anti-inductive aspirations.

To be sure, there are interesting questions to be asked that primarily involve the reconsideration of prior theoretical premises, rather than the analysis of new empirical puzzles: consider, as but one example, Randall Schweller's (1996, 90–121) investigation of neorealism's alleged "status-quo bias." Throughout IR, theoretical schools that begin as attempts to explain "real-world" phenomena can take on an intra-paradigmatic life of their own, with rich research programs revolving around their own internal logics (Lebow 2007, 17). Yet even these discussions are inherently rooted in empirical questions about interesting features of international politics. In Schweller's case, his (theoretical) critique of Waltz's purely structural variant of realism would not have arisen if there had not first been a broader—in Schweller's view, inadequate—realist research program that sought to explain the repeated empirical occurrence of security competition between major powers throughout history.

In short, there is an intractable endogenous feedback loop between theory and evidence: studying the effects of causes only makes sense if we decide that the effects are important enough for their causes to be worth studying.[33] To give very basic and stylized examples, would there be an abundant theoretical literature on nuclear deterrence if nuclear war happened every six months and if it was no less rare when both sides had secure second-strike nuclear arsenals? Would IR scholars be obsessed with the causes of major power warfare if major powers had only ever enjoyed harmonious, cooperative relations? Would Robert Pape's arguments about the strategic motivations behind suicide terrorism (2005) or David Edelstein's conclusions on what causes the success or failure of military occupations (2008)—to give but two prominent and theoretically rich recent works—have turned out the way they did if there were not noteworthy patterns emerging directly from the data?

Democratic peace scholarship is a particularly salient case of the circulatory induction–deduction fusion approach to theory generation: Would there be democratic peace theory, in all of its guises, if there was no striking empirical regularity (Achen 2002, 442)? With loose underpinnings in Kantian philosophy, the notion of a specifically democratic peace emerged from the historical data as "the closest thing we have to an empirical law in the study of international relations" (Levy 1989, 88). This empirical law was followed by increasingly sophisticated theorizations—note the important Waltzian distinction between laws and theories (Waltz 1979, 1–9)—attempting to explain the causation behind the visible pattern (for example, Lipson 2005; Hayes 2012, 2013). These further theorizations have added micro-level specificity (for example, the monadic-versus-dyadic causation distinction), bolstering explanatory power. In turn, such theorization has been followed by yet more sophisticated empirical testing and exploration (Hayes 2011, 767–91); not simply data mining but rigorous, theory-focused investigation. Such a circulation between theory and evidence represents the mechanism by which our understanding of international politics moves forward, in line with the core argument of this article; it is important that neither testing nor theorization alone comes to dominate a topic of study, lest the research program becomes degenerative (Hayes 2011, 768)

Even *Theory of International Politics*—the *magnum opus* of the creative-deductive approach to IR scholarship—has scarcely concealed inductive roots (Guzzini 1998, 130–35). In constructing a theory that produces a causal rationale for the relative stability of bipolarity, rejecting past work on the superior stability of multipolarity in the process (for example, Deutsch and Singer 1964), Waltz's interest in a key empirical puzzle of the time—frosty but peaceful superpower relations—shines through (Humphreys 2011, 265–67). As Alexander George and Andrew

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[33]On the duty of social scientists to study those issues which are *important* rather than merely choosing any that are methodologically tractable in the pursuit of professional output, see Clarke (2009, 31).

Bennett observe (2005, 12) in their critique of KKV, unless you investigate interesting historical cases—an intrinsically inductive (as well as creative) process—you can never move from the logic of testing to the logic of discovery; that is, the creation of new hypotheses (see also Mahoney 2010). The involvement of induction can be "thick": an empirically driven research agenda, in which case the deductive component of effective theory generation must not be forgotten. Or it can be "thin": a theoretically driven deductive research agenda, in which initial empirical puzzles simply serve to identify the relevant topic and questions of investigation. Moreover, there is clearly a spectrum between these ideal types, but it is always present to at least some extent. Indeed, narrow empiricists who claim not to be interested in theory development are nevertheless invariably engaged in the implicit inductive positing of theoretical priors: the most pernicious attitude of all, since this can obscure assumptions and causal linkages that should be open to critique.

In addition, if adopting a question-driven approach is the most desirable way to conduct substantive IR scholarship, rather than wading into intractable "great debates" about the underlying philosophy of the discipline (Monteiro and Ruby 2009, 42–44), then engaging in the induction–deduction fusion approach to IR theory is unavoidable. This is because developing theory in a bid to explain an interesting question about international politics *is* certainly creative, but it also necessarily involves induction from that same real-world puzzle.[34]

One particularly remarkable dimension of Waltz's impact on the deduction–induction debate in IR—specifically, the veneration of deduction at the expense of induction that has become commonplace in the self-identified "positivist" IR community—is that, as noted earlier, Waltz does not deny the potential for induction to make a valuable contribution to theorization. He acknowledges the circularity of the theory-evidence relationship and notes that we "need some sense of the puzzling connections of things and events before we can worry about constructing theories" (1979, 8). Indeed, his contention—contained earlier in the very same sentence—that "induction leads to a theoretical dead end" (1979, 8) is tempered by his assertion, a little later in *Theory*, that "Both induction and deduction are indispensable in the construction of theory, but using them in combination gives rise to a theory only if a creative idea emerges" (1979, 11). Years later, he reasoned similarly that "Evaluating a theory requires working back and forth between the implications of the theory and an uncertain state of affairs that we take to be the reality against which the theory is tested" (1997, 916). Such sentences actually echo the key argument of this article: he may only be calling for the "thin" version of inductive theorization identified above, but he is calling for it nonetheless.

Despite this recognition, however, Waltz still maintains that empirical data and observed patterns should not serve as the "starting point" for theorization, which means that *Theory* can still be read—or at least has been read—as cautioning against giving induction too influential a role in the embryonic stages of theorization. This holds despite the fact that his position vis-à-vis the overall merits of induction is actually more balanced than many Waltz-following positivist IR scholars have taken it to be.[35] Moreover, these followers' own veneration of deduction at the expense of induction—a trend reinforced by the methodological influence of

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[34]Moreover, despite Waltz's work being nearly 40 years old, the habit of downplaying the inductive half of the theory-building process while venerating the deductive half remains prevalent in much of the otherwise most impressive and nuanced contemporary IR theory, as the earlier discussion of Glaser's recent (2010) book—to give but one example—makes clear.

[35]For the argument that Waltz has been influential to the point of being "victorious" in the discipline (or at least the US-centric positivist discipline)—yet has also been consistently misinterpreted—on questions of meta-theory, with negative consequences for the wider theorization enterprise, see Wæver (2009, 205, 216).

KKV's approach to theory testing, as discussed earlier[36]—goes the rest of the way to explain why IR has so often downplayed the important role of induction in theorization. In short, the inductive approach that Waltz cautions against (without wholly rejecting)—roughly paralleled by the "ideographic" approach of Windelband and the "causes-of-effects" approach discussed by Mahoney and Goertz—can certainly lack parsimony and generalizability. However, it is also impossible to avoid incorporating induction into the early stages of building any new explanatory IR theory if we are to actually build interesting theories that explain important aspects of international politics. Such incorporation might take the form of assessing the causes of a calamitous international event (or non-event), probing the contextual factors behind a case (or set of cases) that past scholarship or present intuition judges to be outlying, observing an aberrant pattern in an otherwise regular series of quantitative data, or more besides; all are valuable approaches. The key point is that one does not simply sit down of an afternoon and decide to theorize without at least some prior inference about theory-worthy (ir)regularities in international politics.[37]

Accordingly, even in denying a useful role for induction in the theorization process, those deductive scholars who advocate empirical testing of their premises are in fact opening the door to induction, for past empirical tests play a key role in generating selection rules for new theoretical ideas—an intrinsically circulatory inductive–deductive feedback relationship. Investigating the unique particularities thrown up by key cases, and observed similarities and differences between such cases, can be a particularly fruitful contributor of empirical generalizations worthy of theorization—theory that can then, in turn, be used to identify further cases, as both pro-induction (George and Smoke 1989, 170–71) and pro-deduction (Achen and Snidal 1989, 145) scholars agree.[38] To return to the World War II vignette offered earlier, offense–defense theory may now wear the trappings of a deductive and testable explanation of variation in both international stability and battlefield performance.[39] Yet the theory would never have even existed unless scholars had noted with interest the marked difference in effectiveness between the early-war offensives of 1914–1916 and those of 1940–1941 and worked backwards from this empirical observation. Hollis and Smith identify this theory-evidence feedback loop (1990, 64) and the double-sided pressures they create for IR theorists, when they argue that

> Models are supposed to reproduce the causal features of the world and so the [model's] necessities must be those of the world, and not those of the model's inner logic. . . . On the other hand, there is no alternative to understanding the world through interpretations and hence through what are, in the last analysis, intellectual fictions. . .

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[36]Again, because Waltz is actually opposed to the narrow empiricism of positivism, his argument and that of KKV *ought* to be pulling in opposite directions, yet because Waltz is *taken as* a positivist by many, the methodological influence of KKV has served to reinforce what many positivists *imagine* Waltz to say (Wæver 2009, 205, 216).

[37]Consider rational deterrence theory, a powerful deductive theory that Achen and Snidal (1989) defend against inductive critics who—in focusing on cases of deterrence failure—risk measuring only a biased set of the relevant data (since the innumerable possible cases of deterrence success appear only as immeasurable non-events). Yet for this deductive theory itself to come into existence, as it did in the Cold War, an element of prior induction—from past instances of opposing military forces keeping each other in check through equality of power making conquest infeasible, coupled to data about the destructive capabilities and associated terror of nuclear weapons—was required. Furthermore, the inductive investigation of deterrence failures that some critics hoped would discredit the deductive theory, and that deterrence advocates therefore sought to defend against, has, rather, helped to refine and thus actually strengthen rationalist deterrent thought—another case of a productive circulation between both modes of inference.

[38]Note that while the cited authors disagree over whether contingent empirical generalizations can themselves form the basis for *theories* or merely *laws* (Achen and Snidal 1989, 148), they agree on this much.

[39]See Lieber (2005) for a recent critical analysis.

The first sentence of this quotation makes the case for at least an element of induction since only through empirical observation can we estimate these "necessities" of the real world. Yet the latter sentence similarly implies the necessity of going beyond induction in IR scholarship since only when we have analytically derived theoretical lenses in place can we determine what actually *matters* in the real world. As Waltz himself observes, "Theory and fact are interdependent" (2003, viii). Moreover, with this recognition in mind, Waltz's arguments for what IR theory should aspire to achieve and his critique of induction are no longer mutually compatible. If the ultimate test of an IR theory is how well it explains events in the real world—to paraphrase Mearsheimer's appealing maxim (2001, 11), and as Waltz himself acknowledges in places (1997, 913–17; 2004, 2–6)—then the argument that we should reject induction because we cannot be sure that a state of affairs inductively arrived at corresponds to something objectively real is flawed. For if IR theory is to be judged by its wide-ranging explanatory power alone, and if induction is a vital supplement to creativity in helping us to find a starting point for our deductive theory, then induction has considerable instrumental value from the outset of the theory-building process *regardless* of whether it is capable of showing us "objective reality."[40]

Squaring the Circle, or: How I Learned to Stop Worrying and Love Endogenous Theory Building

The discussion above leads to the conclusion that IR scholarship, like most social science, is characterized by an intractably circular feedback relationship. The need for both induction and deduction to play a role in IR theorizing raises the specter of a "chicken and egg" endogeneity problem not dissimilar to the Platonic proposition that we need to know everything before we can know anything (Waltz 1979, 8). Put simply, if we cannot make sense of the "real world" without an effective theoretical lens, yet we cannot construct an effective theoretical lens without some sense of what matters in the real world, how are we ever to make sense of anything (Trachtenberg 2006, 17–18)?

Despite this seeming contradiction, however, this endogenous feedback process between empirical observation and analytical theory building can represent a prospect rather than a problem for academic IR. Indeed, provided that our understanding of the subject matter keeps expanding, then we are making scholarly progress, whatever the eclectic combination of theoretical and methodological means by which we get there (Reus-Smit and Snidal 2008, 25; Sil and Katzenstein 2010, 10–13). As both Mahoney and Goertz (2006, 231) and Achen (2002, 442) note, even "real" (natural) science often takes an observed effect as the starting point for developing theory, which is in turn tested and revised on the basis of its explanatory power, for the simple reason that the observed effect in question happens to matter (be that the onset of cancer or the disintegration of a space shuttle). Indeed, the very language of "small-*n* testing" (when used as a rhetorical tool to justify a detailed focus on particular cases) and the related idea that selection on the dependent variable is a "cardinal sin" risks obscuring the reality—often missed by political scientists—that "real" (natural) science does not just progress by accounting for regularities. It also progresses by explaining specific events and outcomes (Wagner 2007, 6; Jackson 2011, 189). Only proofs of the purest logic can ever be known through deductive reasoning alone; there must always be a

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[40]Of course, if induction *is* capable of elucidating "objective reality," then there is no need to dismiss it as merely instrumental. This means that fusing induction and deduction in building IR theory is arguably compatible with both Instrumentalist *and* Scientific Realist approaches to the philosophy of science (PoS), removing the need for advocates of such a fusion to also to take a "position" in the (arguably irresolvable) PoS debate (Monteiro and Ruby 2009, 27–35). For arguments critical of various dimensions of the Monteiro and Ruby position, see Jackson (2009), Kurki (2009), and Mercado (2009).

large dose of induction in scientific theorizing if it is to have real-world applicability. That does not render science (be it social or natural) any less "scientific," however, just as theory is not rendered "un-explanatory" by invariably containing a normative aspect (Reus-Smit and Snidal 2008, 6–8; Lebow 2007, 12; Mearsheimer 2009, 241–56).[41]

Incorporating induction at the beginning of the theorization process, furthermore, does not make the ensuing deductive argument any less logically valid (Wagner 2007, 2–8). Duncan Snidal's example of coupled differential equations in his discussion of the role of empirics in formal theory is telling: no matter how formally deductive a set of equations may be, they can just as easily be describing a chemical reaction or an arms race—and therefore have no substantive meaning—until inductively derived empirical referents are added (Snidal 1986, 34–35; see also Reus-Smit and Snidal 2008, 13–16; Sil and Katzenstein 2010, 15). This is not to conflate deductive theorizing with a priori theorizing, however. Both inductive *and* deductive arguments can, conceptually speaking, be made using empirical propositions *or* symbols. The Socratic syllogism—"all men are mortal; Socrates is a man; therefore, Socrates is mortal"—is clearly a deductive argument, despite its use of empirical referents. Conversely, the argument that "most P are Q; R is P; therefore, it is highly likely that R is Q" is clearly an inductive argument, despite including no observational data. The key point, rather, is that in the practical business of addressing important problems of IR, denying the utility of induction as a means of generating a "starting point" for deductive theorization is foolish. Indeed, it is when deduction-aspiring IR scholars deny this valuable role for induction that they themselves risk descending into a priori theorization. So, in the context of the Snidal example cited above, the equation denoting both a chemical reaction and an arms race *can* be pure deduction, as long as the labels are merely fulfilling a symbolic role within the equation. The corollary, however, is that scholars do not get to theorizing about arms races in the first place without a healthy dose of prior induction to identify the objects of theorization. The moment that the labels in the equation take on substantive meaning, it is no longer reasonable to deny that induction was a valuable part of beginning the theorization process.

Deduction is thus tautological; it is a useful truth-preserving analytical tool, but it cannot—as Waltz freely admits—tell us anything new, at least in relation to the empirical world (Waltz 1979, 11; Reichertz 2010, 5).[42] And although deduction by itself can certainly deliver a logically valid theory, logical validity alone does not make a theory useful. Only circulation between induction, creativity, and empirical testing—as well as deduction—can deliver a logically valid argument that also happens to be superior in explanatory terms to other potentially equally valid logical arguments (Wagner 2007, 238). Note that none of this is to say that induction should be excused when done badly: its tendency to "laundry list" explanatory factors without establishing their relative causal importance (Achen and Snidal 1989, 155) must be countered via upper limits on numbers of posited variables (Achen 2002, 446), for example. Nor is it to excuse those who conduct inductive inference subconsciously while claiming not to be interested in theory generation. It is simply a recognition, rather, that induction is an essential component of the real-world theorization process.

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[41]Of course, proofs of the purest logic can themselves have real-world applicability; see the next footnote.

[42]Of course, the "at least in relation to the empirical world" caveat recognizes that at the level of "pure" theory, deductive inference *can* tell us certain new things. The Pythagorean theorem of right-angled triangular geometry and Kenneth Arrow's impossibility theorem of voter preferences (1950), for instance, are both cases of "new" insights with *potential* real-world applications that did not require induction in order to be deductively inferred—as, indeed, are plenty of conceptual insights in IR that arise from the reconsideration of existing theoretical puzzles, à la the earlier discussion of neorealism's "status quo bias" identified by Schweller (1996). The key point, however, is that—as discussed above, in the context of Snidal's (1986) critique of formal theory—such deductive insights require the addition of inductively derived empirical referents if they are to acquire actual real-world applicability.

The continual circulation between observed effects that we seek to understand due to their importance and the creatively generated analytical theories that explain them thus represents the means by which our grasp of international politics and the broader social world moves forward.[43] In this sense, the causes-of-effects and effects-of-causes approaches are not antinomies but complements. The "chicken and egg" problem is only a problem if IR scholars view themselves as engaged in seeking a final, definitive "truth" (Kratochwil 2007, 26–27). If, by contrast, we concede that we are simply engaged in an evolutionary process of improving our knowledge of the world via incremental refinements to our theoretical lenses—a process in which neither induction nor deduction can exist in isolation, and in which scholars' creativity must also play a pivotal role—the explanatory value of which is assessed empirically, then this continual feedback need not represent an epistemological weakness. Rather, it represents a chance for us to progressively improve our ability to explain what will always remain complex social phenomena. Such a contention also implies, encouragingly, that there need not be the gulf between "IR theory" and "foreign policy analysis" (FPA) that is often generated by disciplinary silos, since both can aspire to be real-world, puzzle-driven (FPA's recognized forte) and generate robust, potentially generalizable explanations (IR's prized preserve).

Perhaps the "abductive" inference of pragmatist Grounded Theory, by which new theories are created in the presence of surprising data by moving from one known (an unexplained result) plus a hypothetical rule to *two* unknowns (no-longer-hypothetical rule *and* case) is an approach that needs to be explored further by explanatory IR theorists (Fann 1970, 7–10; Reichertz 2010, 6–7; Heaven 2012, 5). After all, as Thomas Schmalberger and Hayward Alker (2001, 392) note, "abduction is the only mode of inference that generates a set of which the observation is an element," and it is therefore an approach that reflects how much perceptual inference is conducted in the social sciences. Such an investigation lies beyond the scope of this article, however, and it is open to contestation how far abduction actually reduces to some internalized combination of induction and deduction as advocated here. Likewise, there is an undeniable attractiveness to avoiding *all* varieties of "-uctive" label; Peter Ordeshook's "postulated preferences" terminology (1986) represents one such alternative.[44] Again, however, the issue returns to how theorists decide that the preferences being postulated are in fact good approximations of reality and how they acquire sufficient interest in the implications of those same postulated preferences that they choose to postulate them in the first place. Hence, despite the temptation to leave the induction–deduction debate aside, confronting the utility and necessity of using both—in conjunction with creativity, as Waltz argued compellingly—remains an important challenge for IR scholarship.

Such an argument is not a rejection of methodological rigor, and the induction–deduction fusion approach to theorization is of course not without limitations, particularly when used poorly. Most obviously, if the same evidence that is used as a starting point for theory generation is then used to test the ensuing theory—that could well prove methodologically inadequate—unless a compelling case can be made that new or different features of the case(s) were being used in the latter stage compared to the former. Nonetheless, this recognition does not diminish the value of the overall approach, nor overturn the argument's claims to widespread applicability.

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[43]Again, this parallels the Sil and Katzenstein (2010) and Jackson (2011) arguments in favor of analytic eclecticism.

[44]See also Eun (2012) for an approach to complementing the induction-deduction dichotomy.

Conclusion: IR Theorization 101

What does this finding mean for those grappling with how to "do" IR theory in the early stages of a new research project? Quite simply, it means that beginning with induction from a particular empirical puzzle or pattern of interest in one of the manners described above is perfectly acceptable (Achen 2002, 442), provided that the implications of a specific inductively derived finding—which can serve as the "starting point" referent function on which to focus our Waltzian creativity—are then traced forward in an analytically deductive manner to derive parsimonious and potentially generalizable causal arguments. Such tracing forward allows the recognition of further aberrant or consistent cases and regular or irregular data patterns, and the process can then be repeated. Along the way, the veneration of "pure" deduction that has been a key legacy of positivist IR's reading of Waltzian neorealism, and the methodological paranoia surrounding selection on the dependent variable must both be relinquished in cases in which it is appropriate, recognizing that such inductive selection can be the best way to identify problems, patterns, and cases that matter. The precise balance of induction, deduction, and creativity in formulating such arguments is a matter for the theorists' judgment, with reflection on the nature of that judgment process itself representing a valuable avenue of scholarly inquiry (Bull 1966, 361; Tetlock 2006), but all must be present in some portion.

Testing arguments against the empirical record determines the strength of a theory,[45] and if it is a strong theory, it in turn helps us to (re-)identify the empirical data that are of interest. Repeating this cycle allows us to refine our own theories, as well as enabling others to do it for us in a critical manner. It is an iterative, constantly conjunctive process, in which worrying about the primacy of the chicken or the egg misunderstands the nature of theory evolution (Kratochwil 2007, 32–33). Most IR scholars actually already go through this process—Waltz included, given the earlier discussion of his theoretical extrapolation from the stability of Cold War bipolarity[46]—even if the half of the act that they reify and document is the deductive part. However, there is still methodological value in making the process explicit because this creates the possibility of more reliable, systematic theorizing that is open to scholarly scrutiny.[47] There are also sound pedagogical reasons for making this process clear and replicable, since learning to move from an interesting puzzle to a testable theory—even if that is just a tweaked and applied variant of an existing theory—is one of the most difficult challenges faced by new IR scholars at the beginning of their postgraduate careers.

Two postscripts to this overall conclusion are in turn due. First, intellectual honesty improves both the character and the utility of IR theorizing. If scholars are able to acknowledge the observable events, puzzles, and past experiences that lead them to consider certain theoretical questions in the first place, then the explanatory power of the theories that emerge will be all the greater.[48] Second, IR scholars must demonstrate humility in their approach to explanation. Since our understanding of social phenomena can never be complete even when particular events are studied in all their uniqueness, the process of generalization and theoretical abstraction will inevitably lose significant causal detail. IR scholars will

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[45]Of course, scholars from different ontological and epistemological perspectives will bring different understandings of what "the empirical record" constitutes and is composed of.

[46]Of course, "bipolarity is stable" is merely a stylized fact, and not in itself evidence of inductive inference, but the broader understanding of the international system that Waltz develops contains a substantial element of induction from Cold War stability to develop a wider theory of international politics.

[47]Waltz concedes that his statement that theories should be started "creatively" rather than through induction is "unhelpful" (1979, 9). Hopefully, the approach suggested here provides at least an attempt at greater systematism.

[48]Note that, while not meant as an endorsement of the antiabstraction and antiphilosophical aspirations of the "practice turn," the approach advocated here is consistent with such a "bottom-up" approach to experiential theory development (Pouliot 2008, 258–60).

never find a "correct answer," only models that more or less accurately character-ize certain aspects of reality. Barney Glaser and Anselm Strauss (1967, 40) had it right when they argued that "the published word is not the final one, but only a pause in the never-ending process of generating theory". Theorists who can ac-knowledge their limitations are therefore to be admired, not least because such recognition of the remaining room in the debate presents opportunities for fur-ther scholarly advances.[49]

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[49]For an example of a major work of IR theory explicitly acknowledging its explanatory limitations, see Mearsheimer's account of the limits of offensive realism (2001, 10–11).

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