US Dominance and American Bias in International Relations Scholarship: A View from the Outside

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Abstract

This article responds to the *Journal of Global Security Studies* special issue on "American Perspectives and Blind Spots on World Politics," edited by Jeff Colgan. It applauds their significant achievement in offering positivist demonstrations of the bias generated by American assumptions, coding, and preferences, and quantitative demonstration of the systemic and systematic impact of this bias in skewing key assumptions and theories in mainstream US international relations (IR), by selectivizing attention and compromising accuracy. The article pushes the envelope further by arguing that the call to arms is more urgent and more significant than Colgan et al. express. As US hegemony is diluted, the discipline of IR must increasingly account for other parts of the world. Here, cultural bias generates deeper problems with both ontology and epistemology. The article reviews the wider IR field that shows how IR is at once more global and less easily generalizable, driving the imperative to expand the universe of cases for qualitative research. It warns that the problem of US bias and the wider issue of insularity is accentuated by the growing distance between IR scholarship as expressed in top journal publications and "real-world" puzzles and empirical reality—and by ongoing changes in how governments provide state support and funding for IR research and training.

Keywords: United States, hegemony, bias, international relations

Introduction

From the vantage point of a scholar located outside the American core of the international relations discipline, hegemony is manifestly evident in—and reified through—the production of knowledge. In academic bodies, theories form the sinews of knowledge structures; this special issue calls into question some of the key datasets and interpretations—about use of force, war outcomes, regime type, nuclear proliferation, and the domestic-foreign policy nexus—that underlie the received wisdom, the "common sense of things" that

is produced, reproduced, and taught in international relations. Unlike poststructuralist or other critical theory critiques from the outside, this collection stems from the heart of the international relations (IR) *corpus hegemonia*, thus presenting a particularly troubling challenge.

Jeff Colgan and his team of contributors are clear that their main purpose is to speak to the mainstream of US scholarship, specifically, the mainstream neopositivists in the field. In so doing, they perform a vital service that some scholars located outside of this mainstream have grown increasingly weary of attempting—and are perhaps less well-positioned to succeed at. First, the authors here provide various positivist demonstrations of the skewing generated by American assumptions, coding, and preferences, ranging from widely used datasets

to graduate syllabi and research topics. Their work helps to counter the feeling that "bias" is cultural, or somehow qualitative, and therefore subjective and elusive to quantification. Moreover, the bulk of these articles offer quantitative demonstration of the systemic and systematic impact of this bias in skewing key assumptions and theories in mainstream US international relations, by selectivizing attention and compromising accuracy.

Overall, this special issue reiterates what many readers of the *Journal of Global Security Studies* already know: that IR as an American social science is circumscribed by American bias and thus representative of a smaller slice of what is out there in the world than it imagines. The most trenchant reminder of this is Sarah Bush's observation that the silver lining to the US bias in IR is that it can help in the cases where we want to understand explicitly *American* beliefs and perspectives. Crucially, the general interest in such an endeavor is likely to decline in coming years as international power and authority diffuse even further, the United States' pivotal role in international relations is diluted, and "American" ceases to be synonymous with "international." Thus, the responses to this collection of articles will provide important insight into the rediscipling of IR scholarship. In the medium term, the responses from the US IR mainstream to such attempts at evidence-based arguments for rebalancing and opening up of the disciplinary core will help determine the future centrality—indeed relevance— of the US-centric IR academy.

On Dominance

I recall attempting to make these points to a white, middle-aged, male American colleague in a Salzburg pub over a decade ago, at the end of a US-Asia scholarly exchange. His response was to raise his voice and to accuse me of being "racist." To sidestep the dialogue of the deaf that resulted then, I shall start here with the reminder that dominance is a social relationship. Putting forward a view from the periphery or the "outside" necessitates the acknowledgement that our responses to structures and practices of dominance help shape the consequences and nature of that dominance. And within the IR academy, the response of the "dominated" has tended to consist of a large part complicity, with a light sprinkling of polite contestation, all laced with a strong dose of our own chosen parochialisms.

Let me offer a few illustrations from my academic experiences while based in Britain, Southeast Asia, and Australia. As a graduate student within the anglosphere of IR and trained in one of the world's oldest and largest IR departments in the United Kingdom in the 1990s, the

"common sense" was to seek case studies for which documentary records in an intelligible language were available. In my particular department, popular doctoral theses included those investigating aspects of Cold War diplomatic history and international institutions (political, financial, and regional). I was encouraged—and subsequently encouraged my students in turn—to study the US side of the story because of the superior availability of US documentary sources or "third-party" topics (regional conflicts, alliances, integration) that could be examined using documentary records from the United States, United Nations, or the United Kingdom or other relevant Western archives. It would have been unusual to dismiss as unfeasible a study of say, the Vietnam War, on the basis of lack of access to Vietnamese documents or interviews.[1] But by and large, such tendencies reinforce the primacy of the US/Western power perspective and experience. If and when translated into publications, they often reiterate the "300,000 pound blue whale" coverage of the United States in the IR literature (Hendrix and Vreede 2019, 319).

When I began to train my own doctoral students in the "British system," one of the initial questions I would ask promising applicants was, "do you intend to work in an IR department in the United States in future?" I would counsel those who answered in the firm positive to apply to US graduate schools instead, in a similar way as I would advise students eventually wishing to pursue careers in the virtual national closed shops of continental European IR. Such well-meant and pragmatic advice, of course, helps to perpetuate these closed shops. By the same token, when English-language IR departments around the world begin to favor hiring US-trained graduates with their relatively high publication statistics and their research agendas skewed toward neopositivist mainstream approaches, the barriers to entry are voluntarily dismantled in one direction, facilitating the export of the types of US bias enumerated in this special issue.

Of course, many scholars working outside of the United States have already contested the biases evident in its mainstream positivism. But they faced a similar dilemma to that of counterpositivists based in the United States and elsewhere: to be widely read and possibly engaged with, such contestation must include genuflection toward the lexicon of US political science and IR. For example, many IR scholars who study in-depth other countries or regions will have experienced having

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1   For a notable example that skillfully uses western archival material to reveal unprecedented insight into Southeast Asian perspectives of the Vietnam War, see Ang (2009).

to undertake the requisite literature review about "the state of the art," desperately trying to infer or deduce from a largely US (or Western)-emanated literature that is interested in neither their particular region nor the milieu of their cases. Nevertheless, many have jostled to fly the flag of one of the US schools of thought or theoretical tribes and their attendant research designs and methods to show that it *also best* explains their case/country/region. The dilemma? Even though the glad rags may not fit well, as for Cinderella, the glass slippers are crucial for dancing and courting: "international" socialization requires demonstration of familiarity, even allegiance, to a recognizable significant tribe determined by the dominant mainstream[2]; international recognition or "impact" requires publication at least once in one of the leading US IR journals Hendrix and Vreede analyze in their article in this collection.

Yet, this dilemma is tightly interwoven with the shifting international politics of power—in my field of the IR of East Asia, as I discuss below, we have seen this develop and change quickly over the past two decades. In the process, the space has opened up significantly for substantive contestation of mainstream US approaches and development of new bodies of knowledge and theorizing, which could change the wider IR field.

Attention, Accuracy—and Approach

I endorse all of Colgan's thoughtful recommendations in the introductory article about individual, collective, and institutional ways to mediate the American bias in IR scholarship. Particularly important for rationalists and positivists is his call for "cultural bias" (and this should refer to all forms of cultural bias, not just American) to be added to the King-Keohane-Verba triumvirate of threats to causal inference (Colgan 2019, 306–307). But I urge *all* IR scholars (not just those in the United States) to pay attention to the more maximalist end of the reform spectrum, however uncomfortable that may be. Colgan's key suggestions of what to do rely on the potential for self-awareness on the part of the biased scholars—but if Cheng and Brettle (2019, 322) are correct in their diagnosis that the American School of IR is characterized by a "hyper-westernized framework of cognitive biases and normative assumptions," this mindset trap means that the problem is not easily self-correctable. Moreover, while international collaboration may facilitate information exchange and help surface national or cultural biases, the "glass slipper" dilemma I alluded to above suggests that such cooperation may not necessarily overcome the structural incentives to genuflect toward

and adopt mainstream markers and assumptions when publishing.

Above all, I am simply unsure that greater awareness of cross-regional variation or adequate knowledge about other regions can be achieved with only "a modest investment" Colgan (2019, 306). As Kang and Lin (2019, 395) highlight, for example, the 2011 Teaching, Research, and International Policy (TRIP) survey presented the anomaly of a vast majority of US-based IR scholars acknowledging that Asia will be the most important world region, yet only a slim proportion study that region as specialists. The survey did not probe the more interesting follow-up question of how many respondents actually considered that in-depth knowledge about Asia and other parts of the world is necessary in order to undertake good IR scholarship. As Cheng and Brettle (2019, 338) note, "[i]n American IR, it is possible to become a successful IR scholar without ever leaving the United States," an "insularity" that is "acceptable, even desirable, as the perceived objectivity and impartiality of quantitative and formal research gradually gained prestige in the discipline." While there are many distinguished established scholars of the IR of East Asia currently based in the United States, how many recently US-trained students of Asian IR/security possess advanced language ability; have historical, sociological, or anthropological methods and knowledge that they can apply to their research subjects; or even conduct in-depth fieldwork? By means of illustration: in the twelfth year of my academic career, I participated in a "fieldtrip" with US-trained colleagues on a project funded by an august American foundation. I suddenly realized that our local interlocutors at each of our whirlwind three-day stops in three Southeast Asian countries were telling us radically different things compared to what similar interlocutors from the same countries had told me previously about the very same subjects when I had conducted my own field interviews as a non-US-affiliated scholar. It is not simply that such fly-in-fly-out regional engagements are problematically shallow; what are locally perceived to be "American delegations" might well be told what their regional or local interlocutors think they want to hear.[3] US-based scholars and graduate students studying

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2   For example, see the useful review of sociological explanations for citation practice in Kristensen (2018).

3   A related consideration is the motivations for seemingly pro-US sentiment in foreign countries; see, for example, Hamilton-Hart (2012) on the many mechanisms, social and institutional, that give rise to foreign policy elite pro-US "groupthink" in Southeast Asian countries. Note also two common misperceptions in IR: overestimation of one's own influence and importance (Jervis 1976) and wishful thinking (e.g., that people like us because of our "values").

other regions need other forms of "methods" training to manage these other important types of biases that skew qualitative data collection. This point is the qualitative corollary of Levin and Trager's findings in this volume that the citizens and publics of other countries do not necessarily regard foreign policy, the legitimacy of international institutions, or the utility of force in the same way as their US counterparts, thus calling into question core IR assumptions about the relationship between domestic politics and international affairs.

These considerations, together with Cheng and Brettle's final article in this issue, significantly expand the problem set: cultural bias does not only generate problems associated with selective attention and inaccuracy. Once we step toward the reality of IR having to account for other parts of the world, the problem becomes the more fundamental one of *approach*—to what is studied or judged worthy of study, and how. In other words, we run into deep-seated biases and problems that impregnate both ontology and epistemology. Staying with the example of East Asia, Rosemary Foot and I have argued that this region's lived IR experience and context is shaped by duality, hybridity, and contingency—characteristics that the dominant theoretical traditions in IR employed individually are not designed to deal with. Instead, we need "nonparadigmatic approaches that . . . ontologically privilege fluidity over stasis and that epistemologically elevate the study of process to reflect its importance in shaping real-world situations" (Foot and Goh, 2018, 3). We urge that IR studies that take East Asia seriously as a region should both look inside the state and also tackle what happens at the interstices of the regional-global and the unit-system levels of analysis. At the same time, we urge the adoption of theoretical approaches that allow for an explicit focus on socionormative factors.

The Universe of International Relations

Foot and Goh's injunction may appear to some to be unnecessarily demanding and "all over the place." Yet, it is part of a growing struggle across the international IR field to wrest recognition of the fact that international relations not only takes place beyond the water's edge, but that the most interesting aspects of IR actually occur at the "regional" and "global" realms. This has been driven by calls from scholars, based in and outside the West, for a more pluralistic discipline that takes seriously particularities inherent to different parts of the world and how these different entities interact to create dynamic variations in ideas, institutions, and practices. These calls include the growing discussions about "non-Western" IR theory and its relative paucity (Acharya and Buzan 2007,

2017), comparisons of how IR is taught and researched in different parts of the world (Tickner and Waever 2009; Tickner and Blaney 2012; Hagmann and Biersteker 2014; Colgan 2016), and efforts to bring non-US perspectives into key parts of the field that have been almost exclusively devoted to studying the United States, such as foreign policy analysis (Brummer and Hudson 2015).

One key effect of such pluralization is certainly to increase the number of cases that should be used to test and refine core IR datasets and theories such as those examined in this special issue. As a number of the authors here (especially Colgan; Braut-Hegghammer) observe, additional cases can generate disproportionately significant effects when studying small *n* issues like war or nuclear proliferation. Additional cases can also help sharpen our research focus in broader thematic fields qualitatively (see, e.g. Johnston 2012).

However, the chief imperative to expand the universe of cases for qualitative research is because a growing body of plural research is showing that IR may be at once more global *and* less easily generalizable. On the one hand, scholars working in the IR subfield of historical sociology have built a body of work substantiating the claim that the Western-centric focus in general presents a deeply inaccurate understanding of the way in which international relations/order/society evolved. This literature usefully advances other work revealing the deeply Western-centric and racist origins of modern international relations in practice and in theory (e.g., Hobson 2012; Vitalis 2015). Of particular note is Dunne and Reus-Smit's (2017, viii) edited volume demonstrating that a wider "globalization" of international society has been occurring since the fifteenth century, "profoundly influenced by encounters, engagements, and interactions between Europeans and non-European peoples, producing an international order that is culturally and politically far more complex" than suggested by the traditional account of European international society unidirectionally "expanding" across the rest of the world through imperialism and subsequent decolonization (see also Hobson 2004). In a different endeavor, Buzan and Lawson (2015) have also argued persuasively that the nineteenth-century revolutions of "modernity" that have so shaped the current international order were global at birth—*contra* the assumption that subsequent globalization was responsible for spreading that modernity out of Europe. Locating the early development of the study of IR in the nineteenth century also highlights that our field itself started out globally, aided by scholarly activism within the British empire (e.g., Bayly 2017). Alongside the related campaign for "global history" at the nexus between IR and history, Acharya and Buzan

(2017, 2019) have also refocused their non-Western IR theory campaign into one for "global IR."

On the other hand, this recognition of complex multiple global encounters between "West" and "non-West" only reinforces the need for IR scholars to know quite a lot more about other parts of the world. The global nature of international order does not render international practice or theory solidarist, or convergent. Quite the contrary: growing bodies of research reveal the diversity and richness of international society and IR practice and thought in other parts of the world, especially East/Central/South Asia and the Middle East, which are each peculiar in themselves (e.g., Buzan and Gonzalez-Palaez 2009; Buzan and Zhang 2014; Cooley 2008). In East Asia, for example, US mainstream IR security analysis tends to expect regional strategic choices to center on alliances and exclusive alignment with particular great powers. But regional specialists show that contemporary East Asian states' strategic choices are driven by three particular factors: a strong statist ideology, particular regional economic interdependencies created by regional production networks (Ba 2014; Beeson and Breslin 2014), and the tight interlinkages between external economic relationships and regime security (Chan 2012). These factors have caused them to eschew the type of outright balancing or bandwagoning behavior mainstream US IR expects to see vis-à-vis China. Instead, the vast majority of East Asian states have chosen "soft balancing," "hedging," and "omni-enmeshment"—hybrid strategies that combine apparently dichotomous elements of containment and cultivation of interdependence with China (Goh 2007/2008; Jackson 2014; Kuik 2016).

Notably, recent historical studies of some of the world's oldest international orders in East Asia and the Indian Ocean also challenge core IR assumptions or theories built out of European or American history (e.g., Phillips and Sharman 2015; Pardesi 2017). For instance, much mainstream Western IR takes for granted that all nation-states operate under the condition of sovereign equality, and Westphalian homogenization creates the expectation that the international system tends toward "balance of power" equilibria. But studies of pre-Westphalian regional social structures in East Asia strongly support the prevalence and recurrence of hierarchical inequalities and practices in the longer historical record, providing clear evidence that "concentrated power is not 'unnatural'" in international relations (Kang 2013, 200). Other works analyzing IR practices that dominate within hierarchical orders provide alternative categories of strategic behavior by privileging deference, asymmetry, tribute, and imitation (Womack 2006; Ringmar 2012). These sit in contrast to

some mainstream US IR studies that also analyze hierarchies, but are focused narrowly on the unequal authority between the United States and its allies, using rationalist approaches (Lake 2007). Overall, the literature on East Asian hierarchical orders dilutes the heavy focus on resistance (in the form of balancing and containment) in mainstream IR theory by placing equal emphasis on the dynamics of complicity (Goh 2013).

Clearly, the implication is not that current IR in these parts of the world would be simple continuations of their earlier histories; rather, these works draw attention to the complex dynamics of how local imperatives and regional historical experiences interact with subsequent Western encounters (see also Buzan and Goh, forthcoming; Halperin and Palan 2015; Acharya and Buzan 2017). These developments in the IR field reinforce earlier calls for scholars to consciously "provincialize Westphalia" (Hobson 2009). They reinforce the advice to eschew binaries because "it is not purebreds but half-breeds that reflect the contaminated cosmopolitanism of world politics" (Katzenstein 2018, 390); the argument that IR in vital parts of the contemporary world do not conform to existing IR theory expectations or predictions and beg quite different research questions to begin with (Foot and Goh 2018). In other words, the assertion that there is no need for new theories or approaches because existing ones explain the world quite well enough is simply wrong.

Of course, contextualizing this special issue's findings against the wider universe of *international* relations scholarship may moderate our perception of the extent and significance of the problem of US bias in the positivist mainstream. Various leading US IR scholars have already offered relatively optimistic birds'-eye views of the discipline as a whole, assuring us either that there are enough non-US IR scholars who can help balance out American bias (Mearsheimer 2016) or that myriad scholarly micropractices of exchange and mutual learning are already creating new IR theory (Katzenstein 2018, 383). International datasets and surveys seem to support these broad assurances. Analysis of data from the five TRIP surveys since 2004 finds "clear signs of a diverse IR discipline . . . IR scholars around the world study a broad spectrum of topics and regions, and they exhibit a diverse set of theoretical and methodological commitments in their research" (Maliniak et al 2018: 452). Drawing from a range of leading IR journals in the United States, UK, Europe, and Australia, and proceedings of major international conventions, Turton (2015a) also finds both ontological and theoretical pluralism in the discipline: the majority of IR scholars published or presented on a wide range of issues that go beyond US foreign policy concerns; the

majority of published work drew on a very wide range of theoretical perspectives, notably reflexivist, critical, or, even, no theories.[4]

And, yet, these studies also find that this diversity is tempered by structural dominance—due to the relative size and resources of the US academy, US scholars are theoretically and institutionally dominant in IR. For example, this translates into disproportionate representation of US-based authors not just in the top American journals (*International Organization*, *International Studies Quarterly*, *American Political Science Review*) but also other leading journals including the *British Journal of Political Science*, *European Journal of International Relations*, and *Foro Internacional* (see Wemheuer-Vogelaar 2018). Crucially, there is less variation in the methods used in articles published in the top IR journals dominated by US-based authors than in TRIP respondents' descriptions of their own work, and this is biased in favor of positivist approaches. Moreover, indications of US insularity in the tendency of scholars to use national outlets and material are greatly reinforced by complicity from the outside. Maliniak et al (2018, 462) find, for example, that US-based authors dominate IR syllabi around the world. Unsurprisingly, US professors are inclined to "favor their own": US respondents reported about 71 percent of the assigned readings in their introductory courses as written by US authors. No other national community of IR scholars in the 2011 survey assigned its own work at a rate approaching that of the US community. But equally significant was the extent to which non-US IR scholars *also* heavily favored US authors in Singapore (71 percent), Turkey (52 percent), Sweden (51 percent), Argentina (50 percent), and Mexico (50 percent). In other words, "the flow of ideas is largely outward from an insular United States."

The Purpose of International Relations

Two underlying trends in IR and its cognate field of security studies make the problem of US bias and the wider issue of insularity more acute than we might like to think. First—connected to the seeming decline of metatheorizing and the penchant for middle-range theorizing in the field over recent decades—is the growing distance between IR scholarship as expressed in top journal publica-

tions, for example, and "real-world" puzzles and empirical reality (e.g., Jentleson 2002; Sil and Katzenstein 2010; Mearsheimer and Walt 2013). The real-world needs are very urgent, given the complex transitions of international power and order that we are all living through and trying to understand at multiple levels. The selective breeding of IR scholars and scholarship I sketched above has helped create a twofold situation: (1) journalists and policy analysts focused exclusively on current affairs have more impact and input into crucial areas of foreign policy—and strategy-making and debates than scholars with theoretical, historical, and area-studies expertise and (2) IR scholars with such expertise and interest in real-world policy issues have moved into public policy schools, military colleges, and policy think tanks. The latter trend facilitates disciplinary emigration and exacerbates multiple biases within the IR mainstream—over time, such scholars cease to publish in key IR journals, favoring policy- or area-studies journals instead, and the use of their scholarship to train mainstream IR accordingly declines.

Second—and connected to the IR discipline's double divorce from real-world puzzles and from policy relevance—are ongoing changes in how governments provide state support and funding for IR research and training. In the United States, scholars of Russia and the post-Soviet space have been most active over the past fifteen to twenty years in raising the alarm about the post–Cold War decline in American expertise related to vital parts of the world, exactly when the United States was facing new security challenges in these areas. Most notable was the controversy over the suspension of the federal government's Title VIII funding for language training and research on Eastern Europe and Eurasia in 2013 and its subsequent general decline along with Title VI funding for international studies, language training, and international exchanges since 2011 (e.g., Yalowitz and Rojansky 2014). King (2015) is correct in warning about the long-term consequences for US power of the simultaneous attack from parochial elements of Congress on the value of a building a storehouse of knowledge about the rest of the world. This is occurring even as the state and the military begin to play a bigger role in directing the purpose of US primary research into other parts of the world, such as via the Department of Defense's Minerva Initiative to support social science research into areas of national strategic security priorities. The urge to apply a "national interest" test when deciding on government grants for academic research is also growing elsewhere. In Australia, for example, scholars must now state how their proposals will "advance the national interest" after the education minister quietly vetoed the national

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4   The latter point accords with Saideman's (2018) observation that the significantly larger quantity of IR publications in recent decades masks the fact that purely theoretical works have been consistently a niche enterprise in the broader field.

research council's award of more than A\$4 million (Australian dollars) of funding for eleven humanities projects in 2017 and 2018 (Hutchens 2018). Such pressure on scholars to demonstrate not just the relevance of their research, but their ability to enhance national interest or national security as defined or judged by the government of the day, could compromise the fundamental purpose of scholarship.

Obviously, IR, international studies, and security or strategic studies more generally are hardly "free markets"; these fields reflect their milieu and clearly bear the hallmarks of politics, state interests, and sometimes nationalism. At the national level, the study of IR everywhere is influenced by politically defined agendas that are shaped in turn by national histories, ideologies, and mindsets (Waever 1998; Turton 2015b). The influence of the state and national agendas are equally or even more marked outside of the Western core of IR. Tickner and Waever's (2009) volume taking stock of IR scholarship around the world vividly demonstrated the tendency for social sciences to develop in service of the state in Asia, the Middle East, Africa, and Latin America, partly due to state ideologies or academic dependence on state funding. This results in heightened attention to policy orientation and relatively low values placed on theorizing. Resource, linguistic, and institutional constraints continue to limit the development of IR in many cases (e.g., on Brazil, see Barasuol and da Silva 2016).

IR scholarship in China offers a significant indication of interesting challenges arising from outside the Western core of IR. Growing out of a distinctive Chinese communist understanding of theory's worth stemming from its ability practically to guide revolution (Geeraerts and Jeng 2001), the study of IR has developed rapidly over the past three decades into a "multiverse" of permeation, adaptation, and innovation of concepts and approaches (see, e.g., Qin 2018). However, as Wang Hung-jen (2013) notes, one striking element of Chinese IR scholarship remains: the strongly held conviction among many (not all) Chinese scholars, especially those working in Chinese institutions, that a key purpose of IR concepts and their application is to advance China's national image, and a key purpose of being well-versed in American IR theories and assumptions is to facilitate the transformation of relations between Chinese and non-Chinese value systems. In other words, a notable and quite explicit part of the Chinese IR enterprise is to shape China's contemporary encounter with the world. In so doing, Chinese scholars are also free from "the constraints of Western concerns for universal applications of IR theory . . . or by research agendas such as the causes of war or 'Great Debates' when considering Western IR concepts" (Wang 2013, 4).

This provides room for conceptual appropriation, theoretical innovation, but also for the political usage of IR scholarship, both voluntary and state-directed. As the Chinese government reforms and further develops the national system of funding education, research, and training, we should not be surprised to see the harnessing of IR and area studies for explicit national interest purposes either. Such moves—especially if they were observed in Russia or other nondemocratic powers as well—will no doubt create alarmist discourses in the west. Like in many other arenas, the sheer volume of China's market will exert an impact over our field, especially in simple volume-based indicators and matrices. In the natural sciences, for example, Chinese scholars' publications in key international outlets have already altered the distribution and representation in citation indices.[5]

These significant international trends challenging the purposes of IR scholarship draws our attention to the major problem that stems from the findings of this special issue: weakness, bias, and insularity at the core of the US mainstream IR hollows out our discipline's general ability to adapt to and withstand these manifold changes and challenges to scholarship emanating from both inside and outside the core.

Conclusion

Having applauded the significant achievement of Colgan et al. in this special issue, I have tried in this brief piece to push the envelope further by arguing that the call to arms is more urgent and more significant than what they express. Understandably, we may not want to scare the US-based IR horses into immobility, but the risk is that by the time these thoroughbreds are goaded into changing course, the race would have moved elsewhere quite decisively. I realize that my approach runs the opposite risk of whipping said horses into a frenzied battening down of hatches associated with the excessive defensiveness of declining hegemons with a growing inkling of the threat of times changing. But from the outside, my strong preference is to run the latter risk because, to borrow from *Discworld*'s junior witch Tiffany:

> At such times the universe gets a little closer to us. They are strange times, times of beginnings, and endings. Dangerous and powerful. And we feel it even if we don't know what it is. These times are not necessarily good, and not necessarily bad. In fact, what they are depends on what *we* are. (Pratchett 2010, 370)

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5   See "Nature Index China" (2018).

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