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Assessing the balance

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Abstract  Save for the single issue of balance of power theory’s relevance to the current system, where we and some of our critics are in real disagreement (and they are wrong), every aspect of this symposium has been highly productive. Our critics do not directly dispute the proposition that a rapid end of a single superpower world is extremely unlikely. They generally endorsed our core finding that the systemic constraints featured in IR scholarship are largely inoperative with respect to a United States that remains the sole superpower. These essays are consequently devoted mainly to discussing the implications of our findings and the future research agenda. In particular, they developed serious challenges to the idea of US led institutional revisionism, generated new ideas about both systemic and non-systemic constraints, and suggested potentially powerful theories about constraints on other states besides the United States.

Introduction

We welcome this exchange, but readers might be excused for thinking that it has been overtaken by events. Isn’t multipolarity just around the corner, as Christopher Layne seems to argue? Is not America in decline, ‘this time, for real,’ as Gideon Rachman (2011) proclaimed?

World out of balance should not—and, frankly, can not fairly—be read as claiming that history has stopped and power relations have frozen. America’s relative capabilities have declined in recent years, but, as most of the contributors to this exchange recognize, its lead in overall capabilities is so substantial that the world will remain ‘out of balance’ for some time to come. It remains imperative for international relations (IR) scholars to get American primacy right, something we showed they had failed to do. That was our main point. Most of our critics agree.

The world remains out of balance because comprehensive, aggregate capabilities—the kind of capabilities powers need to create and sustain global orders—remain concentrated in the United States to a historically unprecedented degree not anticipated by IR theory. Scholars from Morgenthau to Gilpin and Waltz (and Charles Glaser in this symposium) have long recognized that what matters when thinking about these questions is a state’s share of aggregated power capabilities: raw economic heft, technological prowess, military and naval power, innovation, organizational—institutional competence, size and location, the lot. As seductive as it is to single out one index—so much easier to measure!—it is misleading because each element interacts with others to support a state’s capacity to act on the international stage.
If you read carefully the heralds of multipolarity, you will notice a near myopic focus on aggregate gross domestic product (GDP). Yet by that measure, India would have surpassed Britain in the mid-nineteenth century—obviously not a good measure of their relative capabilities. Forecasting precisely when China’s aggregate GDP will surpass America’s is a highly uncertain game. But what we can be certain of is that matching US aggregate GDP will not make China America’s peer in overall capabilities.

As scholars who have studied power assessment and forecasts over the years, we are well aware that neither economists nor political scientists nor country experts possess crystal balls that tell them when one state’s overall capacity will cross a certain threshold. World out of balance certainly does not pretend to perform such magic. The book is about how the world works when the scales of power are out of balance. Of course, we cannot rule out the possibility that the myriad social, economic, technological, and institutional factors that underlie China’s power will all line up favourably so as to propel it and that the similarly complex set of factors underlying US power will all conspire against it. We cannot rule out the possibility that, in addition, Beijing will make all the right decisions and Washington all the wrong ones. Nor can we rule out the possibility that the other major states in Asia and globally will seek to facilitate China’s rise and hasten America’s decline. And were that concatenation to occur, yes, we might see a very consequential shift in aggregate power relations in a comparatively short time. Although many things are possible, in social science we work with probabilities; and the balance of what we know about economic growth, technological change, diplomatic relationships, institutional and political stability and adaptability, all suggest that a rapid end of a single superpower world is extremely unlikely.

The forgoing contributions actually do not directly dispute this. One reason it sometimes seems otherwise is the many meanings analysts attach to the term ‘unipolarity’, which for some even seems to be a synonym for global empire or hegemony (for example, Huntington 1999; and Mearsheimer 2001a). When unipolarity comes to be defined as the United States’ ability to defend Georgia from Russia, as Layne comes close to doing, we know the term’s usefulness is in trouble. In World out of balance we went to great pains to define and operationalize this term in ways consistent with its origins in polarity theory (Brooks and Wohlforth 2008, 11–13). It is not about influence or outcomes—for example, whether the United States can defend this or that former imperial province of Moscow—but about whether the system ‘contains one state whose share of capabilities places it in a class by itself compared to all other states’ (Brooks and Wohlforth 2008, 13). Barry Buzan (2004) most aptly describes the current system as a ‘1 + X world’, with 1 superpower and X number of great powers.1 Although we use the more conventional terminology of unipolarity, ‘our analysis does not hinge on the particular term used to describe the current system’ (Brooks and Wohlforth 2008, 12). Our book was about how a world with one superpower operates; and while China is rising, no one contributing to this symposium (with the possible exception of Layne) thinks it is or will soon be a superpower.

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1In Buzan’s judgement, ‘There is only one superpower, and there are no other plausible candidates on the horizon for that status for at least a couple of decades’ (2004, 65).
It follows that our analysis is, and will in all likelihood long remain, relevant to understanding international politics. At the outset of our book, we noted that:

The purpose of this book is to undertake a systematic evaluation of the external constraints that scholars have highlighted and thereby gain a better understanding of the United States’ global role. This entails answering four questions: Does the United States face the imminent prospect of having its power checked by a balancing coalition of other great powers? As it has become increasingly exposed to the international economy, has the United States become more vulnerable to other actors’ attempts to influence its security policies? Is the United States tightly bound by the need to maintain a good general reputation for cooperation in international institutions? Does the United States need to adhere to existing rules to sustain legitimacy and thus maintain today’s institutional order? (Brooks and Wohlforth 2008, 3)

Our answer to each of these questions was no. To a greater degree than a casual reader might note, our critics agree (the notable exception is that Layne, Glaser, Randall Schweller, and Charles Kupchan all strongly contest our analysis of balancing dynamics). Hence much of their essays are devoted to discussing the implications of our findings. Let us therefore start with the end of the book, with the foreign policy and research implications of our analysis. Once we have discussed those matters, we will turn to the core issue on which we and our critics are in real disagreement.

Foreign policy implications

Although our book focused on international relations theory, our analysis does have three notable policy implications: that the absence of tight systemic constraints matters for US foreign policy; that the United States has a greater realm of choice than most analysts recognized; and that fewer obstacles stand in the way of a US effort to reform international institutions than scholars appreciated. How do these claims fare in light of the foregoing criticisms?

The absence tight systemic constraints matters

Charles Glaser usefully asks, ‘how valuable is unipolarity anyway?’ His answer—‘not much’—captures the mood of many critics. So many events are not going America’s way, from the financial crisis to the seemingly intractable counter-insurgency war in Afghanistan. But to assess the implications of our analysis, one needs to go beyond today’s headlines. It takes a bit of mental effort to realize that, as challenging as the world may seem, it would be a whole lot more challenging if the United States had to tackle all these problems while also confronting tight systemic constraints.

When the United States seeks to translate its power capabilities into favourable foreign policy outcomes, the international system does not push back against it the way it did against leading powers in the past, and the way it still does against other powers today. As they ponder potential security policies, US decision-makers do not confront the prospect that other great powers will construct a counter-balance through alliances or internal efforts. They need not fear escalating
‘soft balancing’ measures on the part of other powers that would rein the United States in and eventually morph into conventional hard balancing. They do not need to worry that other states are in a strong position to use America’s links to the global economy strategically to force it to toe their line. They do not need to be apprehensive that failure to cooperate in a given international institution might spoil their government’s general reputation for cooperation and thus deny it all the benefits it gets from the institutional order. And they need not worry that if they break some international rule or norm America’s overall legitimacy will necessarily be strongly reduced and its leadership role will come crashing down.

In different combinations, various of these four constraints powerfully shaped the security policies of great powers in the bi- and multipolar systems of the past, and many continue to shape the policies of other powers in the unipolar one of today. Systemic IR theory, developed by hundreds of scholars over five decades, is not a naked emperor. These theories are often powerful tools for explaining the contours of state behaviour. They just do not apply to the contemporary United States. One way to escape the pervasive ‘presentism’ of debates about US foreign policy and get a sense of the importance of these constraints is to perform two mental tasks that American IR scholars often seem reluctant to undertake: think historically and think cross-nationally.

Our book provides short case studies of previous leading powers with international positions roughly comparable to the United States’ today, except that they were in multi- and bipolar international systems: Britain at its nineteenth-century peak and the United States in the latter Cold War. These cases remind readers what it is like to run a great power’s foreign policy in the face of systemic constraints like counter-balancing. The peak Cold War years found the United States devoting some 7–12 per cent of GDP to the military. Increases in US military capabilities were predictably and reliably countered—one way or another—by a Soviet superpower with the means and the motive to check America. During the latter Cold War, US policymakers considering any major undertaking in most of the world’s regions had to reckon the probability that the Soviet Union’s formidable military machine might lend its weight to the other side of the scale. Today, by contrast, the US spends 4.5 per cent of GDP on defence and increased capabilities are not counter-balanced (the combined share of the other major powers’ GDP devoted to defence has shrunk since the 1990s). Does this mean the United States can do whatever it wants? No. But when the United States contemplates an action—for example, the recent surge in Afghanistan—it does not have to consider what will happen if other major powers put their military power at the disposal of US adversaries.

The world is clearly a different place for the United States due to the permissive systemic environment. And the flip side of the lack of tight systemic constraints on the United States is their presence for other powers. The system constrains other powers from counter-balancing and contending for global leadership, for example, as most major powers have done in the past and at least two—Russia and China—would likely do now if the international system’s structure did not render it prohibitively costly. And that is not the only constraint that affects others. Because most other powers lack the material capacity to help redefine rules and provide public goods, international rules and norms constrain them far more than Washington. Moreover, the global economy constrains smaller
powers far more than the United States. While other powers are constrained by America’s outsized role in the global economy from using economic statecraft (for example, sanctions) against the United States, Washington uses them with impunity against others (albeit not always effectively).

That is what the book says. In a way, Schweller helps make this clear when he seeks to reckon US power as the ability to get others to do something they otherwise would not have done. Schweller is certainly right that the United States often lacks power of this kind: for example, it can not get the Chinese to help much regarding Iran, it has been unsuccessful in getting North Korea to relinquish its nuclear weapons, and it struggles to get the Pakistanis to combat Al Qaeda and the Taliban more effectively. Our response is that we also to need to keep in mind another way of conceptualizing power: power is also the ability to get others to not do something they otherwise would have done. Unipolarity conveys a lot of such ‘blocking power’ on the United States, but we almost never see it. Just because this kind of power cannot be seen does not mean that it is unimportant.

Unipolarity gives the United States a wider menu of choice

The claim that the United States operates within a wider realm of choice regarding foreign policy than leading powers in past systems seems fairly straightforward. Yet the contributors to this symposium react to our analysis in extremely varied ways. Schweller argues that ‘if Brooks and Wohlforth are correct, then … a unipole can choose virtually any security policy’. Charles Kupchan, by contrast, argues that we see no real choice for the United States other than to follow the status quo: ‘the United States will as a matter of course continue to deploy its preponderant power on a global basis; the unipole will automatically defend unipolarity’. Layne sees things very differently: finding our analysis wanting, he argues that the United States suffers from imperial overstretch and that this will likely force the United States to change radically from the foreign policy status quo and ‘retract some—or even all—of its overseas military commitments’. Finally, Jeffrey Legro and Glaser weigh in with a yet another response. Glaser reads our analysis as showing that ‘unipolarity is compatible with a wide spectrum of US grand strategies’. For his part, Legro stresses that our analysis does ‘not explain US thinking or choice … because [Brooks and Wohlforth] mainly offer a negative claim … Their argument lacks an explicit positive argument that can tell us what we can expect from the unipole and unipolarity … The lopsided distribution of power may open up the door to expansive hegemonic aims, but it does not push a state through it.’

Legro and Glaser got it right: World out of balance is about constraints. In our view, the nature of systemic constraints today is important because it gives the United States a wider menu of choice in its foreign policy. Contra Kupchan, it is only if the system is tightly constraining (as the conventional wisdom among scholars had suggested was the case regarding the United States) that it would push towards the conclusion that only one foreign policy—or a very narrow of range foreign policies—is appropriate. We stressed that when the system is not constraining in this manner, more room for choice exists (Brooks and Wohlforth 2008, 5–19). Contra Schweller, our claim is relative: because the United States does not face the systemic constraints identified in IR theory, it has a greater realm of
choice in its foreign policy than many analysts had assumed. Schweller does not consider a point we stressed at length in the concluding chapter of our book: there are many other potential constraints that are relevant besides the four systemic ones from IR theory that we examined.

Layne usefully brings up the issue of imperial overstretch, a point now often discussed among the punditocracy. He is certainly right that US military is stretched thin in two wars, and its government budget is awash in red ink. He is right that this will constrain Washington. But he is wrong to see that constraint as somehow being necessitated by the structural position of unipolarity. Neither the US invasion of Iraq, nor its ‘nation-building’ effort in Afghanistan, nor indeed its ballooning deficit are necessitated by its structural position as the lone superpower. These constraints emerge mainly from wars of choice, domestic entitlements, decisions regarding taxes, and the financial crisis and great recession of 2008–2010. All serious matters, to be sure, but not the kind of ‘imperial overstretch’ Paul Kennedy (1987) famously analysed in his 1987 bestseller. In all those cases, overstretch emerged from commitments to contain or pursue great power counter-balancing or hegemonic rivalry. These states had to frame policy in the shadow of a potential major war with peer rival great powers, which meant that devoting a large share of their GDP to defence and extracting more resources from their societies for security policy could be plausibly be seen as an existential necessity. Solving the problem of imperial overstretch through retrenchment was extremely hard for such powers, for decision-makers had plausible reasons to fear that cutting back on various commitments risked geopolitical catastrophe. As we argued, however, unipolarity spares the United States that kind of trade-off. Because its international position does not hinge on the specific policies that got it into its current military and fiscal constraints, it has much greater potential to extract itself from ‘overstretch’ with its position intact than did the historical leading states examined by Kennedy, Gilpin (1981) and others. Will this be easy to do? No. It is simply easier than it would be if the United States faced the kind of strategic situations confronted by the overstretched powers analysed by Gilpin and Kennedy.

One way to see this is to look at the balance of power realists’ own analysis. To their credit, Layne and many fellow realists—notably John Mearsheimer and Stephen Walt—opposed the US decision to invade Iraq in 2003. Reading their writings in the lead-up to the invasion, two salient facts emerge (see Mearsheimer and Walt 2002; 2003; Baylis and Smith 2008; and Coalition for a Realistic Foreign Policy 2002). First, balance of power theory is not part of their reasoning for counselling the United States against the invasion. They predicted that an invasion would have high costs, but counter-balancing by other great powers was not among them. And they proved to be right. The costs of invading Iraq turned out to be high because of the challenges intrinsic to nation building and counter-insurgency, not because of systemic constraints identified in balance of power realism. Second, their main argument was precisely that the invasion was not necessary for the United States’ security and global position. They urged

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2 As we noted, ‘inoperative systemic constraints mean that, much more than scholars generally believe, U.S. foreign policy is in a realm of choice, rather than necessity’ (Brooks and Wohlforth 2008, 19, emphasis added). Schweller only includes the last portion of this sentence in his article.
Washington to adopt other policies—containment and deterrence—that would serve US interests at a much lower cost. At no point did they suggest that invading Iraq was a necessary condition of maintaining the US position of global primacy.3

Bad choices can lead to bad outcomes. The United States chose major military undertakings in two tough countries even as it sharply cut taxes, expanded domestic entitlements, and made some regrettable decisions about its financial system. The consequences of these choices are serious but their origins are not systemic. After all, balanced power is no guarantee against bad choices—think of the Vietnam War, the Savings and Loan debacle of the 1980s, and the very fiscal crises that sparked Kennedy’s and Gilpin’s concerns in the first place. Having a peer competitor not only did not induce prudence in Washington, it clearly fed into a whole series of costly and dangerous interventions in the third world. Invading Iraq was a choice the Bush administration made—a poor choice, to be sure—but not a requirement of the United States’ position.

Ultimately, Glaser rightly stresses that ‘the overreach claim is more of an observation about the past than a well-supported prediction about the future … None of the basic arguments about unipolarity explain why [poor choices] are unavoidable’. America may well exhaust itself, but because it does not face tight systemic constraints the margin of error is actually much greater: it will require a longer series of bad choices to reach true overstretch than if the external environment did powerfully constrain the United States.

**Unipolarity reduces barriers to ‘systemic activism’**

The analytical core of our book is agnostic about what the United States should do. As Glaser correctly notes, our analysis of systemic constraints is ultimately compatible with the full spectrum of grand strategic options ‘ranging from neo-isolationism to primacy’. Although we do not advocate a grand strategy of primacy as it is conventionally understood in the literature, we do conclude the book by stressing a key foreign policy implication of our analysis: that the United States is in a favourable position to pursue a policy we called ‘systemic activism’—altering the institutions that govern the international system. We focused on this policy option since it was the one for which the permissive systemic environment appeared to be most relevant. In our judgement, the main operative constraints on this policy option were, in fact, systemic and thus their absence facilitated it: pushing hard to revise institutions or create new ones would not elicit counter-balancing or soft balancing, or be subject to the constraints associated with reputation, interdependence, or legitimacy. We argued that the ‘mistaken belief in the salience of systemic constraints has generally led analysts to overestimate the costs of a concerted effort to revise the international system to better advance US security interests’ (Brooks and Wohlforth 2008, 208).

Our critics generally accept our core claim about the lack of systemic constraints on a United States led effort to reform international institutions, but they are sceptical of that policy option for a host of other reasons. The criticisms attracted by this part of our book, along with our associated article in *Foreign...*

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3 The same applies to Afghanistan; see Mearsheimer (2001b) and Posen (2001/2002).
Affairs (Brooks and Wohlforth 2009), do clarify an important point: to say that scholars exaggerated the barriers to systemic activism is not to deliver a precise cost–benefit analysis of such a policy. To the degree that we implied otherwise, we are guilty of exactly what we skewed others for doing: we went beyond what our analysis could sustain. But in a larger sense, these criticisms make our point about the shortcomings of existing IR scholarship. For the reasons our critics express scepticism about systemic activism all lie outside IR research and so remain matters of speculation. To know whether their scepticism is warranted requires pursuing the new research agenda we hoped World out of balance would help propel.

**Research implications of a World out of balance**

Though a call for further research is a cliché of academic writing, that is the inescapable conclusion that emerges from this symposium. To be sure, useful, relevant research continues to emerge on key questions regarding unipolarity ranging from US domestic politics, to alliance formation, legitimacy and international rules, the global economy, and US grand strategy. Nevertheless, many of the key questions raised by our critics still can be answered only with speculation until we know more about how unipolar politics operates in two key areas: constraints on secondary states and bargaining over institutional reform.

**Systemic constraints and choices: theorizing unipolar politics as usual**

World out of balance established that the permissive systemic environment gives the United States a wider menu of choice. We thus agree entirely with Legro’s argument that ‘the character of unipolarity depends on the purpose of the unipole. What the unipole wants and is likely to do in a world it has more of a free will to shape will affect that world’. Explaining US choices in a permissive systemic environment is thus an important area for inquiry. Kupchan’s call for further research on how partisanship influences US decisions and Legro’s plea for more investigation of the significance of domestic ideas and strategic concepts are thus welcome. Yet Schweller and Voeten helpfully put on the table the argument that this is only half the equation. They contend that enhanced freedom of choice in a unipolar system is a two-way street: it applies not just to the United States, but also to secondary states, and in ways that hamstring the exercise of American power.

Voeten and Schweller both argue that other states are less constrained under unipolarity than bipolarity. Liberated from the need to toe Washington’s line in

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4 On domestic politics, see, for example, Snyder et al (2009); Kupchan and Trubowitz (2007); and Parent and Bafumi (2008). On alliances, see, for example, Kreps (2008); Walt (2009); Craig (2009); Press-Barnathan (2006); Weitmsnn (2004); and Hansen (2001). On legitimacy, see, for example, Finnemore (2009); Voeten (2005); Schweller and Pu (forthcoming); Clark (2005, forthcoming). On the global economy, see, for example, Mastanduno (2009); Bromley (2008); and Norrloff (2010). On grand strategy, see, for example, Mearsheimer (2011); Miller (2010); Layne (2009); Posen (2007); Legro and Leffler (2008); and Kreps (2011). Ikenberry et al (2009) discuss additional relevant research questions.
order to secure themselves against the Soviet Union, secondary states may be freed to pursue their interests in unipolarity in ways that may be costly to the United States. The argument appears to work best for US European allies: Voeten’s comparison between Kohl’s actions in the Euromissile crisis and Schröder’s policy regarding Iraq is telling. It seems less compelling for China, which shifted from close Soviet ally, to hostile neutrality to US ally, all during the bipolar structure of the Cold War. Indeed, the key decisions that opened China up to the global economy and began its rapid rise occurred a decade before the Soviet Union’s demise. And, as Voeten recognizes, this argument does not apply strongly to countries that did not face unambiguous threats from the Soviet Union. Today, moreover, a great many states in every region from Central America and Africa to Central Europe, the Middle East and North, East and South Asia clearly still face strong incentives to stay close to Washington. While the shift from bi- to unipolarity may have weakened the United States’ leadership capacity over some states, ultimately neither Schweller nor Voeten provide reasons for thinking this applies to all or even most states. Indeed, America’s alliance system of more than 60 countries accounts for some 80 per cent of global military spending (Economist 2011, 34).

A grand net assessment of secondary states’ constraints in the different polar structures may not be possible, but we agree with both Schweller and Voeten that better theories of unipolar politics are needed. While stressing that asymmetric capabilities are important in bargaining, we agree with Voeten that ‘advantages in relative capabilities do not automatically make others states do what you want them to do’. IR theory provides much less leverage on current bargaining dynamics than scholars used to think. In particular, despite scholars’ best efforts to update them, balance of power and balance of threat theories do not provide a valid model for explaining bargaining between the unipole and other states. It strikes us as unlikely that a similarly systemic model will be developed that does provide an overarching framework for contemporary interactions. But, as Bromley, Voeten and Schweller make clear, progress on modelling unipolar politics requires more analytical effort to unpack the system’s implications for states other than the United States than we were able to provide in World out of balance.

Unipolarity and institutional reform

Given the massive scholarly resources devoted to the study of international institutions, one would think that every possible question had been thoroughly investigated. Yet responses to our argument about the potential for systemic activism expose major gaps. As Erik Voeten (2001) observed, institutionalist scholars in the United States generally lost interest in the interaction between power and institutions, perhaps because of challenges of modelling this interaction mathematically. In addition, just as balance of power theorists never considered addressing the question of their theory’s implication for a unipolar system, a scenario in which a hegemonic state creates a ramified set of institutions and then later decides it might want to change them did not figure in institutionalist research. Our critics raise three core issues that are particularly relevant for understanding how much potential the United States actually has for pursuing a policy of systemic activism.
'Lock in' vs near-term gains of cooperation. Voeten and Layne critique our call for systemic activism on the grounds that it is impractical: they both argue forcefully that other states have no incentive to cooperate with any efforts by the United States to revise or create international institutions. Voeten maintains ‘there are no systemic reasons for states to go along with plans to cement the US power advantage into a new institutional order’. If we are right that institutions do not strongly constrain the United States, he argues, then less capable states get no benefit in exchange for agreeing to an institutional bargain that locks in their weaker positions. Layne stresses the related point that if other states expect their relative capabilities to rise, they will never agree to deals that lock in disadvantageous status today.

Both critiques focus exclusively on only one incentive for the United States pursuing institutional reform: ‘locking in’ a favourable power position. They assume that lock-in effects are strong, that policymakers believe them to be strong, and that this belief will be at the forefront as they ponder changes to the institutional order. Having scoured the literature, we have been unable to find research-based reasons to think these assumptions are true. Lock in, of course, is a well known argument, featuring prominently in the work of John Ikenberry (2001), as well as many others, and it is based in large swaths of social science theory. But we do not know how strong it is in international relations compared to other institutional effects.

Our book and associated Foreign Affairs article (Brooks and Wohlforth 2009) emphasized a different impetus for institutional reform: to tackle global problems, such as nuclear proliferation, which the current institutional structure is not well equipped to address. As Robert Keohane (1984) and others established, a powerful leading state may see a need for fostering global cooperation to manage issues that inherently require inter-state cooperation. If the United States sees such cooperation as in its interest, it can benefit from altering institutions even if lock in is not in play. Of course, Bromley is right that ‘it takes more than one for multilateralism to work’ and other states may not agree with the leader’s assessment of the need for cooperation or its strategy for dealing with the global problem in question; but if they do share its assessment that the problem must be addressed, they may resign themselves to adopting the leader’s preferred institutional approach with the knowledge that no progress can be made if the most powerful state in the system opts out.

When assessing other states’ incentives to cooperate with US-led reform efforts, two other factors also need to be considered. First, the United States has a greater share of material resources that it can use not just to coerce states to follow it, but also to offer tangible benefits to garner their cooperation. Second, due to its structural position the United States may not need to actively coerce or offer benefits to states to bring them on board; other states that are dependent upon US military protection or US market access, or both, may bend a little, or perhaps a lot, from their ideal position without the United States having to offer or threaten anything.5

The question is, how will states assess these immediate and near-term benefits and inducements in light of less certain potential future lock in effects? Lock in, as articulated in the works of Ikenberry and others, is a subtle process, working over

5 A paper documenting this dynamic is von Hlatky (2011).
long periods of time in which institutional rules are internalized and path dependency kicks in. Will decision-makers assess this as a palpable threat? And note that the issue for a country like China is not, to put it a bit crudely ‘cooperate with the United States now, when it is the sole superpower, or wait until later, when my country is the sole superpower.’ Not even Chris Layne expects the United States to be replaced by a new single superpower. Rather, bi- or multipolarity is the expectation. So the issue for potential cooperators is ‘cooperate with the United States now, when it is the sole superpower, or wait until later, when no single power will be in a leadership position and my country will have to bargain with a group of peers, including the United States.’ In sum, Voeten and Layne’s objections are preliminary, but point helpfully to a fascinating and policy relevant research agenda.

New constraints on institutional change? Jeff Legro asks an important question: why, if we are right about the greater ability to pursue institutional reform under unipolarity, has Washington not done more of it, and why have its efforts not met with more success? His thoughtful answer is to point to a series of constraints that, he infers, may be in play. Yet the puzzle may not be quite as probative as Legro thinks. After all, the Clinton years saw a lot of successful systemic activism on economic matters, which is where that administration thought the real action was, and under Bush the relevant barrier to institutional reform was clearly internal reluctance not external constraints. When it did try, moreover, even the Bush team met with some success on the Proliferation Security Initiative, terrorist financing, and other matters. In addition, Legro only focuses on formal institutions, not informal ones. Yet when one examines issues like the use of drone strikes and the safety of nuclear materials, one detects a sustained effort by the Obama administration to move norms and customary law in directions Washington views as desirable.

Legro may well be right that the United States has been intimidated by the scale of the task, but the jury is still out as to whether this is due to the system’s potential to push back. Out of the list of potential constraints Legro identifies, one of them—balancing—is irrelevant to institutional reform, several are interesting and worthy of more research but non-systemic (for example, specific interests of specific actors in a given issue area, domestic politics, timing), and one our book stressed is weakly conditional (legitimacy). That leaves transaction and sunk costs as a potentially important systemic constraint about which we know too little. Interestingly, these same mechanisms are often said to benefit the hegemon by locking in favoured practices. If they also hamstring the hegemon in later periods, this is an interesting new twist to the theory that calls for more theoretical development and empirical evaluation.6

Only more research can determine whether any of these constraints actually caused the United States to be less reformist than it otherwise would have been. After all, as in the Bush case, US officials might not share scholars’ estimates of the benefits of institutionalization. They may not believe that the current order is as out of synch with new security challenges as we asserted. They may believe—as have the leaders of many of history’s past top dogs—that the ever changing

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6See also Finnemore (2009), who makes a similar argument about the constraining effect of legitimating a unipole’s role through institutionalization.
interests of a major power are best served by the ambiguity and loopholes that abound in the current order. To the degree that these factors figure in explaining US decision-making, the prima facie case for the importance of Legro’s list of constraints is impugned. Either way, a fruitful new line of research and theorizing emerges. And regardless of what this research finds, Legro’s analysis is ultimately a useful reminder that although it may be easier for a hegemon to alter institutions in unipolarity than in other systems, it is hardly easy.

Expected power shifts and international cooperation. Constraints on US systemic activism and the trade-off other states may confront between ‘lock in’ fears and the near-term gains of cooperation intersect a third issue requiring more research: how expected power shifts affect other states’ incentives to cooperate with the unipole. Layne argues that ‘rising powers such as China need wait only a decade or so to reshape the international system themselves and construct a new order that will reflect their interests, norms, and values’.

This reflects a bifurcated assessment of the ease of altering the system: despite still having far more material power than any other state, any effort by the United States to revise the system will not work, in Layne’s view; yet if all goes well and China becomes a superpower, he expects that for Beijing systemic activism will be a piece of cake. We have already noted a key problem with this argument: it is highly unlikely that China’s leaders really expect their country to become a single superpower, as the United States now is. Rather, China experts report that Beijing’s expectations about power trends mirror the widespread belief that capabilities will become more evenly distributed over the coming decades. China’s leaders therefore can not rationally expect in any policy relevant time horizon to be in as strong a material position to revise the system as the United States presently is. Even if we set that objection aside, Layne’s argument faces a conundrum: if the United States is not powerful enough now to create or revise institutions, then why would China’s position in a decade (or two, or three, or four decades for that matter) allow it to effectively alter the system? Put another way, what is the theory and evidence to support the argument that United States is, and will continue to be, especially incapable of altering the system, while China will be adept at doing so?

Layne’s critique leads to a rich set of researchable hypotheses. One is that, barring a new system-shattering event on the scale or World War II (Legro’s ‘timing’ constraint), no state will be able to engage in major institutional revisionism. Many of the factors Legro points to for why this strategy is likely to be hard for the United States to pull off would similarly apply to China. Another is that perhaps both can successfully pursue this strategy (the United States now, and China later). A third hypothesis is that a state rising to superpower status has more leverage than an existing superpower, perhaps because the mere act of rising creates momentum that other states want to join. A plausible argument can likely be made for this ‘rising momentum’ dynamic (Schweller’s interesting observations about the ‘delegitimation phase’ are certainly very relevant on this score). On the other hand, path dependency is an obvious force pointing in the opposite direction. Developing a better theoretical sense of the ultimate answer is an important area for future research that has immense foreign policy implications; significantly, this is true no matter what assessment one has about how the distribution of power is changing.
Where our critics are just wrong: balance of power theory in a *World out of balance*

So far, so nice. We and the other contributors to this exchange are, in a sense, collaborators on getting new lines of research off the ground in order to better understand the implications of unipolarity. But this should not divert attention from the core issue where real disagreement exists. Of the four questions our book puts on the table for investigation, it was the hoary issue of balancing that attracted the strong pushback. Balance of power theory is the oldest international relations theory we considered, the one written and developed in the multi- and bipolar international systems of past centuries, the one we thought we clearly showed was not operational in a world in which the United States’ primacy is the status quo. Amazingly, even as a growing body of rigorous research confirms its inapplicability to the current system (see, for example, Nexon 2009; and Levy and Thompson 2010), some of the analysts contributing to this symposium just cannot give up on balance of power theory.

*Putting ‘soft balancing’ in its place*

Charles Kupchan insists on the significance of what others have called ‘soft balancing’. He stresses that it would have mattered in 2003, for example, if France, Germany, Russia and Turkey had not opposed the US invasion of Iraq and it would matter now if Moscow stopped pushing back against NATO expansion into its former imperial provinces. We agree that these and other multitudinous developments on the ground in Europe and Asia are important and worthy of attention: as we stressed, ‘Other major powers do undertake actions that impede US goals in foreign policy, including military security. These actions matter, and US policymakers would prefer they not occur’ (Brooks and Wohlforth 2008, 62). Yet the question for us is not whether these ‘constraining actions’ (as we called them) are important and worthy of attention, but whether they alter our core findings about IR theory and systemic constraints on the United States. The answer is no. Our key point is that they are not manifestations of dynamics identified in balance-of-power or balance-of-threat theory. They are manifestations of the fact that states have conflicting interests and seek to pursue them. Always have, always will.

These constraining actions are significant, but trying to associate them with balancing imposes important costs. Calling these policies balancing suggests that they are merely the cutting edge of major new systemic balancing, but they are not. Calling these policies balancing suggests that the ‘real’ issue in play is underlying US power, but it is not. Calling these policies balancing suggests that they will go away as US power declines, but they will not. Calling these policies balancing suggests that the United States can somehow ‘control’ how many of these constraining actions occur over the years by acting, or not acting, in a certain way, but it can not. Our analysis shows that the number and severity of these constraining actions will not rise and fall over the years in direct response to certain US policies (for example, unilateralism) but instead will ultimately reflect a constellation of other factors that are particular to these other societies—the nature of their domestic policies, their commercial interests, and so on—that the United States cannot control.

Dressing up diplomatic bargaining in the garb of systemic balancing exacts an even larger price: the bowdlerization of IR theory. An IR theory worthy of the
name cannot become an elaborate description of unfolding events. To have any analytical use, concepts need to be defined in ways that exclude some phenomena—even important phenomena. Yet some of our critics seem to want to strip IR theory, and especially balance of power theory, of exactly the attributes that make it a theory and not lore or ideology or prophesy. If soft balancing proponents have their way, whenever two governments disagree and seek to push their favoured policy against the wishes of another, it is balancing. The attempt to make balance of power theory relevant to the current era threatens to make it meaningless, rendering all politics balance-of-power politics.

**Faith-based balance of power theory**

Christopher Layne’s response graphically illustrates the cost of this practice. ‘There is increasing evidence that [Brooks and Wohlforth] were wrong about the permanence of US primacy’, he writes, and that, ‘if balance-of-power realists were off somewhat on the timing, their key insight was correct: the over-concentration of power in US hands after the Cold War would spur the emergence of a multipolar international system in which American hegemony would be counter-balanced.’ The evidence Layne cites as validating balance of power realism is ‘relative growth rates and shares of world GDP’. In other words, if the US share of global GDP shrinks for any reason, the balance of power realists are right. But if the US share stays constant or grows, as in the 1990s, the balance of power realists are still right, just a bit off on timing. If we see actual counter-balancing policies of the kind that the theory has traditionally identified—namely, a costly augmentation of military power by a single state, or a strongly combined military build-up by a group of states, to balance the military power of the leading state—as happened in the Cold War, balance of power realists are right. If China had ramped up its defence spending to, say, 15 per cent of GDP—even at the cost of slowing down long term growth and therefore slowing or reversing the decline in the US share of global GDP—we can be pretty sure that Layne would have claimed that as evidence that the balance of power realists were right. Yet the fact that China keeps defence spending low as a percentage of its GDP (it has never exceeded 2.5 per cent since 1992 and has been almost exactly 2 per cent every year for the past decade) is also evidence that balance of power realists are right. In turn, if China and Russia (or China, Russia and India, or France, Germany, and

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7 At points (for example, note 5) Layne seems to say that the world is already multipolar, but most of the discussion is about trends toward multipolarity. Either way, he claims the trends are enough to validate his theory.

8 When specifying what specific behaviours balance of power theory would predict, Kenneth Waltz (1979, 125) identifies two things: (1) ‘states allying … even though they have strong reasons not to cooperate with one another [like] the alliance of France and Russia, made formal in 1894’ and (2) ‘making internal efforts to strengthen themselves, however distasteful or difficult such efforts might be [as with] the United States and the Soviet Union following World War II … the United States by rearming despite having demonstrated a strong wish not to by dismantling the most powerful military machine the world had ever known; the Soviet Union by maintaining about three million men under arms while striving to acquire a costly new military technology despite the terrible destruction she had suffered in war.’

9 Source: SIPRI Military Expenditure Database.
Russia, or any other combination of potential balancers) had formed an alliance to counter US power, the balance of power realists surely would have claimed vindication. If, despite on-the-record predictions that such a balancing effort would occur, no such alliance emerged and the chief alliance formation since the Soviet Union’s collapse has been NATO’s expansion, the balance of power realists still claim vindication.

This is taking a venerable IR theory down to the scientific status of Brezhnev-era Marxism–Leninism. Any shift of GDP balances of any kind that reduces the US share validates the theory. Any other kind of shift is a timing problem: the revolution will come, even if the proletariat is getting fat. Counter-balancing policies validate the theory, as does their absence. According to Layne, China’s economic growth—as well as Russia’s, India’s, Brazil’s and Europe’s—is a response to unipolarity. Layne wants us to believe that if the United States were split in two, these countries would suddenly decide to stop growing so fast. Moreover, Layne thinks our analysis is falsified if unipolarity turns out not to be permanent. In other words, if history has stopped, we are right; if not, balance of power realists are right.

We, by contrast, developed and tested empirically a falsifiable proposition: that balance of power theory does not apply to a unipolar system. Note that we do not claim to falsify the theory. We recognize that balance of power theory does not purport to explain states’ rates of economic growth and decline, or major events internal to states. The Soviet Union collapsed for reasons largely exogenous to balance of power theory, and so the mere emergence of unipolarity cannot be said to falsify it. Similarly, China’s rapid growth and the slower growth rates of the United States and other wealthy states are not explained by balance of power theory and their occurrence does not impugn our analysis.

Unipolarity’s durability does not bear on our proposition about the current irrelevance of balance of power theory, but rather affects the expected shelf life of our analysis. Twelve pages of the book (Brooks and Wohlforth 2008, 27–35, 40–42) document the fact of US material primacy. Layne calls this ‘a static analysis’. Actually, it is a snapshot of the present with dynamic implications: all else equal, the bigger the gap in comprehensive power, the longer it takes for others to catch up. Our snapshot showed that the world was out of balance and systemic theories developed for balanced eras needed re-examination. If we expected mutipolarity to reappear after a brief unipolar ‘moment’, as Layne, Waltz and many others did in the 1990s, then the shelf life of our analysis would be short. As noted above, although we can not know what the future holds, a rapid end of a single superpower world does appear extremely unlikely. Exogenous shocks that either lengthen or shorten the system’s lifespan do not affect our argument about balance of power theory’s inapplicability to a unipolar system. If an asteroid impacted China tomorrow and stalled the country’s growth for 20 years, this would not make balance of power any weaker than it already is, nor would it make our argument any stronger. If the asteroid hit the centre of the United States and split the country in two, balance power theory would not be vindicated, and our argument would not be impugned.

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10 As Layne phrases it, his prediction is that ‘unipolarity would stimulate the emergence of new great powers that would act as counterweights to American hegemony’.
Our analysis is subject to empirical verification and falsification on two counts. First, it leads to the expectation that so long as power remains heavily concentrated in the hands of the United States, there will be no counter-balancing. But we did not stop there; we also undertook a second testing strategy: we delineated the observable implications we would expect to find in the historical record if our argument were valid as well as those we would expect to find if balance of power realism had any purchase in the current era. Readers can evaluate for themselves the evidence we present for why our argument is valid. Given that Layne does not engage this evidence, nor specify any causal mechanisms linking changes in global economic balances to his theory, it is hard to know what to make of his objections other than to lament their distance from evidence-based social science. In this regard, Campbell Craig (2010) tellingly notes:

> The absence of traditional military balancing against the US since the end of the Cold War, a fact of international life that almost no one now denies, poses a major problem for balance-of-power Realists, who argue that major powers are destined to build up their own military forces, and/or create formal military alliances, in order to balance against a dominant state. Prominent structural realists have predicted such balancing behaviour since the early 1990s, but it hasn’t happened yet … [B]alance-of power Realists must show why major powers have not shown any indication of balancing so far over two decades, indisputably a long time in the context of modern international history … Otherwise, [they] are forced simply to assert that a new polar system will emerge, someday, simply because that is how international politics operates. This, as any student of social science knows, is an unfalsifiable argument. Nations simply are not trying to match the US in military capabilities or to form formal military alliances as they constantly did with respect to far less preponderant states over the past 300 years. Conventional Realists need to account for this: they need to show how balance-of-power theory can explain this absence. Responses that amount simply to a plea to ‘just wait’, or to describe behaviour as ‘balancing’ that never would have been called that before, do not fulfill this demand.

**Beyond possibilistic security centrism**

In contrast to Layne, Glaser and Schweller see the same world that we see: one in which counter-balancing is absent. They respond to this by insisting that counter-balancing is possible. We agree, but took pains to emphasize that it is highly improbable (Brooks and Wohlforth 2008, 46–48). Their responses make our point. Glaser’s argument that counter-balancing could happen relies upon the highly improbable assumption that it is reasonable to treat two regions—the European Union and ‘Northeast Asia’—as if they can and will act like countries. For his part, Schweller sees an anti-US Russia–EU coalition as possible; we see it as highly improbable. Furthermore, both Glaser and Schweller note (as does Legro) that although the power differential in favour of the United States makes it very hard for the other major powers to counter-balance US capabilities, they might be sufficiently motivated to bear the extremely high costs of doing so if the United States threatened them by going on a campaign of global aggression and conquest. We agree. But what is the probability of this happening? As we noted (Brooks and Wohlforth 2008, 16), ‘plausible arguments for the utility of widespread conquest in an age of nuclear weapons and low benefits of holding territory are hard to
imagine.’ To say that the balancing constraint would kick in if the United States goes on Nazi-style rampage of global territorial conquest gives a sense of the theory’s contemporary relevance.

Glaser also briefly contests our explanation for why counter-balancing is not occurring. In this instance, Glaser’s ability to address our argument is hamstrung by the fact that he clings to a very particular view of state preferences. Specifically, his particular counter-argument only works if we assume that protecting the ‘core security’ of states—that is, ensuring their prospects for survival as sovereign units—is the sole motivation for balancing behaviour. Yet there is no reason to think that this is the case; status and ‘secondary security’ issues also matter in this regard. The latter part of Chapter 2 (Brooks and Wohlforth 2008, 48–58) provides compact case studies demonstrating how these other preferences drove earlier states toward counter-balancing and hegemonic rivalry with the leading powers of multi- and bipolar systems. On both theoretical and empirical grounds, therefore, we see little reason to accept Glaser’s laser-like focus on core security as the Ur-preference of all states. And the moment one relaxes that assumption, it becomes clear, as Kupchan notes, that ‘other states are not balancing against the United States because they do not have the material resources to do so; the power differential is simply too large’.

Conclusion

We are indebted to CRIA’s editors for organizing this symposium and to our distinguished colleagues for contributing their considerable intellectual firepower to the enterprise. Contributors generally endorsed our core finding that the systemic constraints featured in international relations scholarship are largely inoperative with respect to a United States that remains the sole superpower. They developed serious challenges to the idea of US led institutional revisionism that we failed to address and that lead directly to a promising research agenda. They generated new ideas about both systemic and non-systemic constraints that may well apply today. And they suggested potentially powerful theories about constraints on other states besides the United States. Save for the single issue of balance of power theory’s relevance to the current international system, where we and some of our critics are in real disagreement (and they are wrong), every aspect of this exchange has been highly productive.

The contributors to this symposium are absolutely right that the world is changing. Capabilities are diffusing, the 2008 financial crisis and its legacy have harmed the United States’ relative position, and China’s spectacular rise continues to be felt globally and especially regionally. But there is a big difference between the exciting process of becoming a newly capable actor on the world stage and actually assuming a position of leadership. The premise for our book—that comprehensive, aggregate material capabilities are concentrated in the hands of a single state to a degree not encompassed in systemic international relations theory—continues to hold and, in all likelihood, will continue to do so for a long time.

As long as this is the case, our central theoretical conclusions will still obtain and the implications of this exchange for further research will remain centrally
important. When linked with other important areas of inquiry we have discussed in this article, these new lines of research promise compelling and relevant explanations of political outcomes in a world out of balance. While Simon Bromley is right that the question of the ‘political utility of American primacy … remains to be answered’, this exchange has at least brought the answer a bit closer.

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