Balancing in the Balance

An INTERNATIONAL STUDIES QUARTERLY ONLINE symposium

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INTRODUCTION

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The balance of power has been a concern long before the field of academic international studies ever existed. Rulers, historians, politicians, and philosophers have all been concerned with the notion: what does it mean to have a balance of power? Are such balances stable? Can they only exist between independent polities, or within polities as well? Are balances the product of deliberate policies or institutional design, or are they the unintended consequences of other actions? Is a balance even desirable? Centuries of pondering such questions preceded the inauguration of international studies as a distinct academic realm in the early 20th century, making this a central concern not only for those in the modern academy, but for much of the entire rich history of reflections on politics.

After so much ink has been spilled one might think that there is nothing more to say — nothing new, at any rate. This would be a mistake. Jørgen Møller's recent ISQ article (2014), which reflects what William Wohlforth calls a "sea change in scholarship" about balances of power, makes the dramatic claim that we should be concerning ourselves not with the recurrence of balances, but with the very *existence* of balances in the first place. Widening his focus beyond the European great power system that served as the raw material from which earlier generalizations about tendencies to balance were derived, Møller suggests that this European outcome was actually something of an anomaly. Balances of power between sovereign states in Europe, he argues, came about in Europe because of a measure of independence of social groups within those polities from the organs of institutional power and authority — an internal balance, so to speak, between the state and social groups.

The contributors to this Symposium advance several trenchant criticisms of Møller's argument, both "internal" critiques that accept the basic logic of the argument but raise questions about particular cases or assumptions, and "external" critiques that take issue with the way that Møller treats "balancing" in the first place. Participants include William Wohlforth, Deborah Boucoyannis, Stuart Kaufman, Benjamin de Carvalho, and Victoria Hui. Møller replies at the end.

Comment on Møller

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ISQ readers who have not been following research on balance of power theory please take note. Jorgen Møller's fine article (2014) reflects a sea change in scholarship on what my coauthors and I once called "a foundational question of the academic study of international relations: whether and under what conditions the competitive behavior of states leads to some sort of equilibrium" (Wohlforth et al. 2007, 156). For about two decades after 1980 most scholars assumed that Kenneth Waltz's (1979) neorealist theory had answered that question definitively. As we summarized it (Wohlforth et al. 2007, 157), the theory posited that "because units in anarchic systems have an interest in maximizing their long-term odds on survival (security), they will check dangerous concentrations of power (hegemony) by building up their own capabilities (internal balancing), aggregating their capabilities with those of other units in alliances (external balancing), and/or adopting the successful power-generating practices of the prospective hegemon (emulation)."

Like many great theories, Waltz's ultimately generated progress in knowledge by spurring research that proved it wrong. It started with studies focused on Europe (Vasquez & Elman 2002), which seemed problematic but were temporally too limited to impugn so general a theory. For a grand but general proposition about subtle systemic tendencies over massive spans of time, a different research set up was needed, one that could encompass multiple inter-state systems. Thanks to the work of a small but dedicated band of scholars, most of whom Møller cites in his article, we now know that sovereign territorial political units interacting in anarchy do not generate any systematic tendency toward balance (or, more accurately, against "hegemony"—the domination over or subordination of other units by one especially capable actor). Twenty years ago, scholars thought that balancing was something approaching a law of inter-state politics and any deviation from that law was an anomalous puzzle. They saw the European states system either as the only relevant case or as a case exemplifying a universal tendency. Now, in light of further theoretical development and the careful empirical study of Europe and other inter-state systems, it is clear, as Møller puts it, that "the puzzle is not why balancing outcomes sometimes *fail* to occur but rather why they occur at all."

Møller proposes an intriguing two-level explanation for why balancing occurred in Europe but not in the theoretically probative comparison case of Warring States China. The argument is that the presence of independent "organized societal groups" in European states (or other geopolitical units) but not in Chinese states accounts for this divergence. Given the space confines of an article, Møller does a commendable job of evaluating his conjecture. The evidence seems especially compelling concerning the causes of the Hapsburgs' failed bid for hegemony. Making his case also requires that he revisit Hui's (2005) comparison between these same systems and especially her assessment of state-society relations in Warring States China.

I am in no position to assess the differences in the two scholars' accounts.

But Møller does raise the question of generalizability. If this explanation holds for these two major systems, what might it portend for others? Are systems comprised of autocratic states lacking multiple organized societal groups prone to hegemony (because such states are good at generating the power to conquer but, once conquered, are easy to rule), while systems comprised of states with such groups are prone to balance (mainly because the groups resist the ruler's power-generating efforts)? Much needs to be clarified before scholars can make headway on that.

First, the theory appears to assume that organized social groups must lose from conquest. If the median group stands to gain as the state of which it is a part expands, it is not clear why it would act to frustrate such expansion. And it is unclear at the outset why we should assume that the returns to conquest never redound to groups' benefit.

Second, the theory needs to be developed to yield predictions for mixed systems. For we know that systems comprising both states with and without politically relevant social groups exist. Møller refers to research suggesting that democracies are systematically better at war than autocracies (e.g., Reiter and Stam, 2002). The very existence of this whole research project is the result of the fact that modern international systems are always mixed, containing some states with multiple independent societal groups and others without such groups. Yet the article considers only two kinds of systems: those populated entirely by constrained or unconstrained states. What does Møller's theory say about mixed systems, the norm for at least the last 200 years? As written, it seems to predict that in any mixed system unconstrained units should have the advantage over units constrained by societal groups. And this seems to be belied by the 20th century experience when totalitarian states that were by all accounts as unconstrained as states can get were nonetheless reliably balanced by states that by all accounts fell at the highly constrained end of the spectrum. Indeed, the article contains no argument (other than path dependence) for why states can't eliminate the political power of social groups within them if the circumstances seem to permit it and call for it. Given several cases of rulers doing just that (read: Stalin), this leaves a serious gap.

But I am probably being too conservative to limit this comment to comparatively recent times. States with multiple independent societal groups have existed for a lot longer than that. Casting my eye across world history, problems for the theory seem to emerge all too readily. The Roman Republic rolled the table against a posse of absolutist territorial monarchies. Not only is the brute outcome problematic from the perspective of the argument Møller forwards, but one of the most popular explanations for its success, first advanced by Polybius, focuses precisely on the existence of privileged estate groups that gave Rome the edge (Eckstein, 2007; Deudney, 2007). And that case seems to show a setting in which conquest paid for these groups in terms of lucre and social status. More generally, though balancing is not the dominant tendency in world history, it does occur outside Europe—and seems to occur in systems comprising units as lacking in privileged estate groups as Warring States China. Perhaps blinded by my association with the coauthored project reported in *The Balance of Power in World History* (Kaufman, Little and Wohlforth, 2007), I can't help but reach for the explanations we found persuasive in such cases, notably the existence of "marcher" states brought into the previously autonomous system.

Needless to say, these are quibbles about an argument Møller only hinted at in this article. The precise claim he did make is bold enough, seeking to account for major macro-political outcomes with huge import for major theories of international politics. That he was able to accomplish that while also suggesting fascinating avenues for further research is auspicious indeed.

STATE CAPACITY AND COLLECTIVE ORGANIZATION: THE SINEWS OF EUROPEAN BALANCES

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Much can be praised in this article, especially its "bottom-up," institutionalist approach: top-down approaches have constrained our understanding of how balances or hegemonies form. A major point Møller makes is that something peculiar to Europe made balances possible; accordingly, one cannot assume that a similar "balance" can be expected to hold when the system units have different structural features.

There is more to commend (e.g. latest historiography on China), but space is limited, so I offer two critical points. First, that European outcomes (may) appear as balanced does not mean that the mechanism(s) that generated them depended on balances themselves. The account of European balances thus mistakes a consequent for a cause. Second, the emphasis on Continental balancing ignores the case of Britain, which, despite the most robust system of domestic checks and balances, succeeded in controlling a quarter of the globe—suggesting that constraints per se (not themselves exclusive to Europe after all in today's world) were not the key to "European" outcomes. Internal constraints are thus neither necessary nor sufficient for balanced outcomes.

Internal balancing?

Møller argues persuasively that geopolitics cannot be explained without considering state-society relations, especially the "constraints on the executive and multiple centres of political power:" a balance emerged systemically because of internal state balances. Isomorphism thus exists between the domestic system of a balance of power between groups, which typifies liberal constitutionalism, and an international balance of power between competing units. I am sympathetic to this approach (I argued similarly in Perspectives in 2007).

The problem with this *application* of the point, however, is not that it is descriptively wrong; indeed, the author supports it with rich examples. Rather, the problem lies in assuming that what distinguishes Europe is having early on the same attributes it has today: a balance of social forces, non-domination. Europe is balanced today or in the Early Modern period because it was balanced already in the past.

An article can only do so much (p.24), but the question remains why Europe, as opposed to China, was so rich in assertive communities that generated balances. Why were such domestic checks absent outside the West—with the exception of some (brief) phases in BCE China? Is it that the language of rights, etc. is exclusive to Europe, part of Europe's natural fauna, contrary to a non-West that "succumbs" to authority, empires and domination? And why does this question matter for IR?

Let me answer the former question first, by drawing on research on two other non-Western cases, typical of absolutist regimes that "suppressed" domestic resistance: medieval Russia and the early Ottoman Empire. Examining the remarkably rich records, especially from courts, shows that the demands for rights, privileges, tax exemptions, and justice are as prevalent in these non-Western cases as they were in the West. Similar historical parallels can probably be adduced for Chinese history too.

In fact, on many counts, these cases exhibit demands for rights not encountered in the West. In 1598 and 1613, whilst English monarchs were adopting the worst practices of Continental absolutism, the Russian Assemblies of the Land, which had representatives from *all* social orders, were *electing* rulers, Boris Godunov and Mikhail Fedorovich. Moreover, Russian legal codes protecting rights were as detailed as any of the West. The Russian tax system was riddled with privileges and exemptions, the perennially ominous sign of a weak state—what we know of it stems mostly from exemption charters. Similarly, Ottoman land law, considered the epitome of weak/absent property rights, is on some dimensions even more "liberal" than the English land law, as I show—a comparison hitherto missed even among specialists.

Why does all this matter?

Because it shows that the common focus—which this article shares—on the descriptively accurate but causally endogenous narratives about rights etc., especially in exchange for taxation, misses what truly distinguished Europe *and which is relevant to the modern period*, where the domestic structure of states is already converging to a considerably degree. The answer can be inferred from Møller's points: it is state capacity. On p.12 he states that "lineages" generate the Chinese nobility and "judicial privileges" the European one. But all lineages require is families procreating and preserving their household lore. Judicial privileges, on the other hand, require a state that both grants them and protects them over time. The same applies to all the other factors he highlights.

The key in the West was more effectively organized state authority and capacity, at the national or the local level, where resistance was organized. European communities, "estates," assemblies, towns etc. didn't organize spontaneously nor emerge naturally, like sturdy trees in the Black Forest. They were creations of state authority (with strong Church input), more effective in some periods, but declining over time in Continental cases—a decline Møller recounts. The difference between a fully constitutional regime (England) and a less-than-constitutional one (France) is that England applied uniform laws throughout its population, whereas France faced strong groups holding liberties that could not be revoked.

War pressures cannot account for this state capacity; as Møller states, they are underdetermining. But it is clear in his one English example: the brief baronial rule in the 1250s and 60s, when war pressures were minimal. As I explain elsewhere (Boucoyannis 2015), that moment would never have the balanced (constitutional) outcomes we think natural if English rulers had not already developed a precocious state capacity, allowing them to meet the French "man for man" and "pound for pound" (Strayer, 2005: 52), even though they had a fourth of French population and territory.

Internal balance to external hegemony: Britain.

This brings me to my second point: Britain, and more specifically England. England is as missing from most causal arguments as it was central to the calculations of the European states trying to overcome the ineffectiveness Møller so nicely describes. If European balances happened because rights kept governments constrained, how do we explain why the paragon of constitutionalism, England, ended up controlling a quarter of the globe (so much for the stopping power of water!)?

This is why the point briefly conceded on p.27, that liberal and supposedly "limited" regimes consistently extracted far more than their "absolutist," "unconstrained" competitors is dispositive (see the great work by Mark Dincecco, 2013; I also show England's advantage dates to the Middle Ages). Are the European cases Møller describes examples of the 'checks and balances' model; or are they cases of ineffective, piecemeal organization of latently powerful states?

I would suggest that the balances discussed in the article are not tied to "limits" per se; they are, rather, a group of similar countries that found themselves at a similar level of backwardness—and those that were further behind or weaker got submerged, while those that were temporarily stronger, such as Napoleonic France or Germany in the twentieth century, went on a brief rampage.

Ultimately, European balances (if the term survives operationalization) can be variously explained. One scholar's "self-weakening reforms" is another's "checks and balances." A prior question is what made Europe different: comparative study suggests that, contrary to narratives about greater rights and resistance to authority, it is the peculiar but varying capacity of Western polities to impose collective organization on social groups, thus solving their collective action problem. After all, if central authority were not that strong, why would so many groups be organized against it? Why would we have the Magna Carta, if King John were not *already* extracting from his subjects three to four times more per capita than his Continental counterparts? This led to states *capable* of balancing.

If the "foundational question" is under what conditions state competition leads to equilibrium, the answer must lie in how some states attain similar control over the population so as to be competitors. There is after all a "balance" in Africa and Latin America—no local hegemon, but also no war. But no one is writing about *that* balance of power, except, like Herbst (2000) and Centeno (2003), to explain just why those states are so underdeveloped, questioning, in the process, whether war can generate much itself.

As I argue elsewhere (Boucoyannis, 2007), a balance always results from specific institutional practices, it is made, not spontaneous; even if, as Waltz emphasizes, individual states may have different goals. Waltz's flaw (which he freely retracted in private) was the assumption of spontaneous outcomes. In the market economy, not everyone has to have the *goal* of increasing national prosperity—but if no one does, if no regulation is in place, outcomes will not "spontaneously balance," as we have recently seen; similarly with international politics. We should not confuse the goal of balances with its cause. Sturdy concentrations of power have always lain behind the benign image of balance and rights that the West has claimed as its trademark.

Hegemony and Balance: It's Not Just Initial Conditions

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Jorgen Møller's welcome contribution to the debate on the sources of international balance and hegemony forwards a powerful hypothesis, well supported with evidence from the two cases he considers. While conceding the importance of international influences on international outcomes, Møller's central argument is that one single domestic factor—"the character of state-society relations at the [start] time of intensified geopolitical competition"—provides the overriding explanation for whether international systems tend toward balance or hegemony. I would like to suggest two responses to this argument. First, while initial state-society relations are important, no particular configuration is necessary to push a system from balance to hegemony. Second, and more broadly, Møller errs by emphasizing one particular factor to the near-exclusion of everything else that matters. While Møller has identified an important dynamic, especially in early modern Europe, an adequate explanation of systemic outcomes (hegemony or balance) must also consider other major causal factors.

Møller is persuasive that in early modern Europe, domestic constraints on rulers' power—the existence of powerful classes of nobles, merchants and clerics, and of constraining institutions such as Diets—were an important cause of the Habsburg failure to achieve hegemonic power. This is an important corrective to Hui's (2005) argument about those kings' policies of "self-weakening expedients": existing domestic arrangements made it difficult for them to attempt centralizing "self-strengthening reforms" of the kind that led eventually to Qin domination in ancient China. Both are right to suggest that other cases of successful hegemony in continental systems were also achieved by states with the kind of pre-modern totalitarian political system that Shang Yang built in 4th-century BCE Qin. Other examples of this phenomenon include the Assyria of Tiglath-Pileser III and his Sargonid successors; Magadha, the original kernel of the Mauryan Empire in India; the Incas in Peru; and Shaka's Zulu empire in southern Africa (see Kaufman et al., 2007).

The trouble is that such centralization of power is not a necessary condition for systemic hegemony. The most prominent counterexample is Rome. The Rome that rose to hegemony was a republic that included just the sorts of "multiple privileged groups" that Møller claims should prevent hegemonic rise. It had a wealthy and powerful merchant class, the *equites,* who were pivotally important in elections for Roman magistrates, including at the highest level. It also had a competing, even more powerful landowning nobility that controlled the Senate—but that was further checked by a class of smallholding farmers who formed the backbone of the army, had the power to pass laws in the Plebeian Assembly, and were represented by tribunes with veto power. Finally, Rome's rise depended heavily on a group of allied states in the rest of Italy that had near-total political autonomy from Rome, at least in domestic affairs. Yet far from posing a constraint on Roman power, this republican constitution was, at least in the estimation of Deudney (2007), one of the causes of Rome's success.

Furthermore, Rome is not the only case of a more loosely organized or at least non-totalitarian state achieving systemic dominance. Achaemenid Persia was loosely organized, based on highly autonomous satrapies, yet it dominated the Middle East for two centuries. In modern times, the prediction from Møller's hypothesis would have been that the totalitarian USSR should have defeated the U.S., with its "multiple privileged groups," for global hegemony, yet the opposite happened. Møller's central hypothesis simply cannot stand on its own.

Møller also takes his focus on path-dependence too far. The real implication of his finding is that it took revolutionary change to build the kind of strong states in Europe that could make a plausible run at systemic hegemony, and *ancien regime* Europe never undertook such reforms. The contrast with ancient China is that Qin under Shang Yang did, undertaking reforms that amounted to a "revolution from above." That such an effort was possible in early modern Europe, too, is proved by the example of Ivan IV of Russia, who annihilated the merchant and boyar noble classes that had been a constraining influence on his predecessor by instituting a reign of terror (Yanov, 1981). This, in turn, suggests more credit for Hui's argument that the difference is internal policy, not initial internal structure.

And of course, the closest the European system came to hegemony was at the hands of leaders whose states had also undergone revolutionary change that yielded quasi-totalitarian control for those leaders. Napoleon is one example, coming to power after the French Revolution unlocked the power potential of the French state, enabling a *levee en masse* that paralleled Qin's ancient system of conscription. Nazi Germany is an even more obvious example, one of the original totalitarians states and the one that came closest to world domination. *Pace* Møller, the societal starting point is not decisive in determining later degrees of state centralization; and the degree of state centralization is not decisive in enabling systemic hegemony.

It is true, as Møller would suggest, that a strong and efficient state is a necessary condition for systemic hegemony, whether in the form of the ancient totalitarianism of Qin or Assyria, the tough republicanism of Rome, or the proto-federalism of ancient Persia. But other factors also influence whether hegemony or balance will be the systemic outcome. One of the most important is whether the boundaries of the system expand along with states' increased scope of control (Kaufman et al., 2007; Dehio, 1962). Thus Napoleon's hegemonic ambition failed in large part because Russia joined the earlier, smaller European system; and twentieth-century Germany failed because the U.S. did so as well. Another factor is economic productivity, which of course may be weakened by totalitarian political control: certainly this is why the U.S. bested the USSR in the Cold War (Wohlforth, 1995). Humanitarian norms also play a role: it seems at least probable that many potential hegemonic bids fail because the leaders recoil at the savage brutality that was integral to the rise of Qin, Assyria, or Shaka's Zulu empire. There is evidence that Shaka's predecessors did. Finally, there is the role of strategy. It is a truism that both Napoleon and Hitler could have bequeathed hegemonic power to their successors had they stopped to consolidate their gains before invading Russia—but it is no less true for being well-known.

In sum, Jorgen Møller is to be commended for identifying an important factor influencing whether international systems trend toward balance or hegemony. However, his argument is too monocausal to stand on its own. Such attempts at silver-bullet explanations tend to elicit debates of the form, "my favorite variable is more important than your favorite variable," which rarely yield much cumulation of knowledge. The most useful way of responding to Møller's hypothesis is to consider what it tells us in conjunction with other factors he leaves out.

Overpoising the Balance of Power?

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Jørgen Møller's starting observation and the historical outcome of the processes in China and Europe are easy to agree with. The question is what happens in between. I am not convinced that the balance of power analytic does as much as Møller wants it to do. Can we really say that the outcome in the European case is due to "the success of balancing against would-be hegemons" as Møller would have it, or could the reason lie elsewhere, as in the development of strong prescriptions about the autonomous character of sovereign states following the confessional fragmentation of Christendom? I am no expert on China, but the article raises a number of interesting issues regarding the treatment of early modern Europe. I raise two points here. The first one is immanent to Møller's take, while the second is more of an external critique.

The first point concerns the assumed geopolitical competition. My main contention here is that the assumed geopolitical competition was going on even long before the advent of the state in the 1550s. Taking a broad historical sweep (1100-1800) as Møller does I am unsure whether we can speak of system-wide geopolitical competition at all in the case of medieval and early modern Europe, because there was no real territorial political logic working at the time. Moreover, I am unsure whether we can speak of a continuous logic "of geopolitical competition" between the feudal battles of the knights in armor, the confessional turmoil of the long sixteenth century and beyond.

But if we, for the sake of the argument, accept Møller's assumption that the workings of a balance of power dynamic may operate without actors consciously engaging in the policy, could it have operated in medieval Europe? Møller's argument is that this hinges not upon the existence of a fully-fledged state system, but on system-wide geopolitical competition within an anarchical system. But did geopolitical competition occur enough in medieval Europe to warrant such an analysis? This is one of the central tenets of Møller's analysis. He bases it among others on Thomas Ertman's study of early modern European state formation (1997). Yet, Ertman does not make that claim. Rather, Ertman argues that "while geopolitical competition may have had a crucial impact on the statebuilding process, *the onset of such competition was "nonsimultaneous" – that is, it did not affect all states at the same time"*(1997: 26; emphasis added). Moreover, that difference in timing is central to explaining different outcomes, as different historical actors use different tools and follow different logics of action when (and *if*, I might add) they engage in geopolitical competition. Møller's broad sweep makes it possible to identify behavior symptomatic of anti-hegemonic balancing but that does not necessarily mean that there was a balance of power system in place. Put differently, opposition to a hegemonic bid may be necessary in detecting balancing, but is it sufficient?

Take for instance the Thirty Year's War. The reason why France, Sweden and Denmark entered the war was not solely to to stop a hegemonic bid, but largely enmeshed in the religious wars of the time and in order to guarantee the status of co-religionaries within the

Holy Roman Empire. Indeed, to the extent that we can detect a form of balancing in Europe before the 1650s, it is in terms of an emergent concern over a confessional equilibrium. Anti-Habsburg bids were therefore not necessarily conceived of as opposition to "territorial consolidation of princely power," as Møller would have it, but as anti-Catholic bids, broadly speaking (Carvalho, 2014). Furthermore, the widespread set of alliances Møller refers to were not all formal alliances with other states against the Habsburgs, but often aid to fellow Protestants. As a case in point, French aid to Calvinists in the Dutch Revolt often came from Huguenot commanders as well as "Valois France." The aid from "Valois France" withered after the Saint Bartholomew's eve massacre. Equally so, Tudor England did not ally with the Dutch against Spain, but provided mostly covert aid to the Calvinists. Tudor England was in fact allied with Habsburg Spain at the time (Carvalho & Paras, 2014).

My second point concerns Møller's treatment of balance of power. What he means by "balance of power" in the article is underspecified, which in turn makes the findings unclear. This would have been fine if balance of power "were free from philological, semantic, and theoretical confusion. Unfortunately, it is not." (Haas, 1953: 442) Ernst Haas drew our attention to the unclarity surrounding balance of power already in the 1950s. As he argued, balance of power has a number of different meanings, being both an analytical tool and "participant language." In a similar vein, Daniel Nexon has reminded us about the need to distinguish between balancing behavior and *the* balance of power (2009). Applied to the argument at hand, this means that we cannot assume from the behavior of a few actors that the balance of power is at work for the whole system. Simply put, actions to frustrate the Habsburg hegemonic bid are not necessarily indicative of a balance of power logic operating. Moreover, it raises the question of whether we can speak of balance of power at all when actors are not self-consciously engaging in it. As such, Møller's take on balancing takes too much for granted. A historical take such as Møller's should also be historically sensitive to the question of balancing itself.

Michel Foucault makes the case that balance of power thinking emerged in Europe towards the end of the 1600s, and did so because there was a change in the way one conceptualized force. Thereunto, he holds, it had been impossible to properly measure the force of a state, which again made balancing properly impossible as one had no shorthand for how to measure relative force (Foucault, 2004: 300-305). I am unsure whether an argument which uses the same conception of balancing for rulers engaged in "*raison de famille*" thinking, to paraphrase Daniel Green (2007), confessional "balancing" (Carvalho, 2014), and states increasingly engaged in the pursuit of *raison d'état* can yield the type of conclusions which Møller draws. In the former case, the geopolitical question is not as salient, in the case of the confessional makeup of Europe it was faith rather than geopolitics that mattered, and in the latter case, balance itself (both the means and the outcome in Møller's argument) was not only the outcome of competition, but a goal increasingly shared by the members of the European system (Hedley Bull in Little, 2007: 135). As a case in point, Foucault argues that both Swedish and French ambassadors to Osnabrück and Münster were instructed in the negotiations leading up to the Treaties of Westphalia to be sensitive to the question of equilibrium between European states (Foucault, 2004: 305). Historically speaking, then, balance of power emerges in Europe with the advent of the consolidated state. But as Hedley Bull also argued, there is no "inevitable tendency for a balance of power to arise" as states do not always engage in behavior aimed at increasing their relative power. According to Bull, balance of power emerges only when states consciously counteract other states in order to maintain a balance (Little, 2000: 406)

On the balance, then, Møller's piece is important as it continues the push towards historicizing the balance of power that a number of authors have recently undertaken (Wohlforth, et al, 2007). What is clear, though, is that the discipline needs more work on historicizing the balance of power, not only in terms of applying it analytically to past times, but understanding how historical actors made sense of their actions in terms of balancing (see for instance the work of Morten Skumsrud Andersen). If historical actors did not have a conception of balance of power, we may have to, as analysts, look for other ways of making sense of their actions.

MOLLER CONFIRMS "WAR AND STATE FORMATION IN ANCIENT CHINA AND EARLY MODERN EUROPE"

Victoria Hui
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It is such a pleasure that Møller's "Why Europe Avoided Hegemony: A Historical Perspective on the Balance of Power" confirms my argument in *War and State Formation in Ancient China and Early Modern Europe* (Cambridge University Press, 2005). Møller argues that, "from a historical perspective, the puzzle is not why balancing outcomes sometimes *fail* to occur, but rather why they occur at all," which is also exactly what I argue (Hui, 2005: 26). Møller answers the puzzle by analyzing "the concatenation of interstate and state-society balancing." Again, this is the same as my examination of "the mutual constitution of international competition and state formation" (Hui, 2005: 2). Where he thinks he disagrees, he apparently just misses the relevant discussions in my book. To that end, I outline how the two main features that he raises in his article – state-society relations and rebellions – are in reality fully consistent with my own argument.

First, Møller contends that while his analysis "corroborates a number of Hui's more specific mechanisms of domination and balancing," the state-society relations of Europe and China were always different, so that the former was constrained in matters of conquest while the latter wasn't. More specifically, Møller argues that the presence of numerous strong social groups "prior to the intensification of geopolitical pressure" forced rulers to bargain in what he refers to as a 'bottom-up' process (characteristic of Europe), while their absence allowed rulers "to mobilize the economy and strengthen the state in a 'top-down' manner" (characteristic of China). This Tillyan-sounding argument is a full confirmation of my historical comparison of ancient China and early modern Europe. Regarding differences at the outset, I argue that the early modern European system was constrained by pre-existing state-society relations – what I refer to in my book as "initial and environmental conditions" – while the ancient Chinese system began with relatively few encumbrances.

European feudalism, following Downing's definition (1992: 249), is surprisingly similar to Zhou feudalism in that both provided "elaborate normative restraints on the exercise of power and served the 'constitutional function' of checking arbitrary rule."[1] Nevertheless, because of the late timing in the onset of trade expansion relative to the onset of state formation in ancient China (as opposed to medieval Europe), early Chinese state-makers had no easy recourse but to centralize authority and administrative control in order to build up large armies and raise revenue. In 645 BC, this resulted in a crucial turning point in the

[1] Downing defines feudalism as "a decentralized form of government by which a relatively weak monarch rules in conjunction with an independent, beneficed aristocracy that controls local administration and constitutes the basis of the military" (1992: 249). Møller prefers to define feudalism by "vassalic contractualism." But this does not mean that other scholars cannot use Downing's definition. Møller also seems to dislike Creel, but then he cites Lewis and Hsu and other scholars on which my argument is based.

'feudal' relationship when military service and land tax were extended from the higher social class *guoren* to the more populous *yeren*, which greatly increased the ruler's relative capability and led to the gradual expansion of territory. In my book I add, "[s]uccess at territorial expansion, in turn, allowed rulers to extend military service and land tax to ever expanding populations, thereby blurring the distinction between *guoren* and *yeren*. Moreover, the same self-strengthening reforms that facilitated territorial expansion in international competition also facilitated rulers' amassing of coercive capabilities in state-society relations" (2005: 196).

Still, these self-strengthening reforms were tempered by the need of Chinese rulers to give concessions to the general population in order to motivate them to fight and die in war. In comparison, Europe's more monetized economy allowed rulers to contract out some of the responsibility and cost of conquest to intermediaries, which had the adverse affect of weakening rulers' access to coercive power (Hui, 2005: 50-52). While the prior existence of representative assemblies meant a much stronger balancing logic, European rulers were still able to at times circumvent constitutional constraints by claiming 'evident necessity' in the protection of the state and by using their armies to enforce their dictates for the purposes of collecting taxes. Thus, despite early modern Europe's ultimate constitutionalism and ancient China's eventual absolutism, the outcomes were not preordained.

The role of European intermediaries highlights another important similarity with Møller's discussion of state-society relations. We both believe that a major cause of divergence between Europe and China lies in the fact that there were multiple privileged social (and estate-based) orders in feudal Europe, while China only distinguished between nobility and peasantry. As I write in my book, since "the [Chinese] nobility already enjoyed privileged access to social status and economic benefits without fear of competition from other orders of society, there was little incentive to organize into a formal body" (2005: 196-197). Europe, on the other hand, also had the burgher and clergy classes. The burghers commanded immense bargaining power over cash-trapped rulers who badly needed money to go to war. The Church had large purse strings as well and from time to time supported resistance to lay rulers. Checks and balances, therefore, had much deeper roots in Europe in the early modern period. Even then, again, European rulers sought to aggrandize themselves and their territory, thus counteracting the stronger logic of balancing. It was simply not carved in stone that Europe should follow in England's constitutionalist footsteps at the end of Napoleonic Wars (Hui, 2005: 195-205).

Møller's second argument is based on the prevalence of rebellions in Europe and their rarity in China. He laments that a "detailed analysis" of ancient China "is precluded by the lower quality of available historical evidence." He then takes the rarity of sources to mean the rarity of rebellions. I agree that rebellions were not abundant in ancient China, but again I take the extra step of explaining why. I argue that the lack of strong social networks during the Warring States period left Chinese peasants vulnerable to state domination. Furthermore, by fostering an atmosphere of mistrust even among family members, "the Qin court could simultaneously maximize surveillance, minimize resistance, and lower the costs of domination. In short, Shang Yang achieved 'the ultimate dream of domination: to have the dominated exploit each other'"(Hui, 2005: 186). Moreover, Qin rulers were proficient – if not ruthless - at suppressing rebellions in conquered lands by engaging in "mass killing of royal families and defeated armies, enforced mass migration of noble and wealthy families to the capital, demolition of the six states' defense structures, imposition of direct rule with collective responsibility and mutual surveillance, establishment of settlements in problem-prone areas by Qin's convicts, and so on. Thus, no matter how disgruntled the subjects might be, the First Emperor was able to keep them in awe" (Hui, 2005: 218).

And while the Qin dynasty (221-206 BC) ultimately collapsed after the death of the First Emperor, the population in the prior Qin state (before 221 BC) never rebelled because the Qin king's extractions were by and large sustainable. The Qin state administered economic rewards and punishments to all classes in lieu of granting political rights, which had the effect of generating support for territorial expansion (Hui, 2005: 227). Given that state domination of the society had reached the high point by the time Qin launched the wars of unification, it is surprising that Møller compares the Qin with Habsburg Spain rather than Napoleonic France. If Møller insists on using Habsburg Spain as the reference, then the corresponding hegemon in ancient China should be the state of Wei. It is no coincidence that Wei's hegemony was brought down by the balance of power, something I discuss in chapter 2 of my book.

I am writing this post while watching and blogging about the Umbrella Movement in Hong Kong. It is often overlooked that China *is* a composite state. Because of Hong Kong's colonial legacy, Beijing had to promise Hong Kong "a high degree of autonomy" under the "one country, two systems" model when it signed the Sino-British Joint Declaration in 1984. The promises made to Hong Kong are functionally equivalent to the rights and privileges granted to city-states in medieval and early modern Europe. At the same time, just as European rulers tried their best to eliminate pockets of autonomy, Beijing is finding Hong Kong's autonomy intolerable. The one big difference is that this struggle for autonomy is live streamed for the world to see. And the world is nervously watching how this clash between a strong state and strong social forces will play out.

REPLY TO CRITICS

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I am indebted to Boucoyannis, de Carvalho, Hui, Kaufman, and Wohlforth for taking their time to read my article and write comments, which give much food for thought.

Let me start by briefly addressing de Carvalho's "external critique" in order to set up the arena within which I will engage the comments. By systems equilibrating on balance, I simply mean that political units interacting in anarchy do not generate a long-term tendency toward "domination over or subordination of other units by one especially capable actor," as Wohlforth puts it in his comment. My argument does not hinge on whether balance, in this sense, is a product of deliberate balancing or is the unanticipated consequence of actions with other purposes.

Space is too brief to respond to the numerous insights of the comments, so I single out three points, which – I believe – speak to most of them. First, is a good explanation in social science one that lists all sorts of relevant factors or one that emphasizes a core factor? Kaufman points out that while my paper "has identified an important dynamic," "other major causal factors" must also be taken into consideration to more fully explain the historical variation between balance and hegemony. I concede as much in my article (pp. 8-9). But I think the ideal of social science is nonetheless to identify and isolate particular explanatory factors (or, as in my case, a conjunction of explanatory factors) that have been neglected, but that claim to have wide consequences. Such an explanation is parsimonious, meaning that it explains a lot with a little, and it is open to challenge. Karl Popper (2002) once termed this a "bold conjecture" and argued that it tends to spark constructive debates that contribute to scientific progress. I believe the stimulating comments of this symposium give *prima facie* evidence of this. I therefore disagree with Kaufman that an explanation of the type I propose "rarely yields much cumulation of knowledge." On the contrary, I think that it is the kind of eclecticism often favored by historians, where explanatory factors are piled up without any account of a hierarchy between them that stifles cumulativity.

Second, several of the comments maintain that my argument ignores that endogenous transformation is possible. Kaufman and Wohlforth forward Russia as an example, pointing to the reforms under Ivan the Terrible (followed up by reforms under Peter the Great). I actually think Russia supports my argument rather well. Though historiography on medieval and early modern Russia is riven by disagreements, my reading of it is that Russia never had the multiplicity of privileged social groups that we know from medieval Western Europe. What we find in Russia is something that rather resembles ancient China: an undeveloped social differentiation dominated by a strong hereditary nobility (the Boyars) based on lineages rather than corporate rights, the Church subservient to the rulers, and the towns inconsequential (see e.g. Hosking, 2001). My explanation would predict that – in this context – geopolitical competition would strengthen the rulers at the expense of the nobility. This is exactly how I read the reigns of Ivan and Peter, where, following centuries of isolation, Russia became embroiled in geopolitical pressure first with Poland and later with Sweden.

This obviously does not prove that endogenous transformation cannot occur. But old habits die hard. As I point out at the end of my article, even the Western European instances of absolutism after 1600 were much more constrained than e.g. the Russia of Peter the Great, because much of the medieval legacy bequeathed by autonomous groups survived. According to Kaufman, all this shows is that ancient regime European states never attempted the kind of revolutionary transformation that was needed to "make a plausible run at systemic hegemony." However, as my paper documents, it was not for want of trying. European rulers failed because any successful drive for hegemony required the creation of state capacity in a top-down manner, which triggered collective resistance from the privileged groups who (correctly, as the Russian and Chinese cases show) feared for their traditional liberties. To relate this to a criticism made by Wohlforth, it was for this reason – and not due to any intrinsic opposition to external conquest – that the median organized group of medieval and early modern Europe frustrated expansion. Surely, it cannot be a coincidence that no European ruler prior to the French Revolution succeeded in wielding the gigantic broom of Karl Marx's (1988 [1871]: 54) famous phrase?

Hui also touches upon this issue, albeit from a somewhat different vantage point. As I note in my article, my analysis corroborates a number of the mechanisms set out in *War and State Formation in Ancient China and Early Modern Europe* (p. 9). Hui goes further, asserting that my article confirms her work *tout court*. However, in her comment she also emphasizes what I take to be a red thread in her book, namely that early modern European and ancient Chinese outcomes "were not preordained" and "not carved in stone." Now, both points can't be right. While I am pleased that Hui finds affinities between her masterful analysis and my own, I present an institutionalist explanation that stresses the path-dependent consequences of differences in initial conditions; she, as these quotations illustrate, an account centred on actors' choices, albeit constrained by factors such as the timing of commercialization. More particularly, in her book, Hui (2005: 195–205) first raises but then dismisses the possibility that the differing trajectories were the product of fundamental differences in initial state-society relations. As I point out in the article, my criticism of Hui follows from a reinterpretation of Chinese state-society relations, based on new evidence which documents just such a fundamental difference with medieval Europe. This is not a question of selecting one or another definition of feudalism, as Hui would have it, but concerns the empirical reality. The problem is that Hui bases her reading of initial state-society relations on outdated work by scholars such as Creel, that new archeological work, following the opening of China in the 1980s, has questioned.

Boucoyannis has a different critique about within-unit characteristics, arguing that the focus on privileged social groups is misplaced if we want to understand what it was about the European cases that were conducive to creating external balance. Boucoyannis's point is that the very ability of social groups to achieve rights and the very reason they fought so hard to secure them is that in Western Europe, state capacity was, from early on, greater than elsewhere. Furthermore, interstate balance was shored up exactly because powerful states were able to engage in genuine power-political competition. I disagree with the descriptive premise of this otherwise compelling argument. The parts of medieval Europe that experienced geopolitical competition around 1200 were not places where states were able to penetrate to the local level and grant and uphold rights. Even in England, which, as Boucoyannis correctly points out, had probably seen the most precocious strengthening of state capacity anywhere in Western Europe at the time, we find no standing army, no police force, and no local royal administration on the eve of the Hundred Years' War (Ormrod, 2000: 284-5; see also Bisson, 2009). The social groups singled out in my article stepped in exactly because there was little state infrastructure.

Third, if Western European state-society relations were so peculiar, what explains that? What were, as Boucoyannis asks, the origins of the autonomous groups of the West? De Carvalho emphasizes the importance of confessional fragmentation of Christendom following the Reformation as conducive to balance. If I were to finish this reply by making one more "bold conjecture," I would instead point to the impact of the Gregorian Revolution in the second half of the 11th century. This revolution was probably a relatively contingent event (see, e.g. Southern, 1970: 34, 96). But its effects were world-historical because the consequent split between religious and secular power opened up quasi-autonomous social domains that autonomous groups could inhabit. The corporate status of the clergy, including its right not to be summoned to secular courts, was the first institutional manifestation of this split. But other local communities, including the emerging towns, could step into these niches because of the vacuum created by the absence of state power.

This structural differentiation might shed some light on what otherwise is surely a puzzling case: the Roman Republic. As Kaufman and Wohlforth both point out, this seems an odd case of hegemony based on my explanatory model. The reason could be that while Rome certainly had strong societal groups, it did not have the kinds of quasi-independent social domains that were opened up by the split between church and state. In this context, the median organized group might stand to benefit by external conquest because this did not require sweeping away corporate rights. If correct, this might be said to somewhat diminish the extent to which my explanation can be generalized. But if it is ultimately the medieval conflict between religious and secular power that, via a long and twisted road, looms behind the European balance, it further underscores the take-home point of my article: that outcomes of balance are the historical exception, not the norm for multistate systems – and that a pure focus on systemic pressures, shorn of within-unit characteristics, does not serve to elucidate this variation.

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