

The Chinese Case Against Balance of Power

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Victoria Tin-bor Hui's *War and State Formation in Ancient China and Early Modern Europe* is a bold and original work. The book is bold in its goals and scope: the primary question motivating it is: "Why is it that political scientists and Europeanists take for granted checks and balances in European politics, while Chinese and sinologists take for granted a coercive universal empire in China?" (p. 1). This question is the starting point of an exploration of two very different outcomes in international politics. The first one is the "logic of balance", which replicates the classical Eurocentric perspective on the tendency towards equilibrium in international systems. The second one is the "logic of domination", illustrated in the book by Chinese theoretical perspectives and empirical example of a tendency toward universal domination. To answer the puzzle over the competing logics in this dichotomy the author will try to convince the reader of the usefulness of her "dynamic theory of world politics that blends Eurocentric and Sino-centric perspectives, connects the ancient and the modern, reconciles alternative trajectories and opposite outcomes, and incorporates persistent continuity and endogenous transformation" (p. 2).

Part of the originality (which ideally should not be regarded as original) comes from blending mainstream IR theory with the comparative politics literature on state-formation and state-society relations. Moreover, refreshing insights from classical political theory -both European and Chinese- are common throughout the book.

Her method and case selection are also creative and laudable. Tin-bor Hui compares the Ancient Chinese system during the Spring and Autumn period (656-453 BC) and Warring States period (453-221 BC) with the European system in the Early Modern period (defined as AD 1495-1815). Incorporating the case of Ancient China as an integral part of a theory of international politics, and thus departing from a too Eurocentric and temporarily restricted mainstream IR theory, is a praiseworthy project *per se*. Under the assumption that these systems are "comparable" she embraces the "uncommon foundation" method by using "paired comparisons of uncommon cases" (i.e. Mill's method of difference or most similar systems) in the search of common causal mechanisms that "combine differently with varying initial and environmental conditions to produce radically different outcomes" (p. 8).

The overarching goal can be seen as breaking a deterministic view of political processes as a teleology towards balancing -which naively avoids a serious engagement with a "logic of domination." The author creates two theoretical concepts as her basic tools in achieving this goal: "mechanisms of self-strengthening reforms" and "self-weakening expedients." Each of these would be the causal mechanism explaining the "logic of domination" in the former (i.e. the dissolution of a plural system during the Warring States period and the formation of the Qin Empire in China) and the "logic balancing" in the latter (i.e. the preservation of a system of autonomous units in Europe) (p. 34). The core of the theory (and this is an unjust but necessary simplification) is that certain structural and agential motivations induce states to engineer self-strengthening mechanisms (or self-weakening expedients) that open the opportunity for domination (or balance of power).

This reader could not agree more with the necessity of exploring alternative logics from the balance of power one. IR needs to engage more satisfactorily with the idea of domination and how it is achieved. The literature most linked to this concept is the one surrounding the "hegemonic stability theory" research program (see the work of Gilpin, Organski and Kugler, Keohane and Kindleberger). (1) The most relevant problem with this literature is that its preoccupation (obsession?) with war as a consequence of the end of hegemony (or of power transitions) leaves the primordial -and temporally and logically precedent!- matter of *how* states rise as a given independent variable -or, taking rise as the outcome, with a dependent variable that does not vary. The consequence is that IR has almost nothing to say about an essential part of the story of change in the system: how is it that the power to dominate is constructed.

Although Tin-bor Hui attempts to have the final word for this political phenomenon, her work remains incomplete and not entirely satisfactory for several reasons.

First, the book has a weak theory of state power. The explanation behind the outcome of systemic domination lies in a series of “self-strengthening mechanisms” (effective tax collection, meritocratic bureaucracy, ruthless military stratagems, inter alia) that enable a particular state to conquer the rest of the system. These mechanisms are the result of both agential initiatives and classical neorealist systemic emulation –i.e. states intra-system will tend to reproduce the same type of governmental mechanisms. The Qin state, located in an Ancient Chinese system composed by units pursuing such self-strengthening mechanisms, was enabled to achieve domination by applying these policies. The problem with this argument is that if we assume that these mechanisms are a systemic phenomenon, there is no convincing reason why it would be any easier for a self-strengthened state to conquer other self-strengthened states than it would be for a self-weakened state to conquer its pairs in another system. In other words, if the mechanisms are thought of as systemic, the advantages or disadvantages of pursuing self-strengthening or self-weakening mechanisms would balance out. Even more, it is not difficult to imagine the opposite logic being true –systems with “self-weakened states” would be more prone to Empire- since the existence of weak states opens a window of opportunity for conquer. (2)

The gist of the confusion rests in the author, consciously or unconsciously, taking the risk to ignore one of the most basic premises in the discipline: *power is relative*. The power of a particular state in international politics has no meaning if it is not seen as a characteristic of one unit in comparison to another unit or to the whole system. The consequence of neglecting this maxim might well be that the idea of “self-strengthening mechanisms” as the explanation of domination turns out to have almost a negligible heuristic power.

Moreover, her promise of a “dynamic” theory of change does not hold up to the expectations. In a sense, the author has a quite static perspective on history. While, as said before, she is probably right in arguing that the discipline has avoided a serious study of the issue of domination in international politics, the hypothesis that the road to domination lies in internal reorganization and administrative innovation is incomplete to say the least. There are structural and macro-historical processes of state growth that cannot be shunned in this type of analysis. The author would probably recognize that the Swiss cantons, even if implementing the most comprehensive self-strengthening reforms, were never capable of achieving domination of the European system. There is something special about states like France and Qin that made them able to engage in a quest for domination. This “something special” is one of the most basic and relevant processes in History: the rise and fall of political units. The reader cannot but be skeptical on how much of the causal mechanisms of domination lie in her self-strengthening ideas or in more macro-processes of rise and fall in the system. (3)

On the other hand, the author has a big underlying assumption about states’ motivation that should have been discussed in more depth. This is a kind of Mearsheimerian assumption that states *always* identify global (or regional) domination as the primary goal. There are several critiques that could follow from here. One could come from a “constitutive” perspective: if the units composing the systems are different in some essential elements, most probably the systems as a whole will be different in some way. One of the basic elements in the formation of the Modern European system was the creation of the idea of “sovereignty” as an inter-subjective constitutive characteristic of the system. Sovereignty needs to be part of an explanation, for example, of why Luxemburg could survive in a neighborhood of dangerous colossus –and at times domination seekers- such as France and eventually Germany. A Luxemburg located in Northern China, without the aid of sovereignty, would probably have fall victim to Qin might. (4)

While this does not mean that two different systems cannot be compared, the creation of inter-subjective norms in systemic dynamics should have a more relevant place than the one given in this book. (5)

Another theoretical concern is that self-strengthening and self-weakening mechanisms appear to be very prone to tautological and endogeneity problems. It might be argued that she is trying to explain state change by its effect -i.e. “X is a stronger state, because it was made stronger.” The ultimate

measurement of a self-strengthened state is to achieve domination; but domination is itself the dependent variable explained by the implementation of self-strengthening mechanisms. The idea that states apply self-strengthening mechanisms to dominate and dominate because they are self-strengthened is cyclical and problematic. (6)

Even more, her theory assumes that conquest pays, that is, conquest self-strengthens. This is not wrong *per se*. Nonetheless, it might get the theory in some logical quagmires. For example, Napoleon's quasi domination of Europe was so parallel to Revolutionary France self-strengthening reforms (administrative reorganization, revolutionary military techniques, etc.) that it would be difficult to say which was the cause of which.

Lastly, it may be the case that the author has a confused interpretation of her own mechanisms. Self-strengthening and self-weakening mechanisms appear to be causal mechanisms more linked to an explanation of "state power" than "systemic domination". (7) If this were true, she would need another set of mechanisms or variables to explain the occurrence of domination in international relations. An option to try to dissipate the doubts on how the mechanisms work would be to expand the cases studied to systems in which domination happened without self-strengthening (Philip's Macedonia comes as a possible example) or in which self-strengthening reforms did not come accompanied with domination (if we keep, as Tin Bor-hui does, domination as a necessarily territorial phenomenon, the United States in the Western Hemisphere comes to mind).

War and State Formation is an interesting book, which by opening several lanes of inquiry -and by sometimes giving weak arguments for the issues at stake- encourages new and original research in these important areas of political science. This alone is sufficient to make the book a worthy and entertaining read.

Notas:

(1) Admittedly, these works differ a lot from scholar to scholar.

(2) For a similar argument using Agent Base Modeling see Lars-Erik Cederman, *Emergent Actors in World Politics*, Princeton UP, Princeton, 1997, especially Chapter 4.

(3) Furthermore, there is an empirical problem related to the conceptual one before discussed: the picture of a weak Europe unable to efficiently sustain wars of domination must be seen in a global context in which the European powers were rapidly conquering the world.

(4) For an account of the importance of sovereignty as an essential constitutive characteristic of the European system of states see: Hendrik Spruyt, *The Sovereign State and Its Competitors*, Princeton UP, Princeton, 1994.

(5) This makes one wonder how useful is her theory for the contemporary world...

(6) This concern becomes clear when she argues that: "Domination-seekers in early modern Europe failed because they did not follow the logic of domination fully" (p. 109).

(7) Which may explain why this reader was incapable of finding a satisfactory definition of "domination".

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