effectively. However, de-emphasizing causality may impede the generalizability of his work and its application to cities with different demographics or cities where these factors change constantly due to immigration, emigration, or changes in occupational patterns. Yet for the large parts of South Asia that remain static in these terms, Brass's analysis sounds a cautionary note and offers a useful perspective.

Logics of Hierarchy: The Organization of Empires, States, and Military Occupations. By Alexander Cooley. Ithaca, NY: Cornell University Press, 2005. 191p. $35.00 cloth.

War and State Formation in Ancient China and Early Modern Europe. By Victoria Tin-bor Hui. New York: Cambridge University Press, 2005. 294p. $70.00 cloth, $24.99 paper.
DOI: 10.1017/S153759270707048X

— Abdulkadeer Sinno, *Indiana University*

These two ambitious books by Alexander Cooley and Victoria Tin-bor Hui address the consequences of managerial decisions within empires. Cooley's book borrows organizational models from the field of business management to explain the growing pains of empires and the misfortunes of newly independent states, with a particular focus on post-Soviet Central Asia. Hui draws on classical authors to explain how basic structural administrative choices within states affect systemic competition.

Cooley's first three chapters make the case that hierarchy matters in international politics and that not all hierarchies are equal. Two types of organizational structures produce distinct dynamics with potent explanatory power: the U-form and M-form. U-form organizations have highly centralized structures with specialized branches that need to engage in constant coordination to get things done. M-form organizations have highly decentralized, relatively autonomous, and largely self-sufficient units. Empires that organize along the U-form by expanding their specialized institutions' tentacles to new possessions tend to better assimilate their peripheries but are costlier to establish and suffer from intra-agency competition. If they succeed, they become well-integrated states. Empires that organize along the M-form are less costly to establish but suffer from corruption in the periphery because of information asymmetry and opportunistic behavior by the periphery's elites. Once empires collapse, U-form peripheral institutions become irrelevant because they lose their place in an integrated hierarchy, while M-form institutions survive but continue to suffer from the legacy of patronage, opportunism, and corruption that characterized them during the era of subordination.

Empires are often a mix of U- and M-form hierarchies, according to Cooley. The USSR maintained U-form (armed forces and defense industry), M-form (agriculture), and mixed-form (internal security) institutions in its Central Asian periphery. Cooley argues in Chapter 4 that this configuration led to the "harmonization" (p. 93) of U-form institutions across the empire and the spread of corruption and opportunism in M-form institutions of the periphery. In Chapter 5, he shows that U-form institutional remnants collapsed in postcommunist Central Asia while M-form ones persevered with the same corrupt patrimonial practices that ailed them when they were part of the Soviet empire. One of the sad and convincing consequences of his argument is that aid poured into postcommunist states perversely extended the lives of their dysfunctional and corrupt M-form residual institutions.

Cooley's Chapters 6 and 7 reflect his theoretical ambitions. He extends the explanatory power of his organizational dichotomy to other empires and spheres of interaction. Chapter 6 argues that the U/M dichotomy (or the attempt to transition from M to U in the first instance) explains the collapse of Yugoslavia, the different manifestations of Korea's postcolonial economic development, and the failures of the ongoing U.S. venture in Iraq. In Chapter 7, Cooley attempts to convince the reader that the U/M dichotomy also explains the existence of different monetary regimes and tax havens (many of which are M-form dependencies), the "harmonizing" effect of U-form international credit rating agencies, and the "opportunistic" behavior of nongovernmental organizations (p. 160). The ultimate goal of this exercise is to provide a compelling rationalist theory that undermines the constructivist challenge to existing international relations rationalist theories. Cooley develops a compelling theory but his desire to cover as many applications as possible makes his analyses seem superficial. Chapter 7 left me wondering whether his organizational dichotomy could be legitimately extended to such disparate areas and whether the generalizations he makes are warranted. For example, could the M-form Amnesty International and Médecins sans Frontières "pursue narrow organizational interests" (pp. 175–76) or even be corrupt and patrimonial as the theory would predict? It may be the case, but the author provides us with little evidence to back his thesis.

The idea that organizational structures provide a powerful set of behavioral incentives and restraints that explain much in domestic, transnational, and international politics and bridge the three conceptual domains is quite powerful. But why should there be only two modes of organization (and combinations thereof) that explain interesting behavioral differences across areas as diverse as those where the author ventures? Two distinct types of organizations that Cooley confounds in his search for parsimony, for example, are clientelism or contracting (e.g., contracting firms in Iraq or Hui's feudal lords and tax farmers) and decentralized agency (e.g., his viceroys or Hui's salaried generals). They produce different dynamics because clients and contractors have lower exit costs than agents, as many anthropological and management studies have shown. The author also does not explore whether all M- and U-forms are equal—there are good and poor ways to structure and manage both categories of organizations. Last, Cooley assumes that the organizations of empire are the only ones to ultimately shape the post-imperial state's institutions. This is not the case in zones of imperial conflict such as today's Iraq or (post)colonial Algeria where rival organizations interact strategically with each other, the state, and imperial institutions. The outcome is not likely to only be defined by the institutional legacy of the empire.

Victoria Hui argues that empires expand when states improve their ability to extract resources and mobilize armies at a reasonable cost through effective centralized bureaucracies (similar to Cooley's U-form) when others within the system fail to keep pace with their managerial prowess. The development of such a competitive administrative advantage gained through "self-strengthening reforms," if accompanied by "divide-and-conquer-strategies and Machiavellian stratagems" (pp. 224–25), could lead to the consolidation of a multistate system under a unified empire. When such states emerge, the "logic of domination" prevails. Conversely, states that weaken their own administrations by selling offices and relying on hired mercenaries and tax farmers (what she calls "self-weakening expedients" and could be compared to Cooley's M-form) do not have the stamina to wage wars. The "logic of balancing" prevails in systems consisting of such states or of states that centralize vital activities simultaneously.

Hui develops her "dynamic theory of world politics" (p. 1) in Chapter 1. In Chapter 2, she tests her theory with a convincing study of China from 656 to 284 B.C.E. when the logic of balancing prevailed and from 356 to 221 B.C.E. when Qin rose from relative weakness to unify China. She finds that Qin was able to engage in the kind of sustained warfare that allows the development of the logic of domination because of the creation of bureaucratic government run by meritocratic elites, the introduction of military conscription, and the direct taxation of farmers. In Chapter 3, Hui traces a similar relationship between "self-weakening expedients" and balancing behavior, on one hand, and between "self-strengthening reforms" and dramatic state expansions, on the other, in early modern European history (1494–1815). In Chapter 4, Hui traces the effects of international competition on state-society compacts in China and Europe. She argues that liberalization and state services grow when weak rulers need to cultivate the support of the population to become more competitive internationally. Chapter 5 is a short conclusion that summarizes the book's argument and briefly extends it to the early years of the post-Cold War era.

Hui's well-crafted and compelling volume has two minor weaknesses. The first is that she relies too heavily and too uncritically on classical texts, particularly Machiavelli and Sunzi, to understand motivations and calculations in ancient China and early modern Europe. Second, her narrative relies on a large number of factors to supplement her administrative argument. Those include geographic and economic contingency, path dependence, military innovation (e.g., guerilla warfare in Spain or Qin's innovative strategies), the galvanizing effect of revolution, and leaders' intelligence (e.g., Napoleon, p. 229). Those factors explain much in the narrative but are not part of the theory. They therefore appear to reduce the ability of Hui's theory to predict systemic behavior or to allow cross-system comparisons, but she mitigates their effect by engaging in careful process tracing.

These two books have very different styles, approaches, and methodologies. Cooley wants to show that his organizational dichotomy explains much across the discipline while Hui merely "hope[s] only to take the first step toward broad comparisons of whole system" (p. 7). Hui meticulously crafts well-documented narratives grounded in the comparative method while Cooley is frugal with evidence and does little process tracing. Cooley convincingly develops a parsimonious theory that many social scientists would envy while Hui sometimes struggles with the complexity of hers. Yet, both develop powerful overlapping organizational and administrative theories that are likely to influence scholars across the subfields, if only because of their ability to provide frameworks that convincingly connect domestic, transnational, and international political dynamics. The authors' overlapping findings—in spite of different approaches, methods, historical eras, and cases—should increase confidence in both of their theories. These two books deserve attention from comparativists, IR scholars, and those who wish to do away with the subdisciplines.

Case Studies and Theory Development in the Social Sciences. By Alexander L. George and Andrew Bennett. Cambridge, MA: MIT Press, 2005. 331p. $50.00 cloth, $20.00 paper.
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— Sheri Berman, *Barnard College*

In recent years, there has been a surge in work on what has come to be known as "qualitative methods." The trend is essentially reactive, developing as a response to the outpouring of work on quantitative and formal methods and the assertions by scholars in those areas that case studies and historical work are impressionistic, unscientific, and noncumulative. To counter such claims, some of the field's most distinguished qualitative scholars (e.g., Stephan Van Evera, *Guide to Methods for Students of Political Science,* 1997; James Mahoney and Dietrich Rueschemeyer, eds., *Historical Analysis in the Social Sciences,* 2003; and Marc Trachtenberg, *The Craft of International History,* 2006) have spent much time and ink to show that researchers who eschew regressions or game theory can be just as methodologically aware and sophisticated