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

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as those who embrace them. Alexander George and Andrew Bennett's *Case Studies and Theory Development in the Social Sciences* is an impressive and welcome addition to this literature.

Alexander George has helped to explicate and develop the case study method for decades, and with his former student Andrew Bennett he has produced a book that is not only clear and comprehensive but also bold and frank. The authors' first contribution is to offer a systematic guide to the nuts and bolts of case study–based research. There are chapters on designing, executing, and analyzing the results of case studies; on distinguishing structured, focused comparison, controlled comparison, and within-case analysis; on case studies and the philosophy of social science; and on two particular types of work that George and Bennett advocate, process tracing and the congruence method. Taken together, these chapters tell students and fellow practitioners all they need to know (and more) about how to go about such research.

However, the book also offers a spirited defense of *why* scholars should do it—a subject that other recent books on qualitative methods have neglected. Two of the authors' arguments are worth special mention, those relating to causality and to the generation of new insights, hypotheses, and theories.

George and Bennett argue that case studies offer the best, and perhaps only, way to explore causality. Only by carefully studying the unfolding and dynamics of particular cases, they claim, can scholars figure out precisely what mechanisms and variables caused a particular political outcome to occur. Case study analysis can "check for spuriousness," "document . . . alternative causal paths to the same outcome and alternative outcomes for the same causal factor," and remains "the only observational means of moving beyond covariation alone as a source of causal inference" (pp. 223–24). "Even when rational choice theory or other formal models predict outcomes with a fairly high degree of accuracy," the authors note, "they do not constitute acceptable causal explanations unless they demonstrate . . . that their posited or implied causal mechanisms were in fact operative in the predicted cases" (p. 208). By reminding us that the central challenge of political science is understanding *why* political outcomes occur, they perform a great service to a discipline that in recent years has grown unhealthily obsessed with hypothesis testing (or the "logic of confirmation") alone.

George and Bennett are also keen to assert the contributions that case studies can make to the generation of new thinking. Here they take aim at the claim made by Gary King, Robert Keohane, and Sidney Verba (1994) in *Designing Social Inquiry* that "there is no such thing as a logical method of having new ideas. . . . Discovery contains 'an irrational element,' or a 'creative intuition'" (p. 12). George and Bennett agree that there is no surefire way to produce new ideas, but argue that case study work is uniquely well positioned to uncover "variables that were otherwise left out in the initial model" and generate new hypotheses "through the study of deviant or outlier cases and in the course of field work" (p. 20). Statistical methods, in contrast, "can identify deviant cases that may lead to new hypotheses, but in and of themselves . . . lack any clear means of actually identifying new hypotheses" (p. 21).

In sum, *Case Studies and Theory Development in the Social Sciences* aims to show not only that case study–based work can be as sophisticated and methodologically aware as quantitative and formal work, but also that it offers distinctive advantages in addressing causality and producing fresh thinking. With its fairly accessible style and helpful tips for teaching, this should make it a must-read for all political scientists interested in methodology.

Overcoming Apartheid: Can Truth Reconcile a Divided Nation? By James L. Gibson. New York: Russell Sage Foundation, 2004. 448p. $47.50 cloth, $22.50 paper.
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— William A. Munro, *Illinois Wesleyan University*

In this engaging and meticulously constructed statistical analysis of attitudes among South Africa's apartheid-defined racial groups (African/black, Coloured, Asian, white), James Gibson sets out to do two things. One is to map the state of political reconciliation among these groups. The second is to measure the impact of South Africa's truth and reconciliation process, focusing on the Truth and Reconciliation Commission (TRC), in promoting reconciliation. The analysis is based largely on a national opinion survey carried out in 2000 and 2001, but also draws comparatively, where possible and appropriate, on a similar survey from 1997.

Taking reconciliation as a multidimensional concept, Gibson measures it along four distinct "subdimensions." At the *individual* level, he assesses the degree to which South Africans express equal respect for members of other groups. At the *cultural* level, he measures South Africans' commitment to the rule of law (as a proxy for the human rights culture the TRC sought to build). At the *group* level, he assesses the levels of tolerance that South Africans feel for their political enemies. And at the *institutional* level, he assesses South Africans' loyalty to the new institutional order by examining their attachment to parliament and to the constitutional court. The book walks the reader through each of these measures, and it is an enlightening walk. Overall, the findings are sobering: South Africans are not very reconciled (partly because of racial isolation); no group is strongly attached to the rule of law, though whites are more attached than blacks; South Africans tend to be quite intolerant of their political enemies, and intolerance breaks down significantly along racial lines (though notably, South Africans across racial groups are

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