international relations scholarship. Ideally, there ought to be space for the kind of work advocated here and that done in the conventionally defined subfields of IR, though, of course, relatively impermeable territorial boundaries often do exist, as the authors J. Ann Tickner, Marysia Zalewski, and Weldon highlight. All should understand, however, that there is equal value in studying state behavior without deconstructing the state, and in deconstructing the state to examine its foundation on gender power structures.

A second critique—which might be too easily, and unfortunately, dismissed as a question unique to those who accept dominant IR thinking—is the volume's lack of criteria for evaluating feminist methodologies. It contains a wealth of information regarding how to determine whether or not a methodology is feminist but not how to judge the rigor of such research. This is no small concern. Imagine having to review a manuscript employing a feminist methodology. Assuming all else equal, three referees would likely offer different accounts of the methodological concerns. Even three reviewers identified as feminist methodologists would likely leave an editor unclear with respect to the manuscript's methodological rigor. Feminist methodologies, as noted in the book, are not necessarily replicable and, according to Carol Cohn, often focus on "understanding" rather than "validity." In her chapter "Motives and Methods: Using Multi-sited Ethnography to Study U.S. National Security Discourses," Cohn writes: "I was not trying to prove a point or test a hypothesis, but to see what was there and think about it" (p. 104). She continues: "[There is not] one, true, accurate understanding to which any one of us has privileged access. . . . Each of us will bring different insights to understanding and interpreting that complexity, if we 'listen to the material.'" (p. 105). There exists a fine line between requiring narrow criteria for IR methodology and erasing criteria of epistemic validity altogether. And it is not clear that this volume always navigates this line carefully.

Ultimately, the essays paint a broad-stroke picture of feminist IR methodologies, approaches, and concerns. The essays make clear that feminist concerns are not limited to international relations scholarship narrowly construed, but rather extend to questions of military policy (as discussed by Cohn and Kronsell) and national security more generally (as explored by Jacoby and Maria Stern). Indeed, the volume makes clear that feminist methodologies blur almost all conventional disciplinary boundaries (see, for example, Christine Sylvester's argument for art criticism to be accepted as a valid IR methodology).

This edited volume is instructive in illuminating feminist methodologies and in highlighting the insights gained from them, and in many ways it provides useful cultural insights into the orientations and practices of feminist IR scholars. It tackles a difficult topic and should serve as a catalyst for further debate. Ackerly, Stern, and True make a sizable contribution in providing insight into feminist methodologies and concerns and encouraging the reader to assess her own biases; to question "knowledge," discipline boundaries, and definitions; to identify assumptions and exclusions; and to recognize the necessity of including gender in research—it is tempting to include the phrase "where relevant," though the authors do make a solid argument that gender is always relevant.

Scare Tactics: The Politics of International Rivalry.
By Michael P. Colaresi. Syracuse, NY: Syracuse University Press, 2005. 232p. $34.95.
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— Brandon Valeriano, *University of Illinois at Chicago*

The emerging research program of international rivalries is an important branch of study within the international relations subfield. Rather than focus on rare events such as war, this research program seeks to understand the dynamics at work for what many scholars perceive to be the most dangerous pairs of interstate enemies in the system. Those enemies are historical rivals, typically coded as strategic or enduring rivals. In a rivalry, hatred is endemic and cooperation may be rare. These actors clearly are of a different sort than the rest of the system and need to be studied in their historical context.

While much work has been done to account for who the rivals are and why they might be important, little work has been done to explain how rivalries emerge, escalate, and terminate. It is from this research gap that Michael Colaresi's book emerges. It is an important work that covers the domestic and international causes of escalation and de-escalation of rival states. Little work has been done at this point to connect domestic political motivations with rivalry dynamics. Colaresi does an excellent job of moving between the levels of domestic and international action to present a coherent theory of international action.

The author's theory of dynamic two-level pressures centers on a very simple combination of internal domestic pressures and a state's future expectations. The first pressure is termed rivalry outbidding. If a pair of states is confronted with a public and a set of elites who wish to continue and escalate a rivalry, there is little opportunity for a conflict to be resolved even in the context of a democratic system. Leaders who seek to terminate an external threat may not retain their grip on power if they rid a state of their important external enemies that may be critical for internal support (see, for example, Siad Barre in Ethiopia and his support from Ogaden clansmen). Even democracies are not immune to this effect in that opposing elites may use the peaceful inclinations of a leader to signal their overall weakness in dealing with a threatening actor and thereafter remove the actor from office in the next election (see, for example, Jimmy Carter and Ronald Reagan).

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The second level exhibiting pressure on international actors is the condition of future expectations. States and leaders are aware of future costs and expectations relevant to an international conflict. If they feel they will lose the upper hand in the future, then it will be better to escalate the conflict now than in the future when their military position will be weaker. Colaresi calls this theory the dynamic two-level pressure theory and it works exceedingly well throughout the book. The push and pull of domestic conditions as well as the international situation conflate to either push states to continue a rivalry or pull them to terminate a conflict. This interaction is continuous and thus dynamic.

To test the dynamic two-level pressure theory, Colaresi uses a multimethod approach. Examining the Somali-Ethiopian, Egyptian-Israeli, and Sino-American rivalries with the structured, focused case study method, he is able to construct a variable oriented narrative that illustrates his theory at work. After the case studies, he "zooms out" and uses large-n statistical techniques to test his hypotheses with daily events data. Once again, he finds strong support for his theory where the variables of rivalry outbidding and increased future expectations are observed. He also finds support for de-escalation predictions if the rival dyad has a common external enemy. Issues under contention of a salient nature (usually territory) are also of importance for escalation and de-escalation (if the issue is settled). His statistical tests are impressive for incorporating relevant controls and dealing with a potential selection-effects problem by testing for its existence.

Colaresi's case studies are equally impressive and also timely (particularly the Ethiopia-Somalia rivalry case). His writing style is clear and concise. Some researchers may overlook the historical discussion contained in the case studies in the rush to judge the statistical models, but these case discussions are important for theory construction and subsequent testing. Furthermore, the case studies are important for readers who may be unconvinced by statistics. The author is also able to incorporate strong statistical measures into his case studies with the use of daily events data that show the ebb and flow of the rivalry throughout time.

The book contains a few flaws. First of all, Colaresi has to deal with the problem that data is not available to test his true theory. Therefore, the operationalizations he uses for rivalry outbidding and future expectations may not get at the true meaning of the terms. He does well enough to contort available data into close approximations of his theoretical concepts, yet there is a divergence between what the case studies would consider a negative future expectation of conflict and what is tested in the statistical model. Furthermore, Colaresi, and all researchers for that matter, should present predicted probabilities so that unfamiliar researchers and students may be better able to grasp the importance of the categorical data outputs.

Another potential problem is with the dyadic propositions in the theory. It is unclear if there is truly a dyadic process at work here. For instance, in the Somalia-Ethiopia case, the rivalry outbidding that resulted in escalation seemed to be confined to one side (geographic maps might also have been helpful for the reader unfamiliar with some regions covered). Are dual interactions important, or will the theory work just as well if only one side experiences outbidding and high future costs? Timing issues are typically ignored in international relations research, yet they are critical to the understanding and testing of theoretical propositions. Do these processes work at the same time? Is there more likely to be rivalry escalation if an autocracy escalates first? These are all important questions, yet they cannot be covered since the theory and the data do not currently account for the timing of events. In addition, I would have liked to see a greater emphasis on the importance of issues under contention in the statistical tests (they are clearly important for the case studies). Might the issue variables more correctly account for the variance at work?

Colaresi's work is important in that scholars and the public at large know very little of the process of escalation and de-escalation in the context of long-standing rivals. The work is also critical in that researchers must now begin to focus on how the de-escalation process works in real-time situations. It is clear that scholars should start to incorporate the domestic level into their international relations theories. Without a theory of domestic political pressures, a theory of rivalry escalation and termination would be empty. Second of all, it is important that scholars begin to tackle the question of how the diametrically opposing processes of peace and escalation originate. In the context of rivalry, there are many important questions and answers to be developed. Colaresi has made a good start, and it is hoped that in combination with other efforts, there might be progress toward an accumulation of knowledge in uncovering the rivalry dynamics and processes at work in these important conflictual pairs of states.

Fostering Fundamentalism: Terrorism, Democracy and American Engagement in Central Asia. By Matthew Crosston. Burlington, VT: Ashgate, 2006. 186p. $89.95 cloth.
DOI: 10.1017/S153759270707209X

— Roger D. Kangas, *The George C. Marshall Center*

In his book, Matthew Crosston lays out an interesting and worthwhile argument: that by focusing on short-term security assistance and long-term democracy building in authoritarian regimes, the United States is unwittingly creating conditions for extremism and anti-American sentiments throughout the world. His case study is the region of Central Asia—one that had largely been ignored by policymakers until the need for non-OPEC energy that increased in the 1990s and the military actions

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