

BOOK REVIEWS

Some Systemic Roots of The Democratic Peace

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Puzzles of the Democratic Peace: Theory, Geopolitics and the Transformation of World Politics. By Karen Rasler and William R. Thompson. London: Palgrave Macmillan, 2005. 288 pp., \$79.95 cloth (ISBN: 1-4039-6823-3), \$30.95 paper (ISBN: 1-4039-6824-1).

Whatever happened to systemic analysis in the study of international relations? According to Karen Rasler and William Thompson in *Puzzles of the Democratic Peace: Theory, Geopolitics and the Transformation of World Politics*, at least part of the answer lies in the deluge of dyadic analyses associated with the rise of democratic peace research in the 1990s. However, the “abandonment of a systems perspective is premature” (p. 177), they contend, and *Puzzles of the Democratic Peace* is aimed, at least in part, in bringing systemic approaches back to the center in the study of international relations.

Rasler and Thompson recognize that they have a tough fight on their hands. The democratic peace observation—that democracies are less likely to fight each other than other kinds of regimes—is well established, and they concede that there “must be something going on” (p. 37). Observation, however, is not causation, and not one of the many theories of how democracy might cause peace has yielded much meaningful, clear-cut, and nontrivial predictive power—achievements that lie at the heart of the scientific identification of causality. Nor have scholars paid much attention to the causes of democracy, with many jumping on an “analytical bandwagon” (p. 139) that takes an “immaculate conception” approach to regime type (p. 112). In particular, Rasler and Thompson argue, “theories are constructed too ahistorically,” with little consideration of the timing of democratization (pp. 140–142).

As an alternative, Rasler and Thompson offer a systemic model that combines Richard Rosecrance’s (1986) ideas of how trade reduces conflict with some systemic classics, including Long Cycle Theory (Thompson 1983; Modelski 1985). The main starting point is an assumption of path dependency, which asserts that history influences state choices and preferences (such as irredentist ambitions), which, as a result, “are apt to persist regardless of changes in regime” (p. 95). Geography also matters because insecure borders encourage states to construct larger bureaucracies with stronger extraction capabilities. As a result, power and resources become concentrated. Maritime borders, or having neighbors that do not seek regional expansion, reduce these pressures, giving rise instead to greater external trade.

States that engage in trade can extract resources with smaller bureaucracies by taxing trade. Thus, global expansion can be done inexpensively, and states that choose global rather than regional expansion face fewer threats in their home regions (p. 63). States that choose global expansion and emerge as system leaders tend to protect other states that choose to trade (p. 23). In this process, timing matters. States that choose trading over regional expansion are able to refrain from

domestic power concentration, setting on a path that creates a greater opportunity for successful democratization later on (pp. 45–46). Democratic system leaders impose democracy on losers (p. 188), and the world has “evolved to the point where coercive strategies are becoming more costly” (p. 5). In these ways, an emerging peace is at least partly a systemic phenomena, and path dependency, evolutionary processes, state choices, and geopolitics may “be more important” than regime type in explaining it.

This story is plausible, and it should be taken seriously by scholars interested in the causes of war and peace. As with most theories, however, it is not without its faults. Rasler and Thompson are explicit that this is a work in progress, and they identify many of the weaknesses themselves. Foremost, they acknowledge that their model is not likely to appeal to adherents of parsimony (p. 86). The model contains multiple paths of identified causation involving many variables. For instance, domestic power concentration is thought to be a consequence as well as a cause of regional expansion (pp. 47, 65); warfare is a cause as well as a consequence of path dependency (p. 110).

Nor is the model likely to be popular with those who prefer assumption-driven, deductive theorizing. In *Puzzles of the Democratic Peace*, most assertions of empirical relationships derive from observation rather than theory or assumptions. Some of the story, and much more of the history, sometimes appears to be ad hoc. For instance, we are told that state choices “vary by political economy type,” and that the Netherlands and the United Kingdom had advantages because “their revenue gathering techniques were more pioneering,” “their economies were more innovative,” and they were “good credit risks” (p. 141). Given that we are never told why this is, however, we are left to conclude that these may be yet more exogenous variables. Similarly, Rasler and Thompson assert that early “merchant oligarchies” helped “establish a republican tradition” (p. 25). Unfortunately, this strong statement of cause and effect is treated in a hurried manner. In the subfield of comparative politics, the influence of merchants is not a variable in democratization research (for more on the assertion of this cause and effect, see Mousseau 2003a, 2003b).

The ultimate value of a theory, however, rests on its empirical success, and Rasler and Thompson take on the arduous venture of exploring 24 hypotheses associated with their story. They acknowledge the enormity of their task. First, the broad thesis “in progress” borders on unfalsifiability. Second, many of the concepts, such as “threat,” are hard to measure in a broadly convincing way. Finally, the systemic approach requires simulation or simultaneous equation analysis, neither of which can be used because of poor data availability. Yet, Rasler and Thompson never make claims beyond what the data can sustain. They concede that their analyses can only establish the plausibility of their perspective, and they describe their results as “a cautionary tale about relying too heavily on a single variable” (p. 135). In this regard, they are convincing. One cannot leave *Puzzles of the Democratic Peace* without recognizing that geopolitics may play at least some role in explaining the democratic peace observation.

Puzzles of the Democratic Peace is a must read for scholars of interstate conflict and can be a valued asset in graduate courses. (The complexity of the theory and empirical methods is probably too much for most undergraduates.) The book contains a nice mix of history, theory, and both quantitative and qualitative analysis. It exposes readers to some of the classics in systemic level research and introduces some interesting modifications to Long Cycle Theory—including new distinctions between regional and global forms of concentration. At the same time, earlier versions of most chapters will be familiar to readers who have followed Rasler and Thompson’s research program in recent years.

The most important aspect of *Puzzles of the Democratic Peace* may be that it moves us beyond hoary realist and liberal platitudes for and against the democratic peace

theorem. Instead, we get due recognition of the crucial distinction between observation and causation. The democratic peace observation holds firm, but the analyses suggest that the term “bestows too much credit on one variable” (p. 220). In this way, the book serves as a wake-up call for scholars to grasp the negative consequences of the sociology of our discipline, which in this case may be bandwagoning too quickly around democracy as a cause of the “democratic peace.”

References

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