evidence supporting its arguments, place it on the short list of required readings for any student of global governance.

The Social Evolution of International Politics. By Shiping Tang. New York: Oxford University Press 2013. 296p. $99.00 cloth, $39.95 paper.

— Fred Chernoff, *Colgate University*

International relations debates have become increasingly dominated by structural theories since Robert O. Keohane and Joseph S. Nye, Jr. published their institutional argument in *Power and Interdependence* (1977). The structural approach was soon bolstered by Kenneth Waltz's neorealist *Theory of International Politics* (1979), and reinforced 20 years later on a different front by Alexander Wendt's constructivist *Social Theory of International Politics* (1999). In his interesting and far-reaching contribution to the grand theory debate, Shiping Tang argues that reliance on structural theories' dominance has been a mistake. Structural theories can never be explanatorily adequate because structure's causal role is more limited than is generally recognized and because other elements of systems (e.g., geography) have been overlooked. *Tang maintains that only a social evolutionary paradigm (SEP) can provide an adequate account of the behavior of states and systems.*

Tang says that there was once a "paradise" with no large-scale, organized intergroup violence. Eight or 10 thousand years ago, an "offensive realist" system of war arose; in recent decades another transformation produced a defensive realist world, and there is now movement toward a more rule-governed world. He asks how these transformations can be explained and answers that only the SEP can do the job. The SEP subsumes many micro- and mid-level mechanisms, and indeed subsumes all of the 10 other paradigms, which, in his view, qualifies it as the "ultimate paradigm." The SEP provides not only a social science explanation of the system but also a "macro-sociology of IR schools."

The author's strategy is to build on ideas and processes that are part of evolutionary biology. He rejects all reductionist arguments, including those like Bradley Thayer's attempt in *Darwin and International Relations* (2004) to explain IR in terms of biology. Moreover, he also rejects all previous attempts to devise social evolutionary theories of IR largely because they "did not specify genes, phenotypes, sources of mutation, sources of selection pressure, and mechanisms" (p. 32). Rather, he takes the logic of evolutionary biology, with its causal process of variation-selection-inheritance, and adds a parallel social evolutionary process applied to interstate systems and subsystems. *Hence, Tang's SEP is parallel to a biological paradigm but is distinct from it.*

Two of the great strengths of *The Social Evolution of International Politics* are its evidentiary base and the explicit statement of evaluative criteria. Tang provides evidence from ancient, medieval, and modern international systems, both Eastern and Western. Decreases in the numbers of states and increases in size show the success of conquest strategies and the presence of an offensive realist world; the subsequent stability of the size and number of states shows the failure, and abandonment, of conquest strategies and the transition to a defensive realist world. Tang's marshaling of evidence is impressive, though there are occasional slips (e.g., the claim that Athens massacred all the adult males after the revolt at Mytilene). His highly systematic argument draws on scholarship in genetics and evolutionary biology, as well as archeology, sociology, anthropology, evolutionary psychology, political science, and economics.

With respect to criteria, Tang says that neo-Darwinian biology is—along with heliocentric astronomy—one of the two pillars of modern science. Evolutionary biology enjoys this status for three reasons: its elegance and parsimony, its subsumption of all other micro- and meso-level biological mechanisms, and its rendering unnecessary any exogenous explanation of evolution (p. 15). It is an admirable strength of transparency that Tang makes explicit the criteria on which a scientific theory (including his own) should be judged. I have recently argued that scholars' failure to specify the precise criteria we use has impeded our ability to deal with one another's conflicting hypotheses and theories, and thus has impeded explanatory progress (Fred Chernoff, *Explanation and Progress in Security Studies*, 2014).

Tang is quite careful in offering definitions for many of the key terms he uses. He discusses bedrock paradigms, foundational paradigms, and the ultimate paradigm, namely, the social evolutionary paradigm. However, he never tells the reader what a paradigm is and how it might differ from a social science theory, or even from a research program. This is particularly surprising given what a notoriously contested term it is with a range of meanings even within Thomas S. Kuhn's (1962) *The Structure of Scientific Revolutions*.

Tang is also usually precise in his use of key terms. But because of the complexity of the argument, when lapses do occur they create problems. For example, he credits Wendt for introducing to IR scholars the question concerning how the system of war arose, but criticizes him for an "almost exclusive reliance on ideational factors to explain the transition from one type of anarchy to another" (p. 54). Tang argues that the international system can only be explained by reference to both material factors and ideational factors, the former of which are ontologically prior. Since Wendt relies "almost" exclusively on ideational factors, there is presumably some reliance on material factors, and Tang fails to offer any argument to the effect that ontological priority entails explanatory dominance. So why cannot Wendt's ideational factors

do more of the explanatory work as long as he includes both ideational and material?

Since Tang regards an endogenous explanatory schema as a key criterion of acceptability (p. 93), he is sharply critical of all explanatory theories that rely on exogenous factors to account for transitions (e.g., p. 54). However, when he explains the transition from an offensive to a defensive realist world in Chapter 3, there seems to be no endogenous basis for the first nation-state in the offensive world that follows a defensive approach; at the right time, the defensive approach will prove more successful than the offensive, and eventually, all, or nearly all, states will follow suit (p. 103).

Evolutionary grand theories do not have much in the way of policy implications because, like evolutionary biology, they do not tell us anything about the future —save that things will at some point change and the world will be different. However, Tang offers several predictions, for example, that there will never be a "harmoniously institutionalized 'world state' or 'world society'" (p. 110). Since the prediction draws both on the current book and on his *A General Theory of Institutional Change* (2014), one might think that the derivation of predictions is justifiable, until he points out that the institutional theory is also "SEP-based" (ibid). Predictions do follow from some systems theories, for example, from A.F.K. Organski's classic power transition chapter in *World Politics* (1958), which, on the grandest scale, predict that when all major states are fully industrialized, there will be far fewer significant regional power transitions and, hence, reduced transition-induced warfare. Since Tang contends that his theory is also a macro-sociology of IR, his claim that no superior theory to the SEP is possible is a prediction, but also runs afoul of the widely accepted Duhem-Quine thesis about the impossibility of final theories in empirical sciences.

Good scholarship on grand questions is thought provoking, and *Social Evolution* is indeed a good one that should receive considerable attention from students of contemporary IR theory. While the argument is wide-ranging and fairly complex, it does not require any specialized technical background. The book is of particular interest to students and scholars interested in theories of international relations.

National Security Through a Cockeyed Lens: How Cognitive Bias Impacts U.S. Foreign Policy. By Steve A. Yetiv. Baltimore: Johns Hopkins University Press, 2013. 168p. $24.95.

— Todd H. Hall, *University of Oxford*

Human beings do not always act in conformance with the expectations of rational decision making. They frequently overestimate the extent to which others understand the motives for their actions. They are more likely to blame their own negative behaviors on situational factors, but view those of others as stemming from character or disposition. They may overly focus on particular values or aspects of situations while neglecting others. They may fit new information to existing beliefs, rendering the latter unfalsifiable. They see patterns where none exist. They can become overconfident. And they can engage in short-term thinking.

This is but a small list of the many deviations that cognitive psychologists and others working in similar areas have observed in experimental settings. Steve A. Yetiv tells us that these are also behind some of the most important choices and outcomes within international relations over the past 50 years. Leaders of the Soviet Union thought that outside actors would view their invasion of Afghanistan as a limited, defensive move, but others—United States decision makers in particular—saw it as offensive and expansionist. The reason? The biases at work in how we view our own versus others' behaviors. President Ronald Reagan permitted the Iran-Contra Affair to occur, putting weapons in the hands of the Iranian government in the hope that it would help free U.S. hostages held in Lebanon. Here, too, a cognitive bias was at work: a tunnel-vision-like focus on the hostages that overrode the consideration of other values. In these cases, as well as others involving Al Qaeda's perceptions of the United States, U.S. planning for the Iraq War, and U.S. energy policy, Yetiv presents a bias (or set of biases) he views as key to explaining the decisions and actions of the parties involved.

The author is writing in a tradition that includes, among others, Robert Jervis's *Perception and Misperception in International Politics* (1976) and Richard Ned Lebow's *Between Peace and War* (1981) in that he examines the ways in which humans—and policymakers in particular—may deviate from the expectations of rationalist models due to cognitive biases. The arguments and findings of Daniel Kahneman, the Nobel-prize-winning author of *Thinking: Fast and Slow* (2011) play a particularly large role in his account. Indeed, those familiar with *Thinking* will likely recognize many of the biases Yetiv outlines. His contribution is not that he has identified new phenomena, but that he has used existing research into cognitive biases to shed light on a range of decisions and behaviors by actors on the international stage. What is more, he also offers suggestions concerning how we can "debias" our decision making.

National Security Through a Cockeyed Lens is not a piece of technical, scholarly work, however. As Yetiv himself writes, the book is "written for a broad audience. . . . It may well be of interest to academics, but it is designed to appeal to students and educated general readers" (p. 6). This choice of target audience means that the book is quite accessible and easily read in one or two sittings, but it also means that its arguments and methodology may raise some questions for an academic reader.