

conclusion, which speculates upon the likelihood that such a line will endure as China becomes more powerful.

Luttwak's publication stands as an intellectual counterpoint to Odgaard's. *The Rise of China vs. the Logic of Security* is grounded by a singular set of propositions about the enduring and constraining structure of great power politics and the strategic logic it produces. In a word, within such dictates the prospects for a peaceful emergence of a new power on the world stage are remote. As a result of such enduring laws, Luttwak has a rather dim view of China's ability to navigate its own rise tranquilly. He is particularly pessimistic about the country's concurrent promotion of economic growth and military strengthening as it cannot help but elicit a strong backlash from other states. Such a development is unlikely to play out well for any of the involved parties. Moreover, the author chides, "If Chinese leaders ignore the warning signs and forge ahead, the paradoxical logic will ensure that instead of accumulating more power, they will remain with less as resistance mounts" (p. 6).

The rest of the book is intended to lend substance to this somber pronouncement. Its first half contains a series of brief excursions into the historical and theoretical roots of China's emerging predicament, while also sketching out what Luttwak views as its main features. More specifically, Chapters 2 through 12 make the case that China is particularly prone to falling into the trap, posed by the logic of strategy, into which so many other rising powers have sunk. The second part of the book then describes the manner in which various international actors have already started to react against China's meteoric rise. Chapters 13 through 19 cycle through Asia and report that indications of counterbalancing are proliferating across the continent. After a detour into Europe in Chapter 20 (which somewhat strangely hones in on Norway's recent contentious relationship with China), Chapter 21 finds evidence of strains within U.S.–China relations.

This book appears intended to provoke debate more than to forward a comprehensive argument about China's rise. As such, it is unrealistic to hold the work up to particularly rigorous methodological and empirical standards. Yet even when viewed more as a conversation starter than as a definitive statement, it is a flawed publication. First, as with Odgaard's, it contains no Chinese language sources, and, unlike her book, makes only scant use of interview data. Second, Luttwak makes little effort to place his observations within the context of the expanding literature by other students of great power politics, such as Aaron Friedberg and John Mearsheimer, and to argue how his contentions relate to their arguments about China. Third, the book is peppered with rather odd usage of concepts borrowed from developmental psychology and applied to great power relations ("great-state autism"; p. 13) and, more specifically, China ("acquired strategic deficiency syndrome"; p. 105), terms that are at best strained

and at worst grossly stretched beyond their original meaning. Finally, and most importantly, the evidence that Luttwak makes use of throughout the book is highly stylized and selective, leaving the impression that he was simply seeking out events that confirmed his deeply held first principles.

In sum, both books are stimulating, but neither is entirely convincing, in no small part because both authors fail to attain the elegant balance found between the interlocking yin and yang forces of the Taiji symbol. In the field of Chinese foreign policy and national security studies, such an equilibrium stems from considering how the theoretical arguments that are derived from the broader international relations and security studies literature can facilitate more accurate descriptions and explanations of Chinese behavior, and from contemplating how such actions may require a modification of these general theories. Over the last two decades, this coupling approach has emerged as a state-of-the-art feature in the study of Chinese foreign relations. Despite its limited failings, Odgaard's book comes much closer to reaching such a standard than Luttwak's, and thus merits more attention. However, to be fair, both publications are quite thought provoking; Luttwak's for forwarding a rather succinct warning about dark clouds on China's horizon, and Odgaard's for revealing just how Beijing has been relatively successful, so far, at holding off the storm that it normally produces.

Votes, Vetoes and the Political Economy of International Trade Agreements. By Edward D. Mansfield and Helen V. Milner. Princeton: Princeton University Press, 2012. 240p. \$60.00 cloth, \$29.95 paper. doi:10.1017/S1537592713001217

— Andreas Dür, *University of Salzburg*

The rapid spread of preferential trade agreements (PTAs) is one of the most interesting phenomena of the contemporary international political economy. Especially since the end of the Cold War, countries across the world have been signing a large number of PTAs, many of which go substantially beyond simple agreements regulating trade in goods. For nearly as long, Edward D. Mansfield and Helen V. Milner have done research on PTAs. In *Votes, Vetoes and the Political Economy of International Trade Agreements*, the culmination of this research program, they make a forceful case for the important role that political institutions play in the political economy of PTAs.

Mansfield and Milner argue that two variables are key in explaining which countries conclude trade agreements: regime type and the number of veto players. For one, democratic governments are more likely to sign PTAs than are autocracies, as they can use such agreements to convince the public and pro-free trade interest groups that the government is pursuing an open trade policy. The argument builds on the assumption that the median voter

is more liberal than the government, but that he or she has limited information about governmental preferences and policies. When the country experiences economic difficulties, the median voter will punish the government, assuming that it implemented trade policies that extract rents, even though an exogenous shock may have brought about the economic downturn. To avoid being voted out of office, democratic governments can sign PTAs that credibly commit them to an open trade policy. Since autocracies are less dependent on the median voter, they do not need PTAs as a commitment device.

The second argument is that the number of veto players is negatively related to the probability of PTA ratification. As the number of veto players increases, protectionist interests find it easier to sway at least one of them and thus block the ratification of a PTA. While governments may be able to buy off individual veto players—for example, by including provisions in a PTA that soften the opposition by protectionist interests—as the number of such players increases, this policy becomes increasingly costly. It is also unlikely that foreign governments will accept the inclusion of ever more provisions that buy off domestic interests.

The authors use four brief case studies in the theoretical chapter to show the plausibility of their argument. Across three empirical chapters, they then carry out several systematic tests of the argument. Controlling for other influences on the probability of a PTA being signed, such as the presence or absence of a hegemon, the distance between two countries, and the size of the participating countries' economies, they find consistent support for their argument. Democracies turn out to be about 55% more likely to enter a trade agreement than are autocracies, and countries with a large number of veto players are 35% less likely to ratify a PTA than are countries with few veto players.

A particular strength of the book is the attempt not only to test the two main hypotheses but also to derive a series of auxiliary hypotheses and test them against the available data. Among these auxiliary hypotheses are 1) that democratic leaders who sign PTAs are in power for longer than those that do not, and 2) that left-wing parties are more likely to sign PTAs than are right-wing parties. These tests add considerable plausibility to the argument that relates regime type and veto players to the signing of PTAs.

Overall, this is a highly compelling book that deserves a wide readership. The authors managed to anticipate and defuse many potential objections to their argument. Moreover, the empirical examination serves as a model of excellent research. Nevertheless, a few questions emerge that merit further research.

First, the authors advance some evidence showing that the public is informed about trade agreements, but much of this evidence remains anecdotal. Clearly, many German voters know about the European Union and many US voters have heard about the North American Free

Trade Agreement. But do German voters also know about the EU's association agreement with Jordan, and do US voters know about the US-Oman free trade agreement? And even if they know, can these agreements with relatively small trading partners serve as a credible commitment that the government is not giving in to rent seeking? The causal story thus may apply to some agreements (namely, those with relatively important trading partners) better than to others. One may even wonder why—given the specific argument advanced in this study—the US government, which can use NAFTA and membership in the World Trade Organization to signal commitment to an open trade policy, bothers about signing additional PTAs at all.

Second, I was left wondering whether the authors capture the effect of democracy or the effect of transition to democracy. The case studies that accompany the discussion on the effect of regime type deal with South Africa's decision to join the South African Development Community and the decision of Argentina and Brazil to create the Common Market of the Southern Cone. All three countries underwent a transition to democracy shortly before taking these decisions. The same applies to the many Central and East European countries that signed a large number of trade agreements just after emerging from dictatorship. It would be interesting to test whether there is anything particular about recent democracies that causes them to sign PTAs.

Third, as the authors readily admit, the empirical test of whether PTAs enhance the longevity of democratic leaders is only suggestive. In fact, the causal mechanism stipulates that PTAs are most helpful for the ability of governments to stay in office during economic downturns. An empirical test of this conditional effect could have offered more clear-cut support for the argument than the finding that democratic leaders that sign PTAs stay in power longer than leaders who do not. Several alternative hypotheses, including the reverse hypothesis that leaders need to control a stable majority in parliament to be able to sign PTAs, are compatible with the evidence offered for this step in the causal story.

Finally, the authors hardly question why many recent PTAs cover much more than tariffs. Many post-NAFTA PTAs contain provisions on intellectual property rights, investments, public procurement, and more. Inasmuch as it seems implausible that the median voter both is informed about all of these provisions and has a preference for more liberalization on all of them, the present argument does not seem able to shed light on this trend.

Mansfield and Milner's book serves as an excellent starting point for future research that will tackle these puzzles. However, it will also be of great interest to political scientists and political economists interested in broader questions of international cooperation. These readers will find many interesting implications of the argument for

additional debates, such as those on the design of international institutions and on the role of power and domestic politics in international relations.

Origins of Political Extremism: Mass Violence in the Twentieth Century and Beyond. By Manus I. Midlarsky. New

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— Siniša Malešević, *University College, Dublin*

Despite recent attempts by such influential authors as Steven Pinker, Pieter Spierenburg, or John Mueller to downplay the importance of organized violence in the twentieth century, there is no doubt that this was by far the bloodiest era in human history. No amount of imaginative and arbitrary use of statistics—such as Pinker's pairing of apples and oranges where, for example, human casualties resulting from 1,800 years of Mideast slave trade are treated in the same way as the six years of unprecedented mass slaughter in World War II—can deny this simple fact. Moreover, unlike the premodern world where individuals were generally killed for where they were (i.e., resistance to religious conversion, occupation, enslavement, or territorial loss), in modernity the tendency is to murder people for who they are—their ethnicity, “race,” religion, class, or ideological orientation. In other words, it is no accident that the proliferation of mass murder historically coincides with the expansion of political extremism. In the modern era, and particularly in the twentieth century, ideology has played a much greater role in the mobilization and legitimization of violent extremism.

In this highly erudite book, Manus Midlarsky attempts to explain the origins of twentieth-century extremist social movements and their pathways toward mass murder. Drawing on up-to-date research in social psychology, political theory, history, political science, and philosophy, the author develops an original theory aimed at tackling the emergence of violent political extremism. Since not all belligerent extremist organizations have ended up committing mass-scale slaughter, Midlarsky's focus is on the key social and historical processes that are likely to tip the extremist groups toward indiscriminate violence. In the author's view, a combination of factors, including initial ephemeral gains, contraction of the space of authority, intense emotional experience of humiliation, shame, and anger, as well as heightened awareness of morality and later territorial loss, creates an explosive cocktail that is likely to lead toward the unmitigated killings of huge numbers of individuals.

More specifically, Midlarsky argues that political extremism often emerges in the aftermath of a temporary, but significant, political victory for a particular social movement. However, once this movement finds itself under substantial external threat, fearing that its victory will be reversed (and in some instances this reversal becomes a reality), the

tendency is to develop a shared perception of injustice that ultimately leads to common feelings of anger, humiliation, and shame. The direct consequence of this process is the movement's attribution of blame and stereotyping of a group deemed responsible for their expected or actual loss. For Midlarsky, the pathway to extremist violence also entails a substantial degree of “mortality salience”; that is, an increased sense of one's mortality, which stimulates popular preference for, and unquestioned loyalty to, a charismatic leadership. For example, the route to extremist violence of the Tamil Tigers in the late twentieth century is traced back to the decline and eventual disappearance of the Jaffna kingdom in 1619; ephemeral gains in civil service and economy achieved by Tamils under British rule; the loss, humiliation, and anger experienced by independent Sri Lanka's language bill (1956), which made Sinhala the only official language; and another ephemeral gain accomplished by the Liberation Tigers of Tamil Eelam's takeover of Jaffna in 1986, followed by the heightened sense of threat that the Sri Lankan Army would capture Jaffna. The ultimate outcome of all these historical processes and events was the advent of violent extremism, resulting in the devastating actions of the LTTE suicide bombers.

Midlarsky has written an impressive book. His central argument is well articulated, carefully elaborated, and tested on a variety of examples from Nazi Germany, fascist Italy, Stalinist Soviet Union, radical Islamist groups, Sri Lanka, Poland, the Balkans, Japan, and Turkey, among others. The book is also well written and based on comprehensive research.

Nevertheless, as with all well-ordered theoretical models, Midlarsky's theory cannot capture all of the complexity and messiness of social life. Some of the case studies analyzed in the book, such as that of a tiny and politically insignificant British extremist group, Al-Muhajiroun, and Croatian nationalism before World War II, seem highly overstretched to fit this theoretical model. Several key concepts utilized are a bit too vague or undefined (i.e., radicalism, extremism, or democracy). For example, it is not clear what parameters are used to distinguish extreme from the less extreme forms of nationalism.

More importantly, what is missing in Midlarsky's study is an analysis of the sociological processes involved. While the author is good at linking micropsychological research with broader macrohistorical transformations, there is not much attempt to engage with the subtleties of the mezzosociological world. This is most pronounced in the author's focus on elite behavior; his treatment of nations, ethnic groups, and societies as homogenous entities; and his lack of engagement with the social mechanisms that underpin ideological transformations. While there is little dispute that all human beings share some universal psychological propensities, the complexities of collective action cannot be captured well by the methods of clinical psychology. Simply put, large-scale collectivities such as nations, ethnic