understood through the lens of a joint cyber–IR analytical framework (as provided in this book). Second, the role of the state needs to be newly envisaged as private actors challenge many of its traditional key prerogatives. Third, the ability to control cyberspace is going to shape its future; the authors contend that we need to better understand the contestation between different actors in this space. Fourth, more effort is needed to comprehend the different dimensions of cyber-in-security and its influence on international relations. Fifth, we need to understand the interaction between cyberspace and IR in an even broader context of the natural world we live in.

International Relations in the Cyber Age is a book rich in content that opens up multiple pathways for further investigation. It is written in an approachable manner, avoiding unnecessary jargon or heavy theorizing, and its many examples illustrate the authors' thinking in useful ways. A few of the structural choices are a little puzzling, however, and the overall storyline and main argument are at times elusive. This criticism mainly applies to the chapters that do not directly add to the reader's understanding of the framework or that are, at best, loosely connected to the application of the proposed method. For example, the book mentions sustainability a few times and the need to rethink the cyber–IR interaction in the broader context of the natural environment (see, for example, chapter 5). However, this point is lost in the second part of the book. The "lateral pressure theory," mentioned in chapter 3 and chapter 5, is another interesting theoretical avenue but is not integrated well with the layer framework and is also not followed through systematically to the end. Furthermore, the book would have profited from a more careful discussion of key concepts, such as power and control in its different manifestations, for which there is a wealth of relevant literature.

Indeed, the biggest issue is that the book does not engage with the vibrant new literature on cybersecurity, cyber conflict, and cyber governance that has emerged over the last five years. The field is not only very complex and dynamic, as the authors repeatedly stress, but scholarly knowledge that takes into account more recent empirics has also evolved rapidly in the last few years. For example, there is only one very short subchapter specially dedicated to literature in the whole book (chapter 3, section 7, which only reviews literature up to 2010). In the chapter on cyber security and cyber conflict (chapter 8) the referencing is very thin, even though this is the area where several substantial and important contributions have been made, especially by US scholars. Furthermore, in the chapter on governance (chapter 9), there is not a single mention of the UNGGE process (United Nations Group of Governmental Experts on Developments in the Field of Information and Telecommunications in the Context of International Security), even though that is arguably the most important international attempt to come to an agreement on cyber "norms of behavior." The consideration of other key events, including the Russian interference in the US elections in 2016 or the knowledge gained after 2013 about the domestic surveillance activities of intelligence organizations, including the NSA, should not be missing from a book published in 2018. Given that they are, the book is outdated in several of its statements and assumptions.

Nevertheless, the book is commendable for how it manages to bring different disciplinary perspectives together and for its bravely detailed discussion of the sociotechnical foundations of the cyberspace system. It contains interesting thoughts on how to move forward in an interdisciplinary manner so that neither technological imperatives nor political considerations are lost. Indeed, as the authors stress in their conclusion, the complex interaction of technology and sociopolitical systems needs innovative theoretical approaches that help explain altered realities.

War and Chance: Assessing Uncertainty in International Politics. By Jeffrey A. Friedman. New York: Oxford University Press, 2019. 240p. $34.95 cloth.
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International relations scholars and foreign policy analysts typically claim to be interested in prescriptive theorizing, but this usually takes the form of a perfunctory concluding chapter about how policy makers should behave if their theoretical premises are right, which of course they are. The normative implications of academic work are generally a tacked-on epilogue. We are usually much more interested in explanation than prescription. Not so with Jeffrey Friedman's excellent book *War and Chance: Assessing Uncertainty in International Politics*, the best book on improving decision making through rigorous empirical analysis since Philip Tetlock's landmark *Expert Political Judgment: How Good Is It? How Can We Know?* (2005).

Friedman is particularly interested in the role that probability assessment plays in foreign policy judgment. In an extensive analysis of the explicit guidelines for assessing uncertainty provided by the US military, intelligence services, and foreign policy agencies, he finds an aversion to offering fine-grained probabilistic judgments. A qualitative and quantitative review of decision making during the first decade of US involvement in Vietnam illuminates a number of common pathologies. Elites rely on relative rather than absolute probability, asking whether certain options offer better chances of success than others, rather than estimating an absolute number. Or they engage in what Friedman calls "conditioning," making the case for instance that success requires a particular action but not actually estimating the likelihood of success given that action. Both have the common weakness of asking whether success is actually worth it at all, given the costs. Elites might think they are carefully estimating probability, but actually they are avoiding estimating it. Collecting and analyzing all references to probability in US documents concerning the Vietnam War, Friedman reveals something dramatic: more than 90% of assessments of strategic options make this mistake. Political elites consistently avoid precise judgments about the overall likelihood of success, with (literally) fatal consequences. The policy most likely to yield success is that which usually has the most costs, and those should be taken into account, but often they are not.

In this way, Friedman's book fits squarely in the tradition of psychological research on foreign policy making, which is united by a pessimistic assessment of the quality of judgment exercised by our leaders. However, Friedman does not leave it there, as so many scholars do. He rejects the arguments of those he calls *agnostics,* who claim that probability judgments are too subjective to be of any use, and *rejectionists*, who go so far as to argue that these judgments are counterproductive and harmful to foreign policy interests. He makes two compelling points. First, in a policy-making domain where uncertainty is always present, elites must nevertheless make decisions. They might not like uncertainty, but they cannot avoid it. So much psychologically inspired research leaves us hanging. People are terrible decision makers, yes. But knowing that, what are we supposed to do? Most research is content to simply score the theoretical points against mainstream theories by showing that human beings demonstrate systematic departures from rationality. In full disclosure, I would even count myself in this group.

Second, Friedman argues that this skepticism might be misplaced, because rarely do scholars actually test whether probabilistic judgment fails more in comparison to nonprobabilistic judgment and whether the former can be improved. This second point receives most of the attention in the book. Friedman is much more optimistic about the prospects for probabilistic judgment than the conventional wisdom, if elites would just give it a try. The book is equal parts criticism and pep talk. As he writes, "The worse our performance in this area becomes, the more priority we should place on preserving and exploiting whatever insight we usually possess" (p. 10).

Friedman first takes on the agnostics who argue that probability judgments are essentially useless because they can vary so wildly, drawing on research he undertakes using surveys of national security experts at the Naval War College, as well as forecasters involved in the Good Judgment Project organized by Tetlock and others. The data arising out of the forecasting tournament organized by the latter are the most compelling. By asking thousands of forecasters to make predictions about foreign policy events, it becomes possible to assess their quality of judgment. By asking them to also specify probabilities, it becomes possible to estimate the gain in judgment accuracy when compared to coarser estimates of the kind typically made in the foreign policy establishment, such as whether an outcome is very likely or highly improbable; that is, what the agnostics think is the most that we can expect from foreign policy makers. Friedman does this by rounding up or down probabilities offered by forecasters into broader "bins" corresponding to the number of categories typically found in guidelines for uncertainty judgment seen in the foreign policy establishment and then testing the overall "Brier" scores—an established measure of good judgment—for the coarser and more precise judgments over the course of all predictions. In other words, Friedman estimates how much they gain by being more precise, with the conventional wisdom being not very much.

The results show that it always pay to try to be more precise, with substantial increases in judgmental accuracy. Foreign policy makers may not want to make these probabilistic judgments, but they do better when they do. What agnostics might dismiss as gut decisions based on the arbitrary assignment of numbers to some underlying intuition actually appear to be much more than that. Moreover, a finer-grained analysis shows that variation in forecasting prowess is not a function of those factors that cannot be refined by foreign policy agencies, such as numerical fluency or educational attainment. Better forecasting is not a dispositional trait that is unamenable to training, effort and experience.

Of course, foreign policy judgment does not occur in a laboratory or online tournament but in a political environment where getting things wrong can come with intense political punishment. This creates incentives for those actually making these decisions for a living to avoid this very precision, using vague terms to protect themselves if their estimates turn out to be wrong. This creates the possibility for what Friedman calls *elastic redefinition.* Or foreign policy makers can overstate the possibilities of worst-case scenarios in what he labels *strategic caution.*

Friedman does not deny that foreign policy makers are punished for foreign policy failures. However, in a review of all of the major intelligence failures of the post–World War II era, he finds that using strategic caution or elastic redefinition did not preempt criticism. Noting that there has actually never been an empirical test of whether the public chastises intelligence officers more for specifying numeric probabilities as opposed to offering vaguer qualitative assessments, he undertakes a survey experiment and finds that actually publics are more forgiving when foreign policy analysts make more precise estimates and they turn out to be wrong.

In making the case for more rational decision making, Friedman provides a useful counterweight to the often overly pessimistic psychological literature, which has become the conventional wisdom in the scholarship on judgment. Given that there are no greater costs to making precise estimates and there are significant benefits, Friedman's book ends with an admonition to rely more on explicit probability judgments. In essence, we are more capable than we give ourselves credit for, if we would give chance a chance. This is the best type of prescription, that which is based on exemplary research.

Reinventing Regional Security Institutions in Asia and Africa: Power Shifts, Ideas, and Institutional Change. By Kei Koga. London: Routledge, 2017. 226p. $125.00 cloth, $39.96 paper.
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Reinventing Regional Security Institutions in Asia and Africa examines the causes of and processes within regional security institutions (RSIs). The study deploys process-tracing and historical institutionalism throughout, relying on a rich mix of primary and secondary sources collected through interviews, archival research, government reports, and thousands of documents obtained through fieldwork in Ethiopia, Indonesia, Nigeria, and Singapore. It is enriched by ambitious and robust fieldwork examinations spanning two continents (Asia and Africa) with two very different regional institutions in Africa (Economic Community of West African States [ECOWAS] and the Organization of African Unity [OAU]) and the Association of Southeast Asian Nations (ASEAN) in Asia.

The second chapter of the book outlines the theoretical framework adopted to analyze institutional change. To answer the research question of "why and how regional security institutions (RSIs) undertake change," Kei Koga draws on agent-centered historical institutionalism in comparative politics, focusing on critical junctures, path dependence, and lock-in effects. The author identifies two processes for RSI institutional change: a macrolevel process triggered by external actors and microlevel change triggered by member states' expectations and assessments of the RSI's security utility (p. 26). Thus, the process of institutional change follows a two-step approach: (1) expected or actual changes in the regional distribution of power creates the conditions for an RSI's institutional change, and (2) member states reassess their RSI's security utility. In the face of actual or expected changes in the distribution of power, the outcome of the reassessment determines the type of institutional change: consolidation, displacement, or layering. RSI member countries interact with their regional institutions, making calculations about expected/actual change in the distribution of power and reacting according to their evaluations of their institutions. Member states will consolidate the current status if the evaluation is positive, will displace it if the outcome is negative, and will layer it if there is a high level of uncertainly among the members. This theoretical framework is then applied to three empirical cases.

Chapter 3 traces and explains two periods of institutional change within ASEAN: 1968–76 and 1988–97. The author finds that ASEAN endured many institutional changes and adaptations during the two time periods examined. Some of these changes took place because of external forces such as the United Kingdom's dissolution of the Anglo-Malaysian Defense Agreement (AMDA), US-China rapprochement, and the United States' Vietnamization policy.

Chapter 4 moves from Asia to West Africa with an examination of ECOWAS. The author considers two time periods here as well: 1976–81, marked by the creation of the Protocol of Mutual Assistance and Defense (PMAD), and 1989–99, marked by layering and consolidation (p. 118). Koga finds that institutional change in this case study came about through a combination of external triggers and member state expectations. The chapter makes two related arguments about institutional change within ECOWAS: "expected not actual change in the regional distribution of power triggered ECOWAS's institutional change" and "ECOWAS member states' divided expectations regarding the institution's security did not prevent institutional change" (p. 120). Where there is a high level of uncertainly from member states, the author finds that layering occurs.

Chapter 5 considers the OAU by examining two periods of institutional change. The first one (1979–82) was marked by little security-related institutional change because of financial hardship (with the exception of a peacekeeping mission in Chad that the author argues was conducted to keep foreign powers out). The second period (1990–2002) was marked by institutional layering. The author argues that OAU's institutional security preference (ISP) was founded on the non-interference principle. Koga argues that non-interference facilitated decolonization efforts, but hindered the objective of creating an African conflict-resolution mechanism (p. 163). Chapters 6 and 7 round out the analysis and summarize the book's findings.

The in-depth analysis and rich empirical material used for the analysis make the book very interesting. The author takes issue with international relations theory (IRT) for several perceived limitations in the way it engages or studies institutions. The limitations of IRT, according to the author, are that it (1) treats all institutions monolithically, focusing primarily on their utility; (2) assumes that the functionality of international institutions is a given, instead of digging deeper into the processes of how norms come about; (3) fails to focus on RSIs created by non-great powers (i.e., IRT has a Western bias in its study of