

their status, and others to think of one that might reduce it. He also measured their scores on social dominance orientation (SDO), involving their approval of social hierarchy. When they competed in a game with escalation as an option, those with a status threat fresh in their minds were more aggressive, as were those with higher SDO scores. A second study used a subject pool that included military officers and security officials, a population that in the future might be making international affairs decisions, and looked at the relation between their personal histories of holding powerful positions and their readiness to escalate. Experience with power seemed to reduce their responsiveness to status concerns.

Renshon then defines a scale for international status. In the mid-1960s, J. David Singer and Melvin Small measured it by counting the diplomatic representatives a state received, giving more weight to ones with higher titles (“The Composition and Status Ordering of the International System, 1815–1940,” *World Politics*, 18(2), 1966). Renshon uses their data but puts more weight on diplomats who come from states that themselves receive more diplomats. Recognition by a higher-status country raises one’s score. He validates his scale by considering just diplomats sent by the United States and calculating the correlations of status score with mentions of the country in the *New York Times* and visits by the secretary of state and the president. With further data on power resources and on international violence from 1816 to 2005, he finds, for example, that five years after a state initiates and wins a war, its expected status goes up 6.7 places more than one that did not initiate a war.

I believe that status is an interesting explanation only insofar as it is different from power. Separating the two is hard given Renshon’s view, which I share, that status concerns trigger conflict for strategic reasons, not just through jealousy or anger. It is important to base our operational measures and hypotheses directly on our definitions, and it is unclear how sending diplomatic representatives shows the recipient’s status rather than its power, as he defines these concepts. A country setting up a foreign embassy wants to communicate with and persuade the recipient; this motive seems to reflect the latter’s power. Also, how do the author’s validating variables—newspaper mentions and high-level visits—reflect status rather than power? I find his recursive conception of status innovative and plausible, but again it is important to show why the modification follows from his definition. The measure’s results do not seem compelling; the top-five status countries in 1817 were #1 Bavaria (#13), #2 France (#2), #3 Saxony (#18), #4 Baden (#19), #5 Austria (#4). Singer and Small’s rankings around that year are in parentheses. The orderings disagree significantly, and both put France at #2 right after it lost a catastrophic war.

In my view, status differs from power in more ways than Renshon’s definition suggests, and these might be exploited for empirical tests. For one thing, it has a normative component. The group members generally feel that they really *ought to* follow the pattern of deference. The normative sense is strong in sociological and psychological treatments, including the questions used for subjects’ SDO scores.

Also, unlike power, a party’s status is not based only on its objective characteristics or on others’ assessments of them. A status ranking is an equilibrium; it is self-referential in that members follow it in their deference because they commonly expect others to follow it. The choice among possible equilibria may be set by apparently arbitrary events, such as one’s historical status ranking. States constantly try to get their way in symbolic matters that are of low innate importance. Symbolic conflicts may reflect the winner’s desire to prevail but not its objective capability. Renshon (p. 130) gives examples of angry battles over precedence among diplomats. His hypothesis—that countries enter conflicts to demonstrate their capabilities—does not tap status’s contrast with power. The equilibrium nature of status explains its connection to higher-order beliefs. The author admirably uses a variety of methods, but for this issue, I would propose one more, game-theoretical analysis. Our social intuition and behavior can handle higher-order beliefs, but they are confusing to talk about in natural language, and so we should use the formal system that focuses on them.

Interest in status-related explanations in international relations has grown (see Allan Dafoe et al., “Reputation and Status as Motives for War,” *Annual Review of Political Science*, 17, 2014). The new approach should clarify not just military moves but also diplomatic interactions of many types, and will connect international relations with sociology and psychology. Renshon has moved the discussion forward, partly by what he does and partly by the standard he sets for others. As well as his introduction of network methods to identify status communities, the group of states that each one uses for comparison, I would point to his care in defending his hypothesis against other explanations. Renshon has been prominent among those arguing that status-related variables are important, and *Fighting for Status* will surely be central.

Spy Watching: Intelligence Accountability in the United States. By Loch K. Johnson. New York: Oxford University Press, 2018. 632p. \$34.95 cloth.
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— Joshua Rovner, *American University*

Loch K. Johnson has been studying oversight since the 1970s, when he served as the special assistant to Senator Frank Church (D-ID) on the committee investigating the intelligence community over a range of alleged misdeeds.

For decades he has wrestled with the problem of holding secret agencies publicly accountable. Government transparency must be the default position in a democracy, but intelligence agencies require secrecy in order to function effectively. Balancing accountability and effectiveness is difficult for any country with a dedicated intelligence bureaucracy. In *Spy Watching*, Johnson evaluates how well the United States has struck the balance.

The book is ambitious. It includes a theoretical overview of accountability, along with a sweeping history of U.S. intelligence oversight. It covers a number of issues, including intelligence collection, analysis, covert action, and counterintelligence. Johnson sprinkles the narrative with his own experiences and observations, along with interviews with intelligence officials. And he pulls no punches in his summary judgment. While oversight has improved since the creation of the Senate and House intelligence committees in the 1970s, members of Congress have often failed to investigate the intelligence community with the same vigor as Church's, and they have been far too quiescent over the last two decades, as the intelligence community embarked on "the era of mass surveillance" (p. 171).

The theoretical heart of the book is in Chapters 6–8, where Johnson discusses the "shock theory" of intelligence oversight. Oversight usually resembles what Matthew McCubbins and Thomas Schwartz call the "police-patrolling" style of oversight. But major intelligence failures or scandals shock them into a burst of "firefighting." They go beyond routine monitoring and engage in serious and critical examination of what went wrong, with the hopes that they can reform the community and prevent future breakdowns. What follows is a period of intense patrolling, which becomes less intense over time, until another shock forces their attention. The cycle repeats.

Johnson argues that the media are essential for stimulating oversight. Some threshold of sustained media attention is necessary to spur overseers into action. That threshold is straightforward: "fifty articles, all relatively concentrated in time" (p. 271). The expanding media environment begs the question of whether this number is still meaningful, however. Johnson's standard may have been appropriate when there were just a handful of national newspapers, before the advent of cable news and social media. What counts as sustained media attention today is unclear, given the extraordinary scope and pace and volume of news.

Crossing the media threshold is necessary but not sufficient to stimulate a thorough congressional investigation. Johnson suggests that the timing and nature of the scandal also matter. If the issue is too narrow, for instance, it is unlikely to generate sustained attention on Capitol Hill. He analyzes the difference between scandals by way of comparative historical vignettes, but the book would benefit from more theoretical treatment of these

variables, which seem to be the critical mechanisms that transform media coverage to congressional scrutiny.

Above all, the personality of overseers determines the strength of oversight. Johnson provides a simple typology. "Ostriches" (Chapter 8) pay little attention to their role, treating intelligence agencies with benign neglect. "Cheerleaders" are aggressive advocates of the intelligence community. "Lemon-suckers" see the profession of intelligence as suspect, and are reliable cynics about intelligence activities. They take oversight seriously but go in with the assumption that intelligence agencies are up to no good. "Guardians" believe intelligence is vital for national security, but also that intelligence agencies are capable of misdeeds and poor performance if not held accountable. Johnson holds Church up as the guardian exemplar, and urges more members of Congress to live up to his standard.

Other scholars have described why they do not. Brent Durbin's recent *The CIA and the Politics of US Intelligence Reform* (2017) explains why inertia prevails in the intelligence community. Legislators lack the time, expertise, and political incentives to focus on the complex work of intelligence oversight. Amy Zegart and Julie Quinn have similarly argued that intelligence committee assignments are unappealing for ambitious lawmakers. True guardians are rare for all of these reasons (see Amy Zegart and Julie Quinn, "Congressional Intelligence Oversight: The Electoral Disconnection," *Intelligence and National Security*, 25[6], 2010). Johnson alludes to this work but argues that less prominent members of Congress and committee staffers are at least as important for ensuring dedicated oversight. Particularly ambitious members "may be exactly the wrong people to count on for attentive oversight, because they are too busy in chamberwide leadership roles or off running for higher office" (p. 35). Perhaps, but this is puzzling, given that no one was more ambitious than Senator Church, whom Johnson holds up as the ideal.

Johnson's evaluation of oversight is uneven. His basic finding is that the situation has improved since the high-profile congressional inquiries and reforms of the 1970s. But those changes failed to prevent the litany of subsequent scandals he describes, starting with the Iran-Contra affair in the 1980s. Moreover, Johnson is at times scathing in his condemnation of congressional oversight after the September 11 attacks, especially on issues regarding domestic surveillance. As he tells it, Congress passed the PATRIOT Act without reading it closely. The George W. Bush administration then expanded domestic collection, surreptitiously, but Congress did little to stop it even after it got wise. The Senate intelligence committee's investigation into alleged CIA torture was a rare exception to the rule.

One reason why Johnson's conclusion seems inconsistent with the evidence he presents has to do with how he measures the quality of oversight. If the focus is on process, then the case is straightforward. Clearly, there have been

many innovations since the 1970s that have enabled Congress to take a closer look at intelligence activities. Perhaps most important is the creation of full-time professional committee staffers who have the time and mandate to master bureaucratic and budgetary arcana. Other laws have codified Congress's role, to the chagrin of some presidents who would like a freer hand to use intelligence as a foreign policy instrument. It is tempting to focus on process, because we can describe these new laws and regulations in detail.

If we measure the quality of oversight according to outcomes rather than process, however, the picture is murkier. Johnson's discussion of factors like media attention and personality suggests that process alone does not tell the whole story. The combination of factors that enable oversight to work—that is, to actually constrain the behavior of intelligence agencies—are formal and informal. Some factors are related to internal processes and some are external both to Congress and the intelligence community. How these come together to influence intelligence judgments, and stop intelligence agencies from misbehavior, remains mysterious. Nonetheless it is possible that oversight succeeds, even if we do not quite know how and why.

Oversight works when nothing happens. Policymakers and intelligence officials exercise restraint, either because they fear congressional scrutiny, or they fear a public scandal or something else. Truly successful oversight narrows the range of actions they might consider in the first place. In this sense, measuring oversight raises the same knotty methodological problems as measuring deterrence. Telling a convincing causal story about a non-event is inherently difficult, even if the logic is sound. The expanding field of intelligence studies, and the increasing integration of intelligence studies with mainstream political science, makes it likely that enterprising scholars will tackle this problem directly. *Spy Watching* will prove a valuable resource for their efforts.

The Despot's Guide to Wealth Management: On the International Campaign against Grand Corruption. By J. C. Sharman. Ithaca: Cornell University Press, 2017. 274p. \$29.95 cloth. doi:10.1017/S1537592718001755

— Mlada Bukovansky, *Smith College*

I feel slightly envious of Jason Sharman. He must have learned a thing or two about fine wine, well-cut suits, and exotic cars while swimming in the shark pool of bankers, politicians, lawyers, investigators, advocates, and regulators involved in either enabling or combating (and sometimes both) the money-laundering industry that services the kleptocrats of this world. Nobody sees fit to mention it, but I suspect there is a pleasurable side to corruption. At least that seems true of the "grand corruption" that is the focus of *The Despot's Guide to*

Wealth Management. The mansions! The yachts! The shoes! The thrill of beating the system! But that pleasure is surely bought at the expense of a great deal of misery. The focus of Sharman's study is not on the misery and on the costs of corruption to those he calls "victim countries," but rather on the facilitators, those in the wealthiest countries that serve as the centers of global finance: the United States, Switzerland, the United Kingdom, and Australia (the latter may seem out of place in this lineup but has a methodologically important purpose, and offers up more than its share of relevant scandal). Rather than doing his research in hardscrabble places, I imagine that the author spent a good deal of time in tall shiny office buildings and probably some fine restaurants as well, in places such as Manhattan, London, Bern, and Sydney.

Envy aside, Sharman is right to draw our attention to the wealthy, for in the academic study of corruption, the scholarly attention paid to poor governance in the economic periphery seems to far outweigh the attention paid to poor governance in the core. But it is the core that takes in a good chunk of the proceeds of grand corruption and allows its practitioners to launder them in secret (and sometimes not so secret) bank accounts, shell companies, trusts, and that eye-popping high-end real estate that has inflated the markets in the world's great cities and made living there difficult or impossible for those of ordinary means to afford. The study of corruption needs to be about more than improving governance in those countries, inevitably of the "developing world," that routinely rank lowest on Transparency International's Corruption Perceptions Index. Although legal regimes have been evolving to deal with the "supply side" by targeting bribe payers, thus putting pressure on wealthy countries to constrain their corporations from offering bribes to foreign public officials, much remains to be done to address the facilitating role of finance and banking, real estate, law, and law enforcement if societies are to get serious about addressing corruption as the globalized phenomenon it has clearly become.

Sharman brings us up to date on developments relevant to the 2003 United Nations Convention Against Corruption (UNCAC) and its articulation of "an international anti-kleptocracy norm" (p. 47). Kleptocracy literally means "rule of thieves." The author aims to explain the emergence of the anti-kleptocracy norm, evaluate its effectiveness, and suggest ways in which its effectiveness might be improved. Summary does not do the argument justice, but in a nutshell: Sharman argues that the norm emerged more as a result of structural changes in the international system than as a result of the advocacy of norm entrepreneurs, and that there is a big gap between the anti-kleptocracy norm and actual practice. The end of the Cold War removed the propensity of Western governments to turn a blind eye to kleptocrats pillaging the public purse as long as they remained