

## Book Reviews

Stephen D. Krasner, *Sovereignty: Organized Hypocrisy*, Princeton: Princeton University Press, 1999.

The problem of sovereignty has returned to the study of politics and international relations recently. It was a major subject of inquiry and debate in the early years of American political science, dominating many of the periodicals in the field in the early twentieth century. Under the influence of the scientific approach to politics that began just before World War II, analysis of important concepts such as sovereignty passed on to philosophers and historians, where it remained safely out of sight to students of international relations. Despite more recent analyses of sovereignty by Bertrand de Jouvenel, W. J. Stankiewicz, F. H. Hinsley, and Alan James (none of whom is American), few American international relations scholars have bothered to read them. This is regrettable because a renewed interest in sovereignty arising from the consequences of globalization makes the concept critical to any study of the state and its predicament in an interdependent world.

Stephen Krasner carefully delineates four meanings of the term sovereignty as it has been employed in the international relations and law literatures. These refer to domestic sovereignty (exclusive legal authority within a territorial realm), interdependence sovereignty (a state's capacity to control and regulate transnational flows), international legal sovereignty that deals with recognition of states, and what Krasner calls 'Westphalian Sovereignty' or the 'Westphalian model' which he defines as 'territoriality and the exclusion of external actors from domestic authority structures' (p. 20). Krasner's analysis concentrates on the latter two meanings, and particularly on the Westphalian model.

Aside from the distinctions between these four meanings of the term, Krasner delves no further into conceptual analysis. His purpose, rather, is more broadly to explore the overall role and influence of norms in international relations. Starting with his published doctoral dissertation, 'Defending the National Interest' (1978), Krasner has consistently argued that interests and power rather than norms drive foreign policies. This work demonstrates through comparative case studies how and why states frequently violate the norms of sovereignty, both in their Westphalian and international legal versions, when it is in their interest. State behaviour, Krasner argues, is dictated either by a 'logic of consequences' (calculations of interests) or a 'logic of appropriateness', meaning that values and norms guide ultimate policy

choices. In the case of international politics, the logic of consequences predominates, particularly when there are major power asymmetries between actors. How do we know this?

Krasner argues that the efficacy and role of norms in international politics can be measured by their degree of durability and institutionalization. If a norm lasts a long time, whether or not states comply with it, it is durable. If behaviour conforms reasonably consistently to the requirements of a norm, then it is institutionalized. The major theoretical schools of international relations, as Krasner shows in a very effective chapter, disagree fundamentally on issues of durability, institutionalization, and conformity of behaviour to norms. Realists, for example, argue that norms are neither durable nor institutionalized, and thus have no serious consequences on state behaviour. This is the classical view of the consequences of anarchy. In contrast, sociological approaches, including the English school and many social constructivists, insist that norms 'socialize' and effectively constrain or even determine behaviour. All policy is embedded in normative structures that include both values (desired objectives) and norms (the way these objectives are pursued). In the sociological view, the central guide to policy is not interest or power, but what is the right normative principle in a given situation. Where behaviour is consistent with norms over a long time (durability), Krasner claims the norm may become 'embedded'. Organized hypocrisy, in contrast, is the situation where a norm is durable, but where behaviour frequently violates it. The behaviour of states concerning diplomatic recognition and interference in the authority.

Krasner is among the few theorists of international relations who takes history seriously. He has amassed a great number of historical cases to demonstrate that 'the basic principle of Westphalian sovereignty, the autonomy of domestic structures, has frequently been compromised through intervention in the form of coercion or imposition by more powerful states, or through contracts or conventions that have involved invitations for external actors to influence domestic authority structures. While the basic principle of international legal sovereignty, the extension of recognition to juridically autonomous territorial entities, has been more widely honored, it too has been violated' (p. 220). Krasner's supporting cases involve issues of minority rights, international lending, human rights, recognition policies, and government structures.

Many of the cases are telling. They include the great powers' insistence that the new Balkan states arising from the wreck of the Ottoman Empire in the nineteenth century guarantee rights to their minorities as a condition for diplomatic recognition. They dictated similar concessions to the successor states of the Russian and Austro-Hungarian empires in 1919, and continued to do so in the 1995 Dayton Accords that ended the Bosnia war. In the case of international lending, Krasner shows how the Concert powers set up a government in Greece upon recognizing it as an independent state in 1832, and created a virtual second government, made up of their own officials, to assure that Greek finances remained in good order. The practices of imposing financial supervision on states continued through the era of gunboat diplomacy when, for example, American authorities virtually took over Cuba, Haiti, and some of the Central American republics to make certain that they paid their international debts. Today, the World Bank and International Monetary Fund not only extend loans to the needy, but also attach sovereignty-violating political conditions to them. The role of the IMF in causing the demise of the Suharto government in Indonesia is a particularly telling recent example.

In the case of government structures, the major powers have frequently imposed

governments and/or constitutions on states, particularly where there were great power disparities. Starting in 1832, they not only wrote the constitution for Greece, but also chose the country's king and prime minister. In the early twentieth century, Americans frequently selected the personnel to govern supposedly sovereign countries, such as Cuba, Haiti, and Nicaragua. They also maintained troops in them, just to make certain that the local personnel governed according to American interests. After World War II, the Soviets imposed communist regimes in Eastern Europe and in many cases not only selected the personnel to run them, but also appointed Soviet communist party and military officials to oversee the national authorities. To help establish the authority of his main theoretical arguments, Krasner also investigates the processes of de-colonization. He points out that because the colonial powers had few vital interests at stake and certainly not the power to maintain hold of the colonies, they did not violate the Westphalian model or norm. In the Cold War's main arena in Europe, in contrast, both the Soviets and the Americans, often supported by the British and/or French, made certain that their respective allies had 'friendly' governments. American financial and other forms of intervention in the critical 1949 Italian elections are just one notable example. Here we have the same great powers involved, but the outcomes in international relations differ. In the case of de-colonization few attempts to impose regimes are evident; in the Cold War arena, the great powers constantly interfered in the internal affairs of their allies.

Violations of the international legal norm of sovereignty may not be as ubiquitous as those of the Westphalian model, but there are so many deviations from the requirement that states recognize only fully independent, territorial entities, that it is also an example or organized hypocrisy. Krasner cites the examples of the international recognition of the Knights of Malta as a state, the admission of India to the League of Nations long before it became independent, Hong Kong's membership in the World Trade Organization, and Andorra's recent admission to the United Nations.

Krasner provides a mass of detail in various issue areas to support his theoretical position. The frequent violations of sovereignty seriously challenge constructivist, liberal institutionalist, and English school positions about the importance of norms in international relations. There is also throughout the book an important subtext that challenges many of the post-Cold war assertions that we are seeing a withering of the state, that human rights norms are replacing or compromising older sovereignty norms, and that globalization is a new phenomenon that is somehow transforming the world. Krasner offers compelling arguments and much relevant evidence to challenge these assertions. The thesis is clearly drawn, the case materials are well chosen, and history is put to good comparative use. Nevertheless, there are three serious theoretical problems. They concern Krasner's treatment of the concept of sovereignty, the problem of consent, and the question of standards.

Krasner joins the company of many recent international relations analysts, such as James Rosenau, the late Susan Strange, Yale Ferguson, and Richard Mansbach, in failing to make the fundamental distinction between influence and authority.<sup>1</sup> They conflate the two concepts, which results in fundamental analytical errors. Influence and power concern capabilities and outcomes. They are ubiquitous in international relations. In contrast, authority concerns rights, obligations, and status. Krasner's Westphalian model or norm of sovereignty is based

<sup>1</sup> Krasner's error is puzzling because in some of his earlier work he acknowledges the importance of the distinction.

on notions of influence rather than authority. His model specifies that governments should not attempt to *influence* the domestic political structures and personnel of other states. This sets impossibly high standards, and is not consistent with the content of the Westphalian peace. The treaties of Osnabrück and Münster do not mention the term sovereignty. But they did proscribe the *right* of the Holy Roman Emperor and the pope to exercise *authority* within the various realms of the developing European states. Article 64, for example, shows that the treaties were not so much creating new ideas like sovereignty, as *restoring* ‘ancient rights, prerogatives, liberties, privileges [and] free exercise of territorial rights’. Pope Innocent XI condemned the treaties as ‘null, void, unjust, damnable, reprobate [and] inane’ not because they diminished papal influence, but because they took away all vestiges of papal and imperial authority, that is, the *right* to prescribe laws and regulations – including the right to issue decrees on purely secular matters – within the realms of the princes. If we accept Krasner’s definition of the Westphalian model as precluding even influence attempts (pp. 20, 29, 33, 53, 73, 106), sovereignty would be less a norm than a myth. More of this below.

The second problem concerns consent. By definition, a sovereign has the right to accept constraints; otherwise it would not be a sovereign. But Krasner includes any constraint on autonomy (not authority) as a violation of the Westphalian model, even those achieved through conventions and contracts, that is, through consent. If constraints are accepted voluntarily, can they be deemed a violation of the Westphalian model? When France signed the Genocide Convention and thus obligated itself not to kill all French Jews and Muslims, did it violate its sovereignty? When the United States signed ILO-sponsored treaties that established various labour standards within the United States, did it violate its sovereignty? When developing countries accept loans from the IMF and in turn agree to put into office a new finance minister, are they violating their sovereignty? There is no such thing as a purely autonomous state, and certainly Westphalia did not construct one. By confusing autonomy with sovereignty and authority, Krasner sets an impossible standard. Governments constantly bind themselves through treaties and other forms of promise that limit their autonomy. But this is not the same thing as granting a foreign power the *right* to rule within one’s own territory, or applying its laws on foreign territory (extraterritoriality). These are clear violations of sovereignty. They involve authority, not autonomy.

Moreover, as Krasner acknowledges in some of the cases involving supposed violations of sovereignty, governments formally granted rights to minorities under great power coercion, but after obtaining recognition they quickly broke their promises. This was the case in the late nineteenth century when the great powers made the guarantee of minority rights in the Balkan states a condition for diplomatic recognition, as it was a century later in Bosnia. A more recent example (not cited by Krasner) occurred when James Baker, the former American Secretary of State, visited the new Central Asian states in 1992 to insist that the United States would recognize them only if they committed themselves to democracy and to market economies. President Korimov of Uzbekistan met Baker at the airport and waved a document, ostensibly a democratic constitution, and verbally promised to espouse the liberal economic faith. That was enough to gain American recognition. Both promises have been systematically violated since in all the new Central Asian states. In such cases there may be violations of a mythical Westphalian norm, but they are only symbolic and impose no subsequent constraints on governments.

The final problem is benchmarks. Krasner cites a large number of cases of sovereignty

violation in the domains of minority rights, human rights, borrowing, and government structures. But taken together, do these establish a pattern of (1) violations of the Westphalian norm by most states most of the time, (2) violations by some states at some times, or (3) are they anomalies, that is, a few violations by a few states in unusual circumstances? We have no way of knowing. On an impressionistic basis it seems no exaggeration to claim that most states most of the time adhere to the Westphalian norm even in its extreme Krasner form. If behaviour is consistent with the norm most of the time, then the norm is, in Krasner's term, 'embedded' and is not really an example of organized hypocrisy. Moreover, some of Krasner's cases of imposition of government structures and personnel from outside are so extreme (e.g., Soviet imposition of communist regimes in East Europe in the late 1940s and American invasions of Caribbean states in the early twentieth century) that they might better be termed 'occupations'. In these cases sovereignty is in effect suspended; the hypocrisy is that states continue to recognize such occupied territories as sovereign states. Overall, given that all the cases are selected to prove the main argument, it requires an act of faith to accept Krasner's placement of the Westphalian norm in the organized hypocrisy category.

The absence of conceptual analysis and familiarity with the philosophical discussions of sovereignty thus leads to difficulties. Krasner unfortunately uses concepts such as authority, influence, exclusion, and autonomy interchangeably (cf., pp. 3, 8, 20, 22, 29, 37, 53, 73, 125) when they should be distinguished from each other. Influence deals with acts, processes, and outcomes. Authority relates to rights, obligations, and consent. Sovereignty concerns the latter, not the former, although admittedly the line between them is not always clear and distinct.

With a stricter interpretation of sovereignty, we can more easily speak of norms. Violations of rights and territoriality do occur – witness the attempts of the United States to apply its laws and jurisdiction in Mexico, Colombia, and Panama – but the general practice of demanding extraterritorial jurisdiction, common in China a century ago, has almost disappeared. In contrast, it is difficult to agree with Krasner's proposition that states that voluntarily accede to international conventions are violating their sovereignty. To make every constraint on behavior a violation of sovereignty is to set up an impossibly high standard, which then means that sovereignty would be a norm only for saints, not for states.

The theoretical contribution of this study is nevertheless prominent. Krasner offers a robust argument – one consistent throughout his career – that the 'logic of consequences' better explains outcomes in international relations than does a 'logic of appropriateness'. This is a formidable challenge to those theoretical schools in the field that emphasize 'identities', discourses, roles, and norms as the primary sources shaping foreign policy. The many interesting cases he offers provide strong support for the older tradition in the field which sees the continued importance of power asymmetries, interests, and coercion in the international realm. However, in making the case for the continuing predominance of power and interest in international relations, Krasner goes to the extreme of denying *any* effective influence of norms and institutions in the shaping of interests and actions:

Given the absence of any international authority structures, the asymmetries of power among states, and the diversity of norms espoused by rulers and their constituents, it is *impossible* [my emphasis] for any institutional arrangements at the international level to become embedded . . . Norms and actions have become decoupled. Logics of consequences have trumped logics of appropriateness (p. 220).

There are too many cases suggesting otherwise to make this a valid generalization. Krasner offers a powerful case, but his commitment to it regrettably overlooks critical conceptual distinctions and a mountain of evidence suggesting more modest or nuanced conclusions about the role of norms and institutions in international politics. Krasner has contributed to the debate but this important work does not lead to closure.

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Robert H. Bates, Avner Greif, Margaret Levi, Jean-Laurent Rosenthal, Barry R. Weingast, *Analytic Narratives*, Princeton: Princeton University Press, 1998.

Comparative politics, as a sub-field of political science, is undergoing a great transformation. In the post World War II era, students of comparative politics sought, through a variety of theoretical approaches, traced to the seminal works of Marx, Weber, and their twentieth-century disciples, to account for the fundamental differences between states. Their goal was to navigate the historical paths to democracy, to state order, and to nation building, and to differentiate those paths from the ones that lead to autocracy, revolution, and national separatism. These patterns were discovered inductively, and focused on macro-independent variables, such as class alliances, social differentiation, and economic development. Yet the testing of these theories on a general scale presented formidable problems. With the demise of colonialism, the number of states in the international system grew rapidly, and it was well nigh impossible for any scholar to have a grasp of political processes in the full set of countries. Furthermore, without comparable data across regions, it was impossible to test propositions statistically. As a response to these challenges, comparative politics divided itself by region. A regional specialist could compare a relatively small number of countries, but avoid the problem of overdetermination because many possible explanatory variables could be controlled under the assumption of common regional context. For a generation, leading works in comparative politics were inductive in discovery, macro in orientation, and regional in focus.

A crucial concern that challenged standard practice in comparative politics, probably most brilliantly developed in the work of Jon Elster, is that leading theories in comparative politics were often obscure in accounting for the *mechanisms* by which values on independent variables (for example, class coalitions in the period of a country's early period of modernization, as proposed in Barrington Moore's classic work) influenced values on dependent variables (for Moore, whether the country was a democracy or dictatorship in the 1930s). A concern for mechanisms has had broad implications for scientific practice. For one, the great forces of history, if indeed they are forces, need to have an impact on individuals who make choices, because these individual choices constitute the mechanisms that account for outcomes. In this sense, the key variables needed to be micro rather than macro. Second, because individual choices need to be analyzed in such a way as to say, 'any person facing such constraints is likely to choose this course of action over any other', the reliance on regional

expertise provides little leverage. To a considerable extent, a focus on mechanisms requires minute knowledge of cases rather than a broad understanding of common experiences within a region. Third, propositions that are as micro as ‘any person facing such constraints . . .’ need to base themselves on first principles that are built upon reasonable assumptions about how people make choices under constraints. Borrowing from economics that has long relied upon models built on first principles, theorists concerned with mechanisms are attracted to theory that is deductively derived. These deductively derived models give local case work a universalistic appeal. The great transformation in comparative politics, in a search for the underlying mechanisms of received theory, is one that promotes work that is deductive in discovery, micro in orientation, and local in focus, though.

*Analytic Narratives* as a manifesto provides a model for a future sub-discipline that is micro, local, and deductive. This book is authored by two eminent economic historians (Greif and Rosenthal), a political economist who has made fundamental contributions to the field of American politics (Weingast), a political scientist reared in the macro tradition who helped lead the micro revolution in her discipline (Levi), and only one scholar whose pedigree is in regionally based comparative politics (Bates). This group, working together at the Center for the Advanced Study in the Behavioral Sciences at Stanford University, laid down the gauntlet to the comparative field as traditionally defined within the political science discipline. Their book has already, only a year after publication, induced critical scrutiny and lively discussion.

While many of the leading comparativists, taking advantage of the explosion of data bases, have relied on cross-sectional statistical tests of their theories, the authors of *Analytic Narratives* have made a bold and innovative move to give new direction to the great transformation. Cross-sectional data, they point out, do not speak to the great event in political history, those events that drove us into the study of politics in the first place. They therefore committed themselves to study those events microscopically, to build models that illustrate the mechanisms that drove the outcomes, and (in the future) to test the observable implications of those models with comparative statics. Because the cases they choose are invariably unusual, they do not seek universality in explaining differing outcomes. Rather, by specifying a small set of mechanisms that drive outcomes in many different domains of social, political, and economic life, and to understand the conditions under which different equilibria are reached, they hope to elucidate the fundamental elements of political life. Their search for universality, as with Aristotle’s is to ‘cut nature to its bone’. Thus, in *Analytic Narratives*, the narratives are local, but the mechanisms are universal.

The method that gives coherence to the essays in this volume is what the authors call an ‘analytic narrative’. It consists of six moves: (1) The identification of a stunning but insufficiently explained political outcome. (2) A review of standard narratives, with the purposes of showing lack of consensus among experts, revealing inconsistencies with standard theories, but also understanding in an inductive manner the mechanisms that might have driven the outcome. (3) The building of a deductive model, one that analyzes (i.e. breaks down to its constituent elements) the roles of all principal actors and the choices they confronted in the process of reaching the outcome. This model should make predictions about the conditions under which the observed outcome is likely to be reached. In its game-theoretic version, the standard in *Analytic Narratives*, the model should make equilibrium predictions. (4) A retelling of the narrative in line with the model, such that the story is now internally consistent and in accord with historical reality. (If it isn’t, return to (3) and

construct a different model). (5) The mathematical manipulation of the model, such that observable implications of the model are derived. (6) The statistical test of the observable implications, such that if these tests allow for the rejection of the null hypothesis, the analyst gains greater confidence in the aptness of the model in accounting for the stunning outcome.

Concerning the first move, it becomes clear that what holds the chapters in this volume together is the analytic method rather than any particular substantive issue. The outcomes that the authors pick are unrelated to one another. Greif analyzes the creation of a political order in late medieval Genoa that induces economic growth, after a long period of inter-clan warfare and economic stagnation. Rosenthal seeks to account for the differential ability of seventeenth- and eighteenth-century French and English kings to make war, and the implications of those differences for economic growth. Levi seeks to explain the secular shift away from conscription regimes that allowed potential draftees to buy their way out of service (or find substitutes) and toward systems of universal conscription in France and the United States. Weingast develops a new perspective that synthesizes contradictory accounts for the American civil war. And Bates seeks to explain the unusual persistence of the coffee cartel of the 1950s, when standard economic theory would have predicted its rapid collapse. Spanning three continents, a millennium, and a variety of themes, there are no findings about variation on a particular dependent variable that are sustained by a cumulative theoretical exploration. Rather, each author has identified an outcome of sufficient importance and unexpectedness as to demand theoretical explanation.

As for the second move, the review of standard literatures, Weingast's essay is exemplary. He examines the long-standing debate between traditionalist historians who focus on the question of slavery as driving the North and South toward warfare and the New Political Historians whose explanation for the war centers on how local issues such as immigration, land policy, and tariffs overwhelmed the national party system, and undermined the union's ability to address sectional concerns. Weingast's model integrates both approaches in a seamless web, reconciling the two with a simple model of an institutional rule (the Missouri compromise) that permitted local issues to play a central political role, while at the same time the issue of slavery was kept just under the surface. The compromise collapsed due to the demographic growth of the West, and the inability of the North to commit to a Senate that would not in the future be able to challenge slavery. Southern politicians were compelled to politicize the slavery issue and local issues all of a sudden became of secondary importance. Thus local issues were only apparently dominant in political life as long as the North was able to commit credibly to an everlasting Southern veto in the Senate. Weingast thus constructs a coherent explanation out of two opposing standard narratives, an inductive exercise that forms the basis of his deductively based commitment model in the third move of the analytic narrative method.

Capturing the essence of a complex political process in a single model (step 3) is no easy task, and is at the core of the analytic methods. Rosenthal, for example, seeks to explain the institutional sclerosis of seventeenth-century France (as compared with the UK) through a simple model of taxation and warfare. He posits only two actors: the King and the Elite (Members of Parliament). It assumes that the King suffers little from war, but the Elite suffers greatly. Therefore, to the extent that the King can tax for warfare without facing an Elite veto, the state will overspend on war. To the detriment of the country's economy and the evolution



of its political institutions. The model captures in a nutshell why political evolution in France was stunted: the King's private gains from failed warfare outweighed his expected returns from a more peaceful parliamentary regime.

The retelling of the historical narrative in line with the model (the fourth step) yields both insights and anomalies. Levi retells the history of French and American conscription policy through the lens of her model in order to show how the relative influence under different institutional and security situations of the government, the traditional elite, the bourgeoisie, the farmer, and those of low wealth predict an equilibrium conscription policy. Yet much is left unexplained. Even more problematic for her original formulation was the examination of her model in light of Prussia's conscription history, an out of sample case she examined as an additional test of her model. This sent Levi back to the drawing boards, seeking to show why a democratic government would abandon an efficient conscription policy that was perceived by many constituents as racially unfair. Here she begins to develop a not yet fully specified model of contingent consent, with an eye towards explaining why democratic governments value such consent so highly.

Bates also gets considerable theoretical mileage from the fourth step. He first analyzes the rise and decline of the International Coffee Organization with a reputation model that predicts that Brazil and Colombia would expend vast resources to deter entry (especially by African producers) into the international market. While the model is internally consistent, its 'predictions' in this case are wrong, as Bates' first narrative exposes. This failure says nothing about the logic of the reputation game; rather it says that it did not capture the essence of the strategic dynamic in the international coffee market. Bates goes back to the theoretical drawing board, and captures the role of a new set of actors in the United States who in strategic interaction would up supporting the coffee cartel even though this would hurt the interests of their consuming nation.

On the final two steps, the drawing of observable implications and the testing of those implications on a wider set of processes, Greif's work provides a road map. Greif builds a mutual deterrence model to account for the institutional innovation in Genoa that involved the invitation of a foreign leader (the *podestá*) who was able to deter clan warfare, present a common defense to the predations of the emperor Henry Barbarossa, and thereby set the stage for a long period of prosperous trade. The key to the equilibrium is that the *podestá* had to have sufficient resources so that his entrance into a clan battle on the side of the weaker clan would be decisive in its victory over the stronger one. Yet he could not have such resources as to enable him to become a dictator, enabling him to expropriate the wealth of all leading clans. One observable implication of the model is that the *podesteria* could only function if the clans were sufficiently equal in power as to ensure a low salary to the *podestá*. He therefore predicts that cities with greater disparities of wealth among clans would be less able to sustain the *podesteria*. Another observable implication is that both clans would have an interest to assure themselves that the *podestá* did not collude with the other clan, and would attempt to restrict either clan's ability to commit to rewarding the *podestá* following a successful collusion. Greif therefore looks and finds such restrictions. For example, he was not allowed to marry with a Genoese, enter into commercial activity, had to serve only for a short term, and his mail was read in public. To be sure, these are not quite comparative static tests of the model (Greif does that in related papers). Nonetheless these implications were confirmed by

documents in the archival record. Since they were not part of the original model, one gets added confidence that the model is a good representation of political reality.

These six tasks in the analytic narrative approach emphasize a dualist methodology: analytics and narrative. Yet exemplary work in comparative politics is necessarily triadic. Such work must, through induction, find the principal independent variables that help explain variance. It must also give some theoretical accounting for the pattern of outcomes in a manner that is deductively oriented. Finally, it must trace particular cases, such that the mechanisms specified in the theoretical accounts are shown to have force when looking at particular instantiations of the proposed theory.

The authors of *Analytic Narratives* with their dualist methodology have shown the links between theory and narrative in a cogent way. But what is missing from their enterprise is the link between their theories and the explanations for variance across cases, especially cases that are out of sample. For example, several of the papers – Greif's, Bates' and Rosenthal's – theorize on the political foundations for economic growth. Each has a model and a set of narratives that deepen our understanding of the model. But (with the partial exception of Rosenthal) none has identified institutional variables that if plugged into a cross-sectional data set would powerfully predict long-term economic growth. Levi too examines an out-of-sample case, but she does not set up a cross-sectional test to explain the conditions under which states, through their conscription policies, will seek to invest in legitimacy. And thus, while this volume provides the elements of explanation for political outcomes (the basic strategic situations such as reputation, deterrence, commitment), its links to the compounds of political life (civil war, legitimacy, prosperity) remain weak. Incorporating the dualistic method of *Analytic Narratives* into the triadic challenge will be, in my estimation, the next methodological breakthrough in comparative politics.

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J. Robert Brown, Jr. *The Ministry of Finance: Bureaucratic Practices and the Transformation of the Japanese Economy*. Westport, CT: Quorum Books, 1999.

J. Robert Brown's work on Japan's Ministry of Finance (MOF) is written largely in reaction to the heavy criticism the ministry has received in the wake of recent large-scale economic and financial policy breakdown. Drawing on extensive interviews with central government bureaucrats and industry actors, Brown constructs a defense of the ministry by pursuing three lines of argument. First, Brown challenges the conventional assessment of the ministry's performance in recent decades. He argues that the ministry has achieved some extraordinary accomplishments that have gone unheralded amid the policy breakdown. Second, Brown argues that while the ministry's bureaucratic practices played a role in inhibiting effective policy responses, the ministry learned from its mistakes and significantly altered its practices when faced with public criticism. In this way, Brown emphasizes the MOF's institutional

adaptation and responsiveness over bureaucratic inertia and resistance to change. Third, Brown argues against MOF responsibility for policy failure by challenging the myth of an all-powerful and unchecked bureaucracy. In Brown's view, the MOF has received criticism in the 1990s for policy breakdown when criticism should instead be directed at elected politicians who constrained the ministry's actions and then used the ministry as a shield to deflect public criticism from themselves.

Brown's work is rich in detail on the structure of the ministry, personnel practices, and organizational dynamics. The book thereby contributes to the growing body of empirical information on the Ministry of Finance. Yet, Brown's arguments are seriously flawed – whether evaluated individually or considered together. His failure to employ a clear analytical framework or clarify causal relationships leads to an inconsistent account of the MOF's role in policy making and regulation. Poor editing and organization further detract from the quality of the book.

Let us first turn to Brown's assessment of MOF's performance. Brown cites financial system reform since the 1980s and tax policies as areas of policy success for the ministry. He states, for example: 'Both in casting off the antiquated compartmentalized financial system and in shifting the tax base, the MOF has created the dynamics necessary to ensure future growth and to solve some of Japan's inevitable problems, particularly those arising out of the rapidly aging population' (p. 215).

Few would agree with this interpretation of the progress of financial system reform in Japan. The walls dividing financial institutions have only really been torn down since the implementation of the Big Bang financial reforms, begun in 1998. While small incremental changes were effected earlier, many of the problems played out in the Japanese financial system in the 1980s and 90s have been byproducts of incremental deregulation that skewed the incentives of industry actors. Further, Brown's statements concerning the state of public finance and prospects for future growth seem to reflect a genuine ignorance of the facts. Even if Brown's assessment were accurate, the book provides no insight into why the bureaucratic practices and political constraints that he argues contributed to policy breakdown in financial regulation were not problematic in these other policy areas.

Brown's second line of argument, his positive appraisal of the MOF's capacity to adapt and change, emerges paradoxically out of four case studies that detail the ways in which the ministry erred in dealing with policy challenges. Here Brown argues that the MOF simply gave the expected bureaucratic responses to challenges faced. Yet, it is not clear whether this bureaucratic response was a distinctive Japanese bureaucratic response or a response we would expect from any bureaucratic agency. It is unfortunate that Brown does not draw on any of the rich literature on the bureaucracy to show how the MOF fits into broader arguments about the nature of bureaucracies. His only reference to other scholarly works, in fact, is to Johnson's *MITI and the Japanese Miracle* and van Wolferen's *The Enigma of Japanese Power*.

At the same time as Brown argues that practices or habits endemic to the bureaucracy impeded more rapid and effective policy response, he argues that the MOF was, in fact, adaptive in that it learned from the public criticism it faced. Brown asserts that the ministry 'increasingly opted for a more open and objective approach to regulation' (p. 229). For example, he lauds the ministry's greater disclosure of actual fact when seeking public funds to bail out the *jusen*.

This portrayal, however, seriously misrepresents both the source and magnitude of

institutional change. Without public awareness of the grave magnitude of the *jusen* problem, for example, the ministry would never have been able to secure the public funds necessary to resolve the problem. Disclosure and transparency were not systematically introduced, but rather, used only when there seemed to be no other option. Only since the October 1998 financial reforms has a paradigm shift towards greater transparency truly been evident. And, importantly, this shift arose out of the MOF becoming the object of change rather than out of self-initiated reform.

Brown's emphasis on the MOF's changed behaviour leads him to conclude that, even in the absence of significant legislative reform, the ministry's bureaucratic practices would 'continue to evolve and ultimately become consistent with the transformed economy' (p. 216). To Brown, then, the pace of change seems to be of little relevance. Nowhere does he consider the grave consequences that the slow evolutionary nature of institutional change has had.

Brown's third line of argument for political rather than bureaucratic responsibility is similarly problematic. He correctly points out the existence of political constraints upon the ministry. Yet, in trying to absolve the ministry of responsibility for policy failures, he goes too far. The bureaucracy is not without its own interests. The ministry can and has strategically used policy information and technical expertise in agenda setting and alternative specification of policies. Decisions made by elected representatives are based to a considerable extent on the information they obtain from the bureaucracy. And, the MOF has skillfully used its relational networks to manage information in ways that align with the ministry's institutional interests.

Brown's argument that the MOF simply heeds the will of the Diet leads him to assert that '[C]hange at the political level will lead to change within the bureaucracy' (p. 4). As the events of the past decade have shown, however, the nature of the causal link is more complex than he suggests. Volatility in the political party system in the first half of the 1990s served to further empower the ministry resulting from the way in which it led to a power vacuum in political leadership and eliminated an important countervailing force. Yet, political realignment in 1998 correlated with perhaps the greatest period of MOF weakness in the postwar period.

In addition to the above substantive criticisms, it must be noted that the book contains numerous redundancies. A proper introduction to individuals and events is also lacking in many places. Thus, readers not intimately familiar with the personalities and events in Japanese finance over the past decade will find themselves frustrated at times.

In summary, Brown's work acts as a useful corrective to portrayals of the MOF as an all-powerful and unconstrained institution. Yet, the book is plagued by inconsistencies and disjunctures between evidence and argument. In fact, Brown's account of the MOF supports a more nuanced portrayal of the financial policy-making and regulatory process as one involving constant bargaining among actors and shared responsibility for policy outcomes.

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Christopher W. Hughes, *Japan's Economic Power and Security: Japan and North Korea*, New York and London: Sheffield Centre for Japanese Studies/Routledge, 1999.

As one of very few sources in English on the topic of Japan–North Korea relations – and indeed on North Korea in general – Christopher Hughes' timely book surely is a must read for anyone seeking to better understand Japan's relations with its pariah neighbor. The book is well-sourced and documented and provides a wealth of detail on North Korea's economy and security policies and Japan's responses to them, incorporating a wide range of English- and Japanese-language sources. In addition, Hughes attempts to make the book of interest to a wider audience, including students of Japanese policymaking and foreign policy as well as IR generalists interested in how to better understand the link between economic and military security policies in this post-Cold War era. Unfortunately he is less successful at this latter task, though it is useful that his specific case of North Korea is situated in a broader theoretical context.

The greatest contribution of *Japan's Economic Power and Security*, despite relegating 'North Korea' to the subtitle, is the well-researched and well-presented summary of Japan and its allies' relations with North Korea, with particular focus on the post-Cold War period. Three-quarters of the book is devoted to three chapters which focus on Japan's 'conception', 'capacity', and 'will' to devote its extensive economic power resources to bettering its relationship with North Korea. Beyond this theoretical task, these chapters offer a broad history of North Korea and its relations with its neighbors – China, Russia, South Korea, the United States, and Japan – from both a military and economic perspective. It is difficult to find fault with Hughes' competent re-telling of important developments in North Korea and its allies' diplomatic relations with North Korea over the past ten years. One could quibble with the relative weight he chose to devote to various topics – mine would be with the only slightly more than a single page devoted to the issue of Theatre Missile Defense (TMD), and with the relative weight given to the various policymaking actors, which puts far more emphasis on political parties than business leaders and economic ministries despite his theoretical focus on 'economic power'. But on the empirical front, one cannot claim that important aspects of the diplomacy of this period were ignored or that outright errors or errors of omission plague the work.

From a theoretical and analytical perspective, however, *Japan's Economic Power and Security* lacks both breadth and depth. Hughes' main theoretical project is, in his words, 'to construct an overall theoretical model of Japanese economic power and to investigate empirically its use in the service of security policy' (xx). What is odd about his selection of the North Korean case is that North Korea is one state where Japan's vast economic power resources have not been translated into control over diplomatic outcomes. If anything, Hughes' cogent story demonstrates the *limits* of economic power: that the number two economic superpower in the world cannot achieve peace with its desperately poor neighbor despite its clear desire to do so. One statistic Hughes provides aptly illustrates the stark economic disparity: the North Korean market comprises less than one-tenth of 1 percent of Japan's total trade and even still this makes Japan North Korea's second largest trading partner! According to Hughes, and as his theory predicts, 'this high degree of North Korean trade dependence upon Japan clearly gives the latter a degree of direct power to impose negative economic sanctions and force North Korea to moderate its security behaviour' (145).

Yet his analysis shows precisely how little influence Japan's 'power' had over actual outcomes. The reason for Japan's apparent inability to capitalize on its economic power resources in the case of North Korea appears to be due to Japan's deference to South Korea and to the United States in this area. If one were to take his claims of economic power seriously, however, one would expect to see Japan to be far more assertive in the 1990s due to its increased economic power *vis-à-vis* the United States and outright economic superiority to South Korea. That this is not the case requires explanation. If the question Hughes seeks to address is how Japan uses its economic power resources to affect policy outcomes, other country cases would have been more appropriate. One area in which he does succeed, however, is to show how increasingly in the post-Cold War period military security questions involve an economic dimension – such as with the KEDO solution to the North Korean nuclear crisis.

The book also seeks to make a theoretical contribution to one of the great debates on Japanese policymaking regarding the relative weight of bureaucrats and politicians in the process. His detailed story of the North Korean case clearly demonstrates a large role for politicians in this issue area, though, as mentioned above, it is unclear how this argument can be reconciled with his larger project of arguing for the primacy of economic power. Moreover, it is not clear based on the evidence he presents that the deadlock in Japan's normalization of its relations with North Korea is the fault of the Japanese policymaking process, as he claims (p. 156), rather than on North Korea, South Korea, or the United States. He seems to argue that if Japan were better able to capitalize on its economic power, its relations with North Korea could be improved. However, he fails to spell out exactly how he envisions this happening, or where exactly Japanese policymakers went wrong.

A final, unfortunate reality of Hughes' book is that it demonstrates the continuing division of English-language Japan scholarship between the American and British schools. While Hughes' extensive bibliography includes a wide range of both British- and American-based authors, his theoretical analysis is based almost solely on the British school to his detriment. For example, in his introduction he writes: 'fewer scholars have tried to bridge the gap between Japan studies and IR' (p. xx), citing only Reinhard Drifte as an exception. Such an assertion ignores his own bibliographical citations of Richard Samuels, Michael Green, and Thomas Berger among Japan specialists, as well as major American IR scholars such as Richard Betts, Robert Gilpin, Samuel Huntington, Richard Rosecrance, among others, who all have considered the role of Japan as a great power in the international system in their influential works. Hughes own theoretical framework would benefit from more consideration of scholarship from both sides of the Atlantic, as will Americans benefit from his important contribution – which unfortunately will not get the exposure it deserves due to its nearly \$100 price (Routledge and Sheffield please note).

In sum, *Japan's Economic Power and Security* conveys a compelling empirical story that is well worth a thorough read. What it lacks in theoretical gusto is more than made up for in careful detail. As Hughes rightly asserts, the case of North Korea is 'one which incorporates the key post-Cold War and global security issues' in addition to being 'an intrinsically important security problem in itself' (xxi).

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