

## Book Reviews

Eric A. Feldman, *The Ritual of Rights in Japan: Law, Society and Health Policy*. Cambridge: Cambridge University Press, 2000.

In *The Ritual of Rights in Japan*, Eric Feldman takes issue with prevailing views that the assertion of individual rights is fundamentally incompatible with Japanese cultural, legal, social, and political norms. Through careful historical analysis and in-depth case studies, he argues persuasively that, even though individual rights have not always been secured within the legal sphere, the *assertion* of rights has long been a significant component of social conflict, citizen mobilization, and, more recently, litigation. By exploring rights assertion in each of these spheres, Feldman casts new light on prevailing approaches to Japanese historical and legal studies and theories of political culture.

Feldman's primary concern in this landmark book is not so much the extent to which rights have become entrenched in Japanese law as the socio-legal context of rights by aggrieved citizens within the political and legal spheres. More specifically, he explores how the language of rights has been used strategically – and effectively – by social and political activists to 'frame, discuss, and debate issues relevant to social policy' (p. 4) and to mobilize like-minded individuals on behalf of common goals. In Chapter 2, he traces the history of this phenomenon to the eleventh century. He notes, for example, how early peasant petitions, by resting on expectations of assistance from authority figures, constituted a nascent form of the 'justified claim' that is so essential to modern rights assertion. By the late Tokugawa era, peasant unrest had evolved to embrace the notion of individual entitlement – another basic component of modern conceptualizations of individual rights. Although these early forms of rights assertion were often suppressed through legislation and state efforts to discourage litigation, Feldman's message is clear; the fact that the very notion of individual rights has indigenous roots that extend back centuries constitutes a challenge not only to the conventional wisdom that rights assertion in Japan is nothing but a foreign import from the early modern era, but also to: (1) the utility of applying idealized (and hence distorted) models of American rights assertion to the Japanese historical record, and; (2) to the widely held cultural myth that Japan is a homogeneous and consensual society based on commonality of thinking between state institutions and the public at large.

In Chapters 3 through 5, Feldman explores the history and significance of the

environmental, of taxpayers, and of so-called ‘new rights’ (*atarashii kenri*) movements of the 1960s and 1970s—movements which were significant from a contemporary standpoint not so much because they symbolized a heightened public awareness of individual rights, but because they marked for the first time in history that the *language* of rights assertion (‘rights talk’, in Feldman’s words) was consciously intertwined with social protest. Even though these movements were in many ways unsuccessful, they were significant because

At the very least, they laid bare the reality of rights in Japan—that rights are not remote, alien, misunderstood entities of a foreign legal system; that ‘the Japanese’ are not unable to articulate rights claims; that the culture of Japan is not so harmonious, consensual, or hierarchical that conflicts are solved through informal channels to the satisfaction of all parties. (p. 44)

The new rights movements, with their emphasis on ‘rights talk’ and litigation, were also significant because they served as models for the patients’ rights movements of the end of the twentieth century – the empirical foci of Feldman’s study. After exploring the denouement of events surrounding the tainted blood scandals and brain death controversies of the 1980s and 1990s, Feldman devotes a fascinating chapter to the role and significance of litigation in each of these cases. He argues convincingly that despite the often onerous financial and temporal costs incurred by plaintiffs, litigation is actively pursued by citizen activists not only to ‘secure remedies’ for the victims of corporate and governmental negligence, but also to legitimize rights-related claims. In keeping with the latter function, litigation is significant insofar as it publicizes disputes and claims to broader publics, increases the public’s awareness of injustice, and, finally, attracts members to new political/legal causes.

One might also add to this list the *educational* functions of litigation. As students of the environmental movement (see, e.g., Margaret A. McKean, *Environmental Protest and Citizen Politics in Japan*, Berkeley: University of California Press, 1981) have demonstrated, Japanese social movement activists are often frustrated by weak levels of rights *consciousness* within the public at large and have looked to litigation – in addition to various forms of social protest – as tools for educating the uninitiated or disinterested on the value of rights assertion. My own research on consumerism corroborates this point.

With this in mind, I question the universality of Feldman’s argument; an awareness of individual rights and willingness to act on those rights may not, in other words, be as deeply embedded in Japanese society as his case studies imply. I am not suggesting that his findings are unwarranted in the cases of AIDS and patients’ rights activism, but rather that rights assertion may be far less advanced *vis-à-vis* other issue areas. The nature of those issue areas may hold the key to these differences. For instance, rights consciousness and a concomitant upsurge in rights assertion may be more common within relatively small groups of individuals who harbor intense grievances of life-and-death significance, than in cases where grievances are comparatively weak and spread thinly over large populations. Peasants whose chances of survival have been weakened by landlord negligence or the families of those who have perished at the hands of pharmaceutical companies and their governmental accomplices are much more likely to harbor a strong sense of injustice toward those in authority and to invoke the language of rights than a broad consuming public that is forced to pay higher prices as a result of the proliferation of cartels in the economy.

These observations should not detract from the value of this volume. *The Ritual of Rights in Japan* is, quite simply, a very well argued comparative study that provides new and

compelling insights into the role of individual rights in Japan, persuasively critiques prevailing models of the legal system and social conflict in Japan, and effectively highlights the similarities – as well as differences – between Japan and other countries. As such, it should appeal to students of Japanese history and politics as well as legal studies. It is, in short, essential and provocative reading for all students of Japan.

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Peng-Er, Lam, *Green Politics in Japan*, London: Routledge, 1999.

To what extent is Japan converging with the West in its post-industrial politics? The question of whether Japan is converging or diverging with Western norms and politics is an issue that has and continues to fascinate political scientists and comparative politics scholars. In *Green Politics in Japan*, Lam Peng-Er, a lecturer in the Department of Political Science, National University of Singapore, explores the scope and potential for New Politics, or Green Politics in Japan by analyzing a small Japanese ecology party known as the Network Movement.

While the rise of mass parties is associated with an industrial democracy, Dr Lam argues that the emergence of Green parties is connected to a democratic, post-industrial society. In technologically advanced and knowledge-intensive democratic societies, ordinary citizens have more resources, including information, that enable them to participate in politics. A hallmark of a post-industrial society is the prevalence of the white-collar, service sector rather than the blue-collar, industrial sector in its economy. By the 1970s, post-war years of unprecedented affluence had led to 'embourgeoisement' of the working class, and the rise of the white-collar occupational sector had diminished the ranks of the blue-collar workers.

Green parties, described by one scholar as the 'one of the most important political developments within Western European societies in the last two decades', have proliferated in many West European countries, and generated a lot of support due to a 'new' agenda that promotes ecology, gender equality, pacifism, an anti-nuclear platform, the practice of an alternative economy, as well as participatory ethos that set them apart the traditional political parties.

Although electoral support for many Green parties were negligible at the outset of the 1980s, they were able to secure between 5 to 10 per cent of the popular votes less than ten years later. The relative weakness of the Green parties in terms of mass membership – compared to traditional political parties – mask the important role they often play in pushing issues on to the national policy agenda. 'The significance of the Greens', suggests Dr Lam, 'lies in their role of adding a new conflict dimension to old social cleavages. More importantly, this movement reflects the changing structures and the values of a post-modern or post-industrial society' (p. 3).

Since Japan is similar to Western Europe in terms of its post-modern or post-industrial societal structure, are generalizations about Green party phenomenon applicable only to

Western Europe and its cultural offspring in North America, Australia, and New Zealand, or do they apply to all examples of post-industrial economies including Japan?

As Japan attained its post-industrial status in the 1970s and has undergone rapid changes in its value systems, the experience of Western Europe suggests that Japan should also be feeling the impact of New Politics. A major stated goal of the book *Green Politics* is to add a non-European and North American perspective to the New Politics academic literature.

The subject of Dr Lam's inquiry is a small ecology party known as the Network Movement (NET), which successfully obtained 124 seats in prefecture, city, ward, and village assemblies, and was a ruling coalition partner in the local governments of Kamakura, Kawasaki, Fujisawa, Zushi, and Machida cities. In addition to NET, *Green Politics* examines the strengths and organizations of the Liberal Democratic Party and the Japan Communist Party.

According to Dr Lam, various Japanese social movements and political parties have adopted certain principles that are associated with the West European Greens. However, no significant Green parties have arisen in Japan, while NET – the 'greenest' party in Japan – lacks certain characteristics that qualify it as a full-fledged Green party; it appears to be more hierarchical than most European Greens. Moreover, no Japanese New Politics party has won elections at the national level, unlike many West European Green parties.

Does Japan have its own version of New Politics? The answer appears to be no. Despite shared political views on ecological protection, pacifism, and alternative economy, there are many noteworthy differences between NET and West European Green parties, including: 'affluent housewife activists who are led by males; the narrow larger societal issues; and narrow social support from housewives rather than students, youths, male and female white-collar professionals' (p. 160).

Although the book raises important questions and provides an interesting account of local Japanese politics, one wonders if there was enough components of New Politics in NET to justify comparing this organization to West European Green parties. To his credit, Dr Lam notes many of the methodological difficulties in comparing NET to West European Green parties in the book.

The book's most interesting insight may have been the relationship between urban political organizations and what Dr Lam refers to as 'social networks whose cohesion comes from personal ties, group loyalties, and reciprocal obligations rather than a purely ideological approach' (p. 165). The role of social networks in Japanese urban politics represents the critical issue in the book that deserves further scrutiny.

Instead of the title 'Green Politics in Japan', which gives a misleading impression that the book focuses on environmental politics, perhaps a more appropriate title of the book might have been, 'Social Networks in Japanese Urban Politics'.

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Stephen Johnson, *Opposition Politics in Japan: Strategies Under a One-Party Dominant Regime*, London: Routledge, 2000.

For years, the conventional wisdom on Japanese politics has been that ideology played a central role in the Japanese opposition's inability to challenge the long-time ruling Liberal Democratic Party (LDP). This conventional wisdom held that the Japan Socialist Party (JSP) failed in its attempts to develop a thoroughly broad following because of its inflexible and radical ideology and the different parties within the opposition were unable to work together because of their widely varying ideological positions. However, with a new generation of Japan party scholars has come work seeking to dispel this view. Like Masaru Kohno's micro-analytic approach (Kohno, *Japan's Postwar Party Politics*, Princeton, NJ: Princeton University Press, 1997), and Ray Christensen's focus on the difficulties of electoral cooperation (Christensen, *Ending the LDP Hegemony*, Honolulu: University of Hawaii Press, 2000), in *Opposition Politics in Japan* Stephen Johnson indicates a Japanese opposition that, throughout the 1955–93 period he examines, was pragmatic in its decision making rather than blinded by ideological radicalism. In short, Johnson places his work squarely within a literature that is profoundly trying to change our understanding of Japanese politics, in particular in its effort to make us take Japan's opposition parties seriously.

Johnson makes a convincing case that, with its emphasis on the behavior of the LDP, studies of Japanese party politics have indeed been looking in the wrong place. He argues that, especially given the fact that the LDP has not won a majority of the Lower House vote since 1963, the failures of the opposition have been absolutely critical to the LDP's long run in power. Johnson turns his attention, therefore, to the shortcomings of the opposition, examining a wide-ranging set of issues in the post-war history of Japanese opposition parties and their attempts to overcome the dominance of the LDP.

Johnson provides an interesting theoretical foundation for his analysis, centering on the role of 'organizational considerations' in shaping most party decision making. Organization has been at the heart of many English – and Japanese-language explanations of opposition failure in Japan, with the argument that the JSP was constrained by a radical labor organization that would not allow the party to moderate its platform. However, unlike explanations that focus on organization in constraining the flexibility of parties' potential ideological lodestars, Johnson argues that the desire to protect organizational stability was the primary determinant of party strategy. Indeed, examining in great detail the development of ideological reform within the Japan Communist Party (JCP) and JSP over 1955–62, Johnson finds that ideology was often pursued more as a way to win intra-organizational battles than to promote the holy truths of the party or even to win office. For this reason, utilizing two concepts of Angelo Panebianco's, Johnson rejects the 'rational model', in which parties are organized to achieve identifiable ends, in favor of the 'natural systems model', whereby 'official party aims are seen as a façade, behind which different stake holders compete for control' (p. 25). According to the rational paradigm, parties are particularly concerned with particular goals, such as ideological objectives or success in elections. Johnson instead finds parties focused far more on maintaining organizational stability, along the lines of the natural systems model's contention that each party focuses on creating an equilibrium between the groups and interests which make up its organization.

Johnson also downplays the most commonly used analytical framework of the 1990s, the

role of the electoral system in shaping opposition party fragmentation. Examining the failure of candidates running under the banner of Rengô, the new umbrella trade union, in 1992, Johnson indicates how the various union groups were unable to work together successfully, even when nominally joined in the same larger organization. As a result, Johnson argues, organizational factors – born out of Japan's industry–labor relational structure – and not electoral system factors, hindered the ability of different union groups to work together in electoral activities in 1992.

The heart of Johnson's analysis is in three chapters that offer an explanation for the failure of the opposition parties to cooperate effectively over 1970–80, a period when the LDP was especially weak and ripe for the toppling. First, over the course of two chapters, Johnson chronicles the opposition's inability to work together, in particular in national level elections. Johnson explains that much of this failure was due to the fact that many of the parties involved had sought coalitions in large measure to help overcome organizational crises. Based on this finding, Johnson develops a 'stillborn coalition model', which argues that alliances formed as a solution to such crises are very likely to fail: 'Even this solution becomes a new source of instability within the organization, hastening the early demise of the coalition, or alternatively, leaderships are unwilling to take the de-stabilizing steps necessary for success' (p. 59). Second, Johnson delivers a chapter on opposition alliance *success*, the 1975 Kanagawa Prefecture gubernatorial race. Again, citing organizational considerations, he argues that this success was due to the substantial strength and stability of all the opposition parties in the prefecture.

Johnson's analysis would have benefited from greater attention to the development of his core concepts and his research design. Most striking, it lacks a clear definition of 'organizational considerations' or 'crises', and the concept appears to run from simple intra-party battles to the number of votes the party won in the most recent election. The latter conception appears to stretch beyond most definitions of organization, and suggests that Johnson might have done well to develop a model that was more concerned with the *electoral* strategies of the opposition parties *vis-à-vis* each other. From a research design perspective, Johnson cleverly attempts to introduce variance on his dependent variable by adding a case of opposition success in his chapter on the Kanagawa Prefecture governor's race. At the same time, local and national level opposition party cooperation are not fully comparable. National coordination is far more difficult, in particular as it includes working out alliances between candidates in literally hundreds of districts across the country. In short, there appears to be no variance on his dependent variable of opposition alliance success, making it difficult to generate strong conclusions from his analysis.

However, utilizing information on all the parties in Japan, a far simpler explanation than organizational considerations comes to mind: unless forming a government coalition, nearly all national alliance attempts will fail. Johnson makes almost no mention of the LDP, but, like the opposition, the LDP has faced countless organizational crises. In the end, though, it was able to maintain power because of its control of the government. Without some form of comparative analysis to the contrary, Johnson is unable to reject this simpler model.

Johnson's lack of a comparative framework also limits the utility of his natural systems model. Johnson's use of the natural systems model is thought provoking, especially because most analysts do tend to consider parties from the perspective of the rational model. However, in indicating the strengths of the natural systems model in the case of the Japanese opposition, a number of questions naturally arise. When do we know which of the two models to use? Is

the rational model simply wrong? Are Japanese opposition parties different from the LDP? Are Japanese parties different from those in other countries? If so, how and when can we predict when the natural systems model will offer greater explanatory power than the rational model?

Additional evidence would have been welcome in places, especially to hammer home some of his stronger claims. For example, Johnson's rejection of ideology as the primary driving force behind party behavior is usually convincing. Yet, given that he never offers evidence that such ideology did not hurt the standing of parties such as the JSP in the eyes of the voting public, Johnson's claim that ideology 'did not necessarily have a negative impact on any party's ability to compete within the political system' (26) overstates his case. Similarly, Johnson's emphasis on the organizational reasons behind party fragmentation is fairly novel, but does not include analysis that would allow him to reject the electoral system argument.

Johnson's study provides a great deal of grist for future studies. The implications of his discussions of the impact of organizational imperatives are provocative, but, especially given the heavy role of detailed case studies in his analysis, it would have been helpful if he had offered greater explanation of the actual internal workings of the parties. For example, what is the relationship between, say, the labor unions and the individual parties and how is power really exerted in the parties?

Finally, *Opposition Politics in Japan* fails to provide background on a number of individuals and events about which only Japan specialists would have prior knowledge, not unexpected from a book that appears to be an un-revised version of a dissertation. For example, it makes vague references to politicians, such as Ashida Hitoshi (former Prime Minister), without explaining their significance. More important, it quickly mentions the LDP's loss of power in 1993, where 'the LDP was unseated by an election . . . it did not lose' (p. 2), but explains nothing else about the event. As a result, *Opposition Politics in Japan* will be useful to Japan specialists seeking helpful case studies, but, for non-specialists, will probably need to be used in conjunction with others such as those by Christensen and Kohno.

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Keith Krehbiel, *Pivotal Politics: A Theory of US Lawmaking*, Chicago: University of Chicago Press, 1998.

For much of the 1990s, legislative scholars in the United States focused their energies on questions about legislative organization. Many scholars contended that the distributive perspective, which holds that committees are the dominant force in Congress and that these committees exist in order to procure (electoral) benefits for members, presented the most accurate description of Congress. Others began to argue for a more party-oriented perspective on Congress, noting that the majority party has a vested interest both in helping members to achieve re-election and in helping the party as a whole to achieve some of its legislative goals.

Each of these perspectives is identified with a specific set of legislative scholars, including (but not limited to) Kenneth Shepsle, Barry Weingast, and John Ferejohn for the distributive perspective and Mathew McCubbins, Gary Cox, David Rohde, and John Aldrich for the party perspective.

In addition to these two perspectives, a third perspective began to emerge in the late 1980s and 1990s, a perspective that has been developed most persistently and ably by Keith Krehbiel, both in his 1991 book, *Information and Legislative Organization* (Ann Arbor: University of Michigan Press), and in a series of articles with Thomas Gilligan. This perspective, which focuses on the informational and majoritarian aspects of congressional organization, holds that committees exist because of the information that they can provide to the legislature in general. Furthermore, rather than having committees as the dominant actor, proponents of this approach argue that the majoritarian nature of Congress – everything that passes Congress, including things as varied as policies, procedures, and organization – must be approved by a majority of members.

These three theoretical approaches – distributive, partisan, and informational – essentially set the research agenda for students of Congress for the last decade. Now Keith Krehbiel has come along with a new book, *Pivotal Politics*, which again will set the agenda for scholars for years to come. This time, however, his work will influence not only the work of other legislative scholars, but also the work of students of the presidency, and possibly scholarship in other fields, such as judicial politics, or even comparative politics, as well.

The book has two primary explanatory goals. First, it seeks to contribute to our understanding of gridlock in American politics, which Krehbiel defines as occurring whenever a policy that receives support from a majority of legislatures is not enacted into law. More specifically, the book seeks to identify the conditions under which gridlock will or will not occur. Second, the book seeks to explain the size of winning coalitions in Congress.

To address these two questions, Krehbiel develops a simple theory, which he describes as consisting of ‘a few points on a line’ (p. 234). The ‘line’ part of this quote implies that the model is unidimensional. The ‘few points’ refer to the political actors that play key roles in this theory. Not surprisingly, one of these actors is the President, represented by  $p$ . The other three actors are all members of Congress. There is the median member of Congress, denoted by  $m$ , the filibuster pivot,  $f$ , and the veto pivot,  $v$ . The first two actors – the president and the median member of Congress – need no explanation. The other two actors are included because of specific institutional features of American politics. First, the Constitution gives the president the power to veto legislation passed by Congress, and also gives Congress the ability to override this veto if it can muster a  $2/3$  majority in each house of Congress. Thus, the veto pivot is the legislator who is located on the same side of  $m$  as  $p$  (e.g.,  $p < v < m$ ), and who is located at the 33<sup>rd</sup> (or 67<sup>th</sup>) percentile. Second, Senate rules allow for unlimited debate, which can kill a bill, unless  $3/5$  of the members of the Senate choose to end debate by invoking cloture. Thus, the filibuster pivot is the Senator located at the 60<sup>th</sup> percentile (and generally in his analysis, on the opposite side of the median as the president, so  $p < m < f$ ). In essence, then, the median member of Congress is included in the model to represent the majoritarian nature of congressional policy making; and the veto and filibuster pivots are included to represent the *supermajoritarian* nature of congressional policy making.

Krehbiel’s game-theoretic approach includes the actors discussed above and some common features in a spatial model: a given sequence of action, perfect and complete



information, unidimensionality, an exogenously determined status quo, and the use of the equilibrium concept of Nash subgame perfection. Given that it is a simple model (which is a compliment, not a complaint) and that it is very clearly explained, it should be accessible to all scholars. Readers interested in a more thorough and slightly more technical (but still very accessible) version of the model can find it in Krehbiel's 1996 article, 'Institutional and Partisan Sources of Gridlock: A Theory of Divided and Unified Government' (*Journal of Theoretical Politics*, 8: 7–40).

While the model is technical, its findings can be spelled out quite easily. When a legislature seeks to make policy, sometimes it will be able to do so, and other times it will not. There are essentially three cases that emerge from the theoretical model. First, gridlock will occur when the existing policy is located between the preferences of the actors who represent the veto and the filibuster pivots. That is, the legislature will *not* be able to adopt a new policy when the status quo lies between  $v$  and  $f$ , even if a majority of legislators would support this new policy. This interval, from  $v$  to  $f$ , is thus known as the *gridlock interval*. Second, when the status quo is extreme relative to the preferences of all four of these actors, the legislature generally *will* be able to change policy. More specifically, it will be able to move the policy toward what the median member of the legislature prefers. And third, when the status quo is located just outside of the gridlock interval, the legislatures will be able to avoid gridlock and adopt a new policy; but because of the supermajorities needed to override a veto and overcome a filibuster, these policies will not be located at the median.

Thus, the model achieves the primary goal that Krehbiel sets out for it (I view the other goal – explaining the size of the majority coalitions – as less important and less interesting). To begin with, it demonstrates that gridlock can exist. But much more importantly, it demonstrates the *conditions under which* gridlock exists. Gridlock is more likely to occur when the size of the gridlock interval is large. Thus, the more heterogeneous are the preferences of elected politicians, the more likely gridlock is to occur. In addition, gridlock is more likely to occur when the existing set of status quo points are moderate, relative to the preferences of current political actors, than when they are extreme. Thus, when the preferences of a previous set of elected officials are very different from (i.e., likely to be extreme relative to) the preferences of current officials, we are likely to see a number of policies changed; but when the preferences of the previous and current officials are similar, change is much less likely.

Having developed the model in Chapter 2, Krehbiel then tests it in subsequent chapters. The primary test seeks to determine whether the size of the gridlock interval is related to legislative productivity. To assess this, Krehbiel uses a couple of different measures of legislative productivity, including the count of major US laws compiled by David Mayhew in *Divided We Govern* (New Haven: Yale University Press, 1991), and looks to see whether smaller gridlock intervals cause greater productivity, as his theory would suggest.

Any sort of analysis can be picked to death, and this is no exception. Krehbiel uses a measure of legislative productivity (the number of enactments) as a proxy for gridlock; but as Binder ('The Determinants of Legislative Gridlock, 1947–1996', *American Political Science Review*, 93: 519–33) and others have pointed out recently, productivity is not necessarily the inverse of gridlock. This leads the reader to wonder whether the gridlock interval would be an important predictor if measures of gridlock other than productivity are used. Similarly, there are other things that scholars might quibble with (e.g., should a 'start of term' variable be included in the regression analysis). But these sorts of complaints can be levied at any empirical

analysis; and to my mind they should be seen as fodder for future research rather than major faults of the current analysis. And in any case, Krehbiel is very clear about how he chose and measured the different variables. Even more importantly, he provides a theoretical approach to an area of study – legislative gridlock – that heretofore has been dominated almost completely by empirical studies.

While the results do support his theory, the one variable that is certain to raise eyebrows is his operationalization of the gridlock interval. Krehbiel computes this measure in several steps. First, for each chamber he subtracts the percentage of seats held by Republicans in the previous session of Congress from the percentage of seats held by Democrats in the current session of Congress. Thus, a positive number indicates a net gain for Democrats. Second, he averages these numbers for the two chambers. Third, he determines whether the president, who might be either newly elected or continuing in office, has expanded or contracted the gridlock interval. If the former, then the value calculated in the second step becomes positive; if the latter, it becomes negative. In 1980, for example, Democrats lost seats in both the House and the Senate. The seat change was – 7.82 per cent in the House and – 12 per cent in the Senate, for an average of – 9.91. Reagan was newly elected, leading to a contraction in the size of the gridlock interval. Thus, the measure for the gridlock interval is – 9.91.

Most scholars, I suspect, would use some measure of ideological preferences, such as ADA scores or NOMINATE scores, to measure the gridlock interval. Krehbiel does discuss these scores as potential tools for measuring the gridlock interval, but also points out the potential problems with using these scores (e.g., whether these measures can be treated as cardinal measures of preferences). To this I might add that some of these scores (e.g., NOMINATE scores) are estimates, and thus are measured with error, which raises the question of whether they should be used to identify something as specific as a veto or filibuster pivot. Still, the ultimate question is whether these scores are preferable to the measure he has computed. After all, while voting scores undoubtedly have flaws, so does his measure – for example, it does not directly get at the notion of party heterogeneity, which seems central to the concept of the gridlock interval.

After presenting the initial tests of his theory, Krehbiel then moves on to test some additional implications of his theory in Chapters 5 and 6. He uses an innovative empirical approach that he calls ‘switcher analysis’ to determine whether the veto pivot and the filibuster pivot really do deserve such prominent roles in his analysis. If these are the most important actors, he contends, then we would expect to see that actors located near these pivot points are more likely to switch their votes than actors located at other points along the ideological spectrum. Consider, for example, two votes, one on the final conference version of a bill, the other on the vote to override a presidential veto. If the veto pivot is truly a key player in this game, as the theory contends, then we would expect to see members, at or near this pivot, switch their votes at rates much higher than other members. Similarly, when there are multiple votes on cloture, we would expect those actors nearest to the filibuster pivot to switch votes (i.e., vote yes on the first, no on the second; or vice versa) more frequently than actors located farther from the pivot.

Krehbiel views these hypotheses as implications of the pivotal politics theory. I would be more inclined to view them as tests of the maintained hypotheses, or assumptions, of the theory. In either case, they represent creative and convincing parts of his empirical analysis. Because the pivotal politics model assumes perfect information, in equilibrium it predicts that

there will be no filibusters or vetoes. Even if these tests veer off the equilibrium path, however, they provide important support for the theory. More tests of formal models should be conducted like this.

Next, Chapters 7, 8, and 9, Krehbiel examines several applications of his model. He begins by investigating the notion of presidential power. Many previous studies have assessed presidential power by looking at the frequency of veto overrides. A president who is infrequently overridden, according to this view, is more powerful than one who is frequently overridden. Krehbiel builds on his switcher analysis in earlier chapters to come up with an innovative new measure of power. He looks to see whether presidents are able to retain old supporters and attract new supporters, and does so by comparing matched pairs of votes (e.g., before the veto and on the override). In other words, he looks to see whether presidents can use the power of persuasion to attract and retain votes.

Some of his results corroborate conventional wisdom about presidential power; but others do not. A conventional analysis that uses override rates to assess presidential power would rank Bush, who was rarely overridden, near the top and Carter, who was more frequently overridden, near the bottom. Yet Krehbiel's analysis casts this into doubt, showing that both of these presidents were actually in the middle of pack, when measured by the ability to persuade.

The final applications demonstrate the problems of current partisan theories of policy making. In one chapter he conducts some additional switching analysis. He does find some support for a partisan-based approach to politics, although in contrast to most recent theories he finds that it is based in the presidency, not Congress. In the second chapter he relies on case studies to examine whether the pivotal politics approach can provide explanatory power even in a situation where most observers believed that partisan approaches overwhelmed all other factors. In both of these chapters – indeed, in the entire book – Krehbiel issues challenges to scholars to come up with more specific theories and tests about parties, to identify the conditions under which parties will matter.

By now it should be apparent that this is a wide-ranging and far-reaching book. Krehbiel starts with a very simple model. He tests this model directly; then tests it indirectly; then tests some other implications of the model. Along the way, he touches on numerous topics, including the relationship between divided government and legislative productivity, the role of parties in the legislatures, the nature of presidential power, the effect of supermajority rules, and the relationship between Congress and the president. These are some of the most important topics in American politics today; that they all are related to his theory shows the beneficial effects of a careful, specific model.

Perhaps most importantly, this book sets out a research agenda for other scholars. On the theoretical side, there are a number of issues that this analysis raises. Should parties be included in such models? If so, how? What about committees, which Krehbiel discounts because of the lack of official institutional or constitutional rules that would support their inclusion? After all, even though their roles are not constitutionally defined, they do play an important role in preventing legislation from being passed, which is an important part of his definition of gridlock. Are other institutions, such as courts or bureaucracies, influenced by these pivotal actors? Can such an approach shed light on policy making in other countries? How, for example, might such an analysis work and be tested in parliamentary systems?

In addition, this book will no doubt prod scholars to conduct a wide range of empirical analyses. How should the gridlock interval be measured? If President X can attract and retain

more supporters than President Y, what caused President X to have less support in the first place? What constitutes a fair test of party-based theories? Can the bicameral nature of Congress be taken into account in these sorts of empirical tests?

Once again, as he did with his work on the information and legislative organization, Krehbiel has written a book that will set a research agenda for other scholars. Some will support his approach and arguments; others will build on it; and still others may develop theories or find empirical evidence to refute what he has done. All, however, will pay attention to it and learn from it.

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Edward J. Lincoln, *Troubled Times: US-Japan Trade Relations in the 1990s*, Washington, DC: Brookings Institution Press, 1999.

In a 1998 article in *Foreign Affairs* Edward J. Lincoln suggested a new strategy for the United States to deal with what he and others view as the Japanese government's recalcitrance in recent trade negotiations. Lincoln asserted that if the Japanese government does not become more agreeable to American desires to increase Japan's imports of goods and services then the US government should begin to ignore Japan. In Lincoln's view, if bilateral trade talks continue to fall short of American expectations the United States can signal its displeasure by making Japan a peripheral trade and security partner. Lincoln, of course, sees the discounting of Japan's importance in American foreign policy as an extreme option and in his most recent book he offers a series of realistic and workable options. Few observers of US-Japan trade relations have had the policy impact as Lincoln. His stint as advisor to the (former) Ambassador to Japan, Walter Mondale, places Lincoln at the nexus of academia and policy making. The message put forth in *Troubled Times* is timely, authoritative, and important for academics and policy makers to hear.

An underlying assumption in *Troubled Times* is that the United States must continue to rely on bilateral negotiations. While recognizing the merits of multilateral forums, such as the World Trade Organization's Dispute Settlement Mechanism, the author insists the US continue to pressure Japan into opening key sectors, such as telecommunication. Lincoln views bilateral pressure as essential to ensure American firms access to the world's second largest economy. One advantage bilateralism has over multilateralism is related to the fact the United States can lose (and has lost) disputes filed under the WTO's DSM. Therefore, American direct pressure on the Japanese government is, according to Lincoln, paramount to American national interests. Lincoln does not suggest eschewing multilateralism, but he seems wary of turning American interests over to an international organization where the US can find itself on the losing side of judgements. Moreover, he rightly identifies situations where the rules of the WTO and other multilateral institutions do not apply.

After a short introduction, chapters 2 and 3 of *Troubled Times* pick up where Lincoln's 1990 book *Japan's Unequal Trade* left off. These two chapters assess trade and investment statistics, respectively, with data up through about 1997. The story the numbers tell is largely the same as in *Japan's Unequal Trade*. In terms of imports, few readers will be surprised to learn that 'Japan does not absorb as many products from abroad as other nations . . . this fact cannot be explained away with simple economic variables' (p. 73). Non-tariff barriers are pointed as the principal culprits for why Japan imports less than other advanced industrialized countries. During the mid 1990s the data do show some increases in imports in a few sectors. Lincoln surmises this change resulted in fewer domestic demands on the Clinton administration by American firms and this in turn explains the decline in pressure by the administration on Japan in bilateral talks.

In addition to demonstrating that Japan imports at a lower level than other major American trading partners, Lincoln convincingly illustrates that there is less foreign direct investment into Japan than other advanced industrial states. This is in sharp contrast to Japan's foreign direct investment abroad, especially after the 1985 Plaza Accord. The author reviews various reasons for this disparity, including such explanations as the absence of organizational advantages for non-Japanese firms due to the presence of *keiretsu* relationships, high labor and land costs, and a lack of requisite infrastructure in rural areas. Despite some slight improvements over the 1980s and the entry and success of a few high profile American firms (i.e., the busiest Starbucks in the world is in Tokyo) into the Japanese market, during the 1990s, relocating or buying domestic firms remains prohibitively expensive for most foreign companies. Lincoln concludes that there is a market access problem for foreign firms in Japan and hence a role for bilateral trade talks between the US and Japanese governments.

In chapter 4 Lincoln reviews the Bush and Clinton administrations' bilateral negotiations with Japan. Perhaps not surprisingly, given the author's connections with the Clinton administration, he is not generous in his assessment of the Bush administration's efforts under the Structural Impediments Initiative (SII). Lincoln acknowledges some early successes of SII (e.g., Toys R Us), but ultimately concludes, '[a]fter the summer of 1990, the administration's overall enthusiasm for SII faded, and the Japanese bureaucracy resisted negotiations more stoutly' (p. 119). This conclusion is based on his focus on increases in market share and whether SII led to more open markets, not on whether the US government's preferred outcomes were obtained.

Lincoln is fairly balanced in assessing the Clinton administration, devoting ample space to explaining why, in his view, critics who maintained that the administration sought to 'manage' trade with Japan were wrong. Lincoln claims that the administration was not seeking managed 'targets', but rather sought to employ 'temporary market share targets' (p. 146) to measure outcomes of the Framework Talks and other negotiations. This semantic difference was viewed by some observers, notably Jagdish Bhagwati, as specious. The administration had difficulty determining when an agreement was successful without resorting to measures that appeared to ardent free-traders as trade management (i.e., interfering in the market). By the second Clinton administration this problem was not resolved and, more importantly, it was clear Japan's economic downturn was impeding further negotiations.

To address the impact of Japan's changing domestic political economy, Lincoln turns to, in chapter 5, a consideration of whether and how Japan changed in 1990s. Despite signs that since 1993 there has been momentum in Japan to deregulate, the author concludes that

practices such as *amakudari* – along with societal preferences concerning the proper role of the government in the economy – have inhibited true deregulation. *Amakudari* according to Lincoln ‘provides a substantial reason to be skeptical of the extent of deregulation and unilateral market opening in Japan’ (p. 190). He does recognize that, as a result of mounting bad debt, in the financial sector some effective reforms have been implemented. The bottom line for Lincoln, however, is that in terms of market openness Japan in the 1990s looks much like Japan in the 1980s. Moreover, he is pessimistic about the Japanese government’s ability and sincerity to initiate deregulation and other reforms. Lincoln’s solution is to insist the United States maintain pressure on Japan through bilateral mechanisms.

The concluding chapter reviews the evidence presented and offers Lincoln’s guidelines for the American negotiations with Japan. In short, *Troubled Times* represents the views of one of the most influential advisors and economists on US–Japan relations. At times, however, Lincoln’s review of trade and financial statistics will likely enervate the attention of even the most interested reader. Moreover, the statistics and case studies could easily be interpreted differently, in ways less critical of Japan. If there is a major flaw in the text it is one Lincoln readily admits: the focus on access to Japan’s markets leads to a one-sided analysis because it fails to account for barriers faced by Japanese (and other) firms in the American market. Some readers will also be troubled by the emphasis on bilateralism over multilateralism. Nevertheless, *Troubled Times*, is required reading for students of US–Japan trade negotiations.

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